CHAPTER 10

THE GRADUATE STUDENT ERA: 1966-1997

The differences in the dynamics of a research organization (at least the one in which I spent the first 18 years of my professional career) and a research university are more numerous than a casual observer might think. In the former, at least in the part in which I was employed, extramural funding was essential to survival, and any major disruption of cash flow meant no money for salaries. This situation definitely produced a sense of community in the group. Even though the organization was large enough that money seemed to be available somewhere, one never knew when transition funding between grants would be necessary. Therefore, helping others in time of need increased the probability of being helped in times of need. Of course, Ruth Patrick was chair of the Limnology Department of Academy of Natural Sciences Philadelphia (ANSP) and had the final decision on all money allocations, but she had a strong incentive to maintain the group intact. Patrick had a few graduate students over the years I was there, but other members of the group had none. The technical staff, laboratory technicians, etc. outnumbered the curatorial staff (faculty) by a much greater margin than in most research universities. They stayed longer than graduate students; had no other distractions, such as course work; and could easily be moved from one grant to another. The staff worked 35 hours per week—not the long hours of graduate students. The staff was, for the most part, solid and dependable workers, but they did not share the excitement of publication as do graduate students. The staff did realize that publications were a discrete form of academic advertising that helped maintain the cash flow and increased job security. They were not risk takers with new ideas as are first-rate graduate students. Arguably, the most important difference was that they did not make presentations or present papers at professional meetings and were not usually as sensitive to potential weaknesses as those who were exposed directly.

An equally important difference between research organizations and research universities was the vulnerability that purchasing personnel felt financially if equipment purchases were delayed, which subsequently had adverse effects on completing the work for the grant. Accountability was very high in the research organization and purchasing was not impeded by the complex rules (bids, etc.) that were common to research universities. On the other hand, overhead charges in research organizations were much higher because no endowment or other support was available to subsidize salaries for maintenance, purchasing agents, secretaries, and the like. Even though Patrick bore the primary responsibility in the research organization for keeping people employed, I was very aware that "inappropriate" time management on my part would cause financial hardship for others.

At ANSP, I filled in daily time sheets at the half-hour level to show what grants my time had been charged to that day. After detailing working time for approximately 18 years, I was somewhat disoriented when I went to the University of Kansas and did not have to account for how I spent my time. Although tracking working time at the ANSP did tend to compartmentalize my thought patterns, it was not as bad as it sounds. Effects were probably diminished because official work time was 35 hours per week, but the time actually spent was far greater.

The discussion on money in this chapter may have too much emphasis; however, even today in retirement, I cannot do everything I wish to do professionally without funding. During my more active years, even the graduate students who had NSF predoctoral fellowships needed money for equipment, travel, and journal page charges. Isaac Asimov is reputed to have defined academic freedom as "extramural funding." With extramural funding, publications, and a clear view of future research goals, the number of options for obtaining funding is vastly increased over those faculty with good teaching records alone. Although my emphasis on funding may offend some people, it has been central to my career. The worst periods of my career have been the two times when the cash flow temporarily went below a critical level.

As I consider my research career, I realize the growth and importance of my relationships with graduate students. I did not have graduate students at ANSP (despite the institution's

affiliation with the University of Pennsylvania) because I was in the field too much and had no space for a graduate student to work. However, when I left ANSP for the University of Kansas, I acquired my first two graduate students, Richard (Rip) E. Sparks and William T. (Tom) Waller. Graduate students who worked with me during the three different stages of my career were, in one sense, working with a quite different person.

Period I—Buildup Period

Although I went to the University of Kansas as a full professor with immediate tenure, I had no experience chairing graduate committees, had left all my equipment behind, and had no grants or contracts that were transferable (although I quickly acquired new extramural funding). Furthermore, I was about to embark on a very risky research operation—the idea that biological early warning systems could be developed that would produce nearly real-time information on ecological stress with the use of computers.

I was only at the University of Kansas for two years (1966-1968), but Rip Sparks acquired his MS and Tom Waller, who already had an MS from Pittsburgh State University in Kansas, had a two-year start on the PhD. Sparks was intensely interested in the computer-interfaced, biological early warning system that I was then developing; the system was also the focus of Waller's dissertation research. Since research space at Kansas was inadequate for the project, I was delighted when Virginia Polytechnic Institute and State University (VA Tech) offered me a free hand in developing my research program and offered to transfer Waller's and Sparks' academic credits. The Chancellor and the Provost at the University of Kansas very kindly telephoned then President of VA Tech, T. Marshall Hahn, to apprise him of my research space problems; this kindness further facilitated the transfer of the research project. I am deeply indebted to all three.

Despite my brief stay at the University of Kansas (brief because of the poor quality of the research space assigned to me), I had valuable experiences: (1) I was successful in carrying out research in a new setting; (2) the research was of an entirely different character than my past research; (3) I had acquired the ability to interact productively with graduate students; (4) I had become acutely aware of the need to negotiate explicit conditions when moving to a new institution; and (5) I had learned how much one could depend on quality graduate students.

While at the University of Kansas, I gave seminars at North Texas State University (now University of North Texas) in Denton, Texas, and Central College in Pella, Iowa. I gained two graduate students from these visits. At the University of North Texas, I met Ken Dickson, who had obtained his MS with J. K. G. Silvey. At Central College, I met Jeanne Ruthven, who was interested in protozoan colonization of artificial substrates. David E. Wilson of the Biology Department at Central College provided superb help during Ruthven's entire senior year in getting her ready for rapid protozoan community identifications. I met Jeanne Ruthven because Don Huffman, then Biology Department Chair at Central College, had worked with Alexander Smith at the University of Michigan Biological Station where I taught for many summers.

Although I appreciated all them at the time, I realize in retrospect that I was blessed with four capable, independent graduate students at a time of my greatest need. These four graduate students were the key to a successful outcome. Had I not worked with all four before moving to VA Tech, I could not have distributed my energies and time the way I did. Had this division of energy and time not been possible, a successful outcome would almost certainly have been in serious doubt.

The summer before I was to begin the academic year at VA Tech, Jeanne Ruthven and Ken Dickson joined me at the University of Michigan Biological Station on Douglas Lake where I had been teaching since 1964. At that time, I was teaching protozoan ecology. Jeanne Ruthven became my teaching assistant, which required an ability to identify protozoans. Ken Dickson and I started research on the sequential comparison index for which I had already published one paper (Cairns et al. 1968), but for which researchers wanted more detailed directions. I was able to know these students in a far different set of circumstances than is customary in a research university setting.

Waller transported the computer-interfaced monitoring equipment from the University of Kansas, but it was not set up until space in Derring Hall on the VA Tech campus became available

the following academic year. Sparks was able to work on electrical signals from fish opercular muscles and the heart in Alan Heath's laboratory. Sparks, Waller, and I spent much of our time in Price Hall writing grant proposals on rapid biological monitoring systems. In addition, Waller and Sparks completed much of their coursework. Dickson was to work on rapid biological information systems of stream and river communities. Ruthven was to carry on my research program in freshwater protozoan colonization dynamics. Heath was enormously helpful with establishing my program in monitoring at VA Tech and served on both Sparks' and Waller's advisory committees. All this assistance made it possible to reestablish my research program in a very difficult period.

Period II—Expansion (or Growth) Period

I was initially employed at VA Tech in the Department of Biology in a research position, which required no teaching. However, since I had not regularly had the enjoyment of teaching in a university setting, I decided to teach limnology and protozoology the first year. In addition, I was charged with building an aquatic ecology program, particularly at the graduate level. The research in this program was to be supported entirely by extramural funding. All four of my graduate students were PhD candidates who had a large stake in the success of the research.

Since Derring Hall was not completed on schedule, Bob Paterson, the department head, offered me a large room in Price Hall that would serve as office space for me and the graduate students; the space also had microscope benches for protozoology and aquatic insect identifications. Even this temporary space was over four times larger than my space at the University of Kansas. I was cautioned by some of the faculty, who knew that I had little experience with PhD candidates, that sharing office space with them was not a good idea. However, this set up was the only choice for the situation. I had to place enormous burdens on the graduate students, well beyond what should normally have been expected of them. Some of my graduate students have commented recently that this philosophy of graduate student training prepared them for the real world and has proved highly successful in their careers. The fact that they accepted additional responsibilities with enthusiastic consent did not lessen my apprehension because, if this effort failed, I might easily have lost faith in the entire research undertaking, which many of my colleagues felt was too visionary and impractical. In a very real sense, the small group functioned almost immediately as colleagues, although I was still their mentor. The graduate students helped prepare grant proposals, selected some field sites (which I did not have enough time to visit initially), worked together as a team that helped each other, and, most heartening of all, developed the writing skills necessary for publication of parts of their research before completion of the dissertations.

Before the middle of the first academic year, two federal monitoring proposals and two field studies were funded. The little group in aquatic ecology went from poverty to abundance literally almost overnight, and the problem became how to complete all the work without diminishing quality. The graduate students immediately switched to extensive research. Since we had been functioning as a group, obviously successfully, the team spirit never diminished. If someone needed help, one of us volunteered.

When we moved into more spacious quarters in Derring Hall, we kept in close touch with each other as a matter of course. At the end of the first year, inquiries came from other prospective graduate students. I asked my present graduate students for help in determining which applicants seemed best suited to the research program that required a high degree of individuality coupled with a team spirit. This team spirit continued throughout the remainder of my professional career, although, not surprisingly, some individuals preferred the "lone wolf" approach much more than others. However, since the nature of our research required interactions with other disciplines and other components of society outside the academic world, everyone shared the view that a multidimensional approach to environmental problem solving, though demanding, was also exciting.

Unlike my employment at ANSP, my salary would be constant in the university setting even if I failed to obtain grants. However, the technicians, graduate students, and the hourly employees, including undergraduates, would have no income if extramural funding were not available. I could not calculate exactly the right amount of money to obtain because students kept coming and going

and projects changed. I settled on two solutions: (1) obtain a little too much money and have everyone agree to share the extra work load for the benefit of increased security and (2) place my consulting and speaking fees in a special account to be used in emergencies. The group shared the extra work load, and this team approach contributed to the community spirit. However, the amount of funding that I was able to sequester at that time, although significant, was not sufficient to provide the degree of security everyone wanted. Fortunately, the students knew that, when I was absent for consulting or speaking, much of the money I earned went into the kitty and, therefore, benefited them. They knew I was not neglecting them through my absence, but I was actually fostering their professional careers.

Lest this description of community spirit appears too utopian, I must note that many problems surfaced when the students interacted with more than one department. The interactions were absolutely necessary for the graduate students since they needed the help of electrical engineers and the engineering faculty (particularly Cliff Randall in Civil Engineering) with equipment construction (most equipment construction had occurred at the University of Kansas, but continuous remodeling was necessary at VA Tech). Faculty in other departments helped with computer interfacing (Ray Dessey in the Department of Chemistry was particularly helpful in the early stages of development) and statistics (J. C. Arnold in the Department of Statistics was particularly helpful in the early stages and Eric Smith, in the same department, in the later stages). Another problem surfaced when the graduate students published in journals that, at that time, were not familiar to most biologists (my graduate students "home" discipline). Although this challenge ultimately helped their careers, it did isolate them from the faculty in the local biological community, some of whom felt that biology entailed too much for any one person to learn, so why tackle other disciplines. The answer is, of course, that the isolation of the disciplines from each other is an academic artifact that impedes recognition of the connections that exist in the natural world. This isolation of the program and students at that time worsened as the Aquatic Ecology Program expanded to the point that it was recognized as an individual entity. This misunderstanding worsened further with the formation of the University Center for Environmental Studies (UCES), which gave the program an administrative impetus. A variety of interactions under this construct mandated fewer interactions within other members of the biological community.

I served as director of the UCES for roughly two and a half decades. Its purpose was to engage in environmental research that transcended the capabilities of a single discipline and was almost entirely supported by extramural funding. Dickson finished his degree with me when the UCES was launched, and he became my assistant director.

Quality control became a real concern in this program that grew so rapidly and was so different from other departmental activities and whose faculty was young and untenured. As a consequence, in addition to each student having an advisory committee, each was required to report individually once a month to the entire aquatic ecology faculty. Many students regarded this extra requirement as an imposition, not endured by graduate students in other academic units. Dick Pratt (personal communication) remarked that graduate students came to refer to this requirement as "The Inquisition." However, Pratt admits this individual meeting with the faculty was one of the most important aspects of quality control ever practiced. He also feels that it was ultimately good for students—even the ones who hyperventilated. Furthermore, this requirement was necessary for a number of reasons. Many grants at that time were block grants with a number of components assigned to individual faculty members and, hence, to particular students. The student's advisory committee was not particularly intent on ensuring that the conditions of the grant were fulfilled, especially when many of the faculty on these committees were specialists in other areas of biology than stressed ecosystems and might have been inclined to encourage the students to pursue directions not covered by the grant. However, if one or more components of the block grant had notable failures, then the entire block of funding was seriously threatened. Many industrial grants required monthly progress reports (and a few weekly). This reporting procedure was not a well understood situation by either the new aquatic ecology faculty or faculty members outside it. Funding in the Department of Biology at VA Tech was not robust when the aquatic ecology group

was formed, and many students outside the aquatic ecology program were carrying out research while being supported by teaching assistantships or their own money. They were not under the pressure of reporting at two meetings or writing monthly (or even weekly) reports. This difference led to a certain degree of isolation of each group of students from the other.

This funding regime created another dichotomy—the "haves" and the "have-nots." Graduate students whose major professors had money could spend a huge amount of time on their research, particularly when funding during summer was included. Money for equipment, for travel to meetings to present papers, for hourly help and technicians, and the like made research easier. At VA Tech, all graduate students were required to teach a course, a lab/lecture session in general biology (a good idea, since many of them would be teaching general biology in their first professional position). After this requirement was met, those with extramural funding could concentrate on their research. Those without extramural funding were on teaching assistantships, which took a substantial amount of their time. Naturally, some loss of camaraderie was inevitable when some graduate students had what appeared to be endless amounts of money and equipment and others were scraping along on a shoestring. The same was true to some degree among the faculty. Even some of the sources of money (sometimes labeled "dirty money") were from non-traditional sources, such as industry, municipalities, and the like; "clean money" was from the National Science Foundation and other similar organizations. Government grants from organizations such as the USEPA were usually regarded somewhere in between, but more on the side of the foundations. Of course, in more prestigious institutions, the "haves" far outnumber the "have-nots." In institutions devoted primarily to teaching, the "haves" are a minority. The institutions between these two groups have varying proportions and, therefore, a significant dichotomy. This situation is a foretaste of professional life, not only in academic institutions but elsewhere where research funding is scarce.

Funding I obtained for graduate students also allowed them to reallocate time to writing manuscripts and publications. Everyone who contributed conceptually to a project was included in the list of authors. Initially, I was senior author because I had developed the research plan and, in some cases due to the novelty of the approach, extensive interactions were necessary with journal reviewers, and so on. Furthermore, until the graduate students had a bit more experience so that they were reasonably well acquainted with the professional risks involved in publication, I felt students should not be senior authors. As soon as they were well aware of the risks and had made a major conceptual contribution, senior authorship for them seemed an obvious choice. I followed this practice throughout my career and have often been the last author on a publication. The progress from junior author to senior author showed the students that I had sufficient faith in their research to take the risks of publication with them.

I have always guaranteed my students that I would pay for page charges, purchase a reasonable number of reprints, pay extra charges for figures and photographs, and nearly always give money to them for travel to professional meetings to present papers. This assistance was not entirely altruistic. When one presents a paper at a professional meeting, one's attitude towards research is never the same. The same is true after one sees reviewer comments on what the author felt was a perfect manuscript. Almost invariably, one gathers the extra bit of data to make the manuscript even more persuasive and looks at it with more critical eyes after the first professional publication. Furthermore, this process turns all research activities into collaborative partnerships since the partners are attempting to persuade others that the research is sound and are working energetically to minimize criticism. This relationship is entirely different from the one that exists if the dissertation or thesis is being reviewed internally by a clearly identified group of faculty. The "proof of the pudding" of the effectiveness of this relationship is that many of my former students and I continued to publish together long after they have obtained their PhDs.

I employed Darla Donald as an editorial assistant in 1975 and paid her from consulting fees, book royalties, and money from a variety of sources. Graduate students were encouraged to give her their manuscripts when they had reached a certain stage of development. She styled the manuscript for the requirements of a particular journal and edited the writing. Having such a person available

during one's graduate career could make one dependent on an editorial assistant. In fact, however, the training in preparing manuscripts that they received from Darla has been invaluable to practically every graduate student in the program. Even the ones who did not go into academia were still required to write reports for industry or government agencies. The students' acquiring manuscript preparation skills was as important as learning how to write grant proposals (Darla also helped in this regard), prepare budgets, and the like.

Another advantage to the graduate students (and undergraduate students working on research projects under my supervision) was the availability, from the beginning, of a manuscript typist. Initially, the cost of this position was covered by extramural funding but, when I became a University Distinguished Professor, a full-time secretarial position was created in the Department of Biology by the President and Academic Vice-President to cover this function.

In 1970, the President and Academic Vice-President (now Provost) asked me to organize a University Center for Environmental Studies (UCES) as an administratively independent unit within the Research Division so that any or all colleges could be involved whenever appropriate. All my students were already working with people in other disciplines, so the transition was both easy and natural for them. Today, involvement with other disciplines is more common, but, nearly three decades ago, it was remarkable and caused much discussion and comment ranging from admiration to hostility. Some years ago, some civil engineers wanted to form a hazardous materials center and, when difficulties were encountered, permission was granted to extend the designation of the existing Center to University Center for Environmental and Hazardous Materials Studies (UCEHMS). Although the UCEHMS still exists on paper at present, no buildings, equipment, nor faculty are intimately associated with it. Apparently, no one was anxious to take on the responsibility of an organization so dependent on extramural funding. However, the model developed at VA Tech has been emulated by many other institutions, and some have even employed students from the original UCEHMS program. The University of North Texas Institute of Applied Sciences is the most visible example. The Environmental Studies Program at Arkansas State University is developing in a manner similar to UCEHMS. Other programs, such as those at Mississippi State and Clemson University, could be viewed as equivalent models still functioning robustly.

As faculty members in the aquatic ecology group attained tenure and additional seniority, they naturally wanted to explore their personal interests and engaged less in group activities. Since the group never had formal administrative approval, its dissolution was a matter of mutual agreement. Some of the aquatic ecology faculty, such as Ken Dickson and Albert Hendricks, chose to remain with the UCES, and, therefore, the dissolution of the aquatic ecology group did not destroy the group's spirit. The exposure to a multidimensional view of environmental problems, including statistics, economics, engineering, chemistry, and physics, was useful to the students who interacted with these other disciplines. However, the depth of interaction with one's disciplinary peers is necessarily weakened by developing relationships with other disciplines.

A comfort often comes from belonging to some sort of "tribal" group professionally, where a relatively comparable level of literacy on a particular subject exists, problems are common, mutual assistance is offered, equipment is shared, and, perhaps most important, a shared terminology exists. Those graduate students coping with hypotheses and problems that require significant input from other disciplines generally do not share the same needs and are not part of a tribal unit. When graduate students in these circumstances compare their lot with the camaraderie of the more closely knit subdisciplinary groups, the situation is often unsettling. However, such circumstances may be a foretaste of the highly probable circumstances in their future careers. Fortunately, multidimensional approaches to environmental problems are now becoming the norm, so we were on the right track despite the problems that the cross discipline approach caused my graduate students and, of course, me as well.

Period III—Equilibrium Period

As is the case for all rapid but incremental growth in any transdisciplinary group, no abrupt thresholds were crossed, but, rather, a series of breakpoints and thresholds occurred. However, the middle period had a number of distinguishing characteristics: (1) diminished contact with each individual, even my own students; (2) diminished time for my own research despite a 60+-hour work week and no vacations; (3) diminished interactions with the Department of Biology due to involvement with the UCEHMS and the sheer volume of work; (4) delegated to an administrative assistant and a business manager matters I once handled personally; (5) increased attention to persons outside the university, primarily because of chances of extramural funding, which supported the two groups for which I was responsible; (6) caught between two worlds of administration and faculty. In this middle period, I felt I had accomplished the two charges given me when I first arrived at VA Tech: (1) establish a major research program that would receive national and international attention and (2) fund this organization almost entirely with extramural funding.

From the standpoint of both graduate and undergraduate students, the program appears to have been successful despite the issues already mentioned. The students now have challenging positions in the field in which they obtained their degrees. All the presidents, provosts and deans of the research division, deans of the college, and at least two biology department heads were pleased that so many graduate students were supported by extramural funding. I also felt a personal satisfaction to have fulfilled the charge given to me by the university when I was employed.

However, I had no trouble relinquishing the leadership of the Aquatic Ecology Program when the members of that faculty acquired tenure and wished to pursue other ventures. Although I had much help in acquiring extramural funding, I was held personally responsible, both internally and externally, for ensuring that the conditions of each grant were met. Mercifully, the amount of paper work on grants, both internally and externally, was far less in those days. Many years later, I still have a good relationship with students from that period despite the reduced amount of my time available for them individually. This group of students includes some whose committees were chaired by others. Some students from this period have commented recently that I provided evidence that one could do both administration and research (for which Ruth Patrick was my model). Clearly, neither task was done as well as each could have been if each had been done alone. On the other hand, had I devoted all of my energy to administration, my professional career would probably have ended at age 65. I simply cannot imagine working six days a week on administration solely and feeling the same zest I would feel if it were research.

Since neither the Aquatic Ecology Program nor the UCEHMS has survived in its earlier form (the former is a loosely affiliated group of individual investigators, and the latter is a paper organization), I wonder if my efforts were wasted. Gene Odum left a thriving Institute of Ecology at the University of Georgia. Ruth Patrick left a solid organization at ANSP, and other organizations have gotten their starts there. Clearly, the Aquatic Ecology Program and UCEHMS might both have survived if all my energy had been expended on one or the other and on either administration solely or research solely. Still, I learned valuable lessons that might not have been possible any other way. For example, interdisciplinary teams were a good transitional stage for resolving complex problems. However, most team members retained their disciplinary bias, which made communication and, thus, synthesis difficult. The emergence of multidimensional individuals signaled the doom of interdisciplinary teams, although most organizations seem not to have realized this change. After fighting interdisciplinary teams for decades, academic institutions now espouse such teams. These teams produce much information, but rarely is it used effectively. Having to acquire large amounts of extramural finding also sharpened my foraging behavior. I had grants until 2001, even in my retirement years. Arguably, the most important learning experience for me in the middle period was the development of a systems-level approach (top-down is the current jargon), which probably would have developed slowly in other circumstances. The optimism and zest of the graduate students persuaded me to explore new areas that I might not have considered.

The Middle Period was good for the students and for me. Some of the students are saddened from that time when they return to VA Tech and find that both programs have dramatically

declined from an organizational perspective. My view is that programs are just collections of individuals; the concepts have been nurtured by former students and are flourishing elsewhere.

Period IV—Closure Period

Students who worked with me from the time that funding for state institutions in Virginia dramatically decreased in the early 1990s had an entirely different relationship with me than earlier students. We were disassembling an organization that still required much of my time if it were to be done without hurting any of the people involved with it and if the organization's reputation was to be maintained throughout this process. When Karen Holl and I were discussing the possibility of my chairing her PhD committee, I mentioned my perception of trying times ahead. I was confident that, even though I was eligible for retirement with full benefits, I would be working long enough for her to get her PhD and I would have enough resources to support her research. John Heckman, my final graduate student, saw the disappearance of the entire UCEHMS program, the dispersal of the equipment, and the like. I was able, fortunately, to continue a professional working relationship with B. R. Niederlehner and Darla Donald. I retired two years before John Heckman's graduation and was in the process of dismantling the UCEHMS for the first half of his graduate career.

Having students during the closure period was quite different from the earlier periods. During this time, students were working on all the topics that I worked on throughout my career, but the faculty teams that could have aided in this undertaking were gone. While Karen Holl was a candidate for the PhD, students were working on toxicity testing, genetically engineered microorganisms, and various topics in ecological restoration. The intellectual overlap with graduate students in other parts of the program was dramatically reduced. Still, my remaining graduate students interacted well with each other on a personal level and helped each other. Senior technician B. R. Niederlehner was especially important during this period as was editorial assistant Darla Donald. The students were extraordinarily resourceful, which benefited them greatly after graduation. Another major difference was that I was much more available to the students due to greatly reduced field work, less grant money to keep track of, fewer major reports, and far less travel. I also worked fewer hours, although, in the aggregate, they still exceeded 40/week.

Unquestionably, my relationship with my graduate students was different in each of the three periods of my professional career in a variety of qualitative ways. Students in all three eras learned much about the process of science, working with those in other disciplines, acquisition of extramural funding, the process of publication, preparation of quarterly and annual reports on extramural funding, and meeting with a variety of groups (e.g., regulatory, federal and state, industrial, citizens and environmental activists, recreational, and municipal). Students from each of the three eras have careers in quite different professional positions in academe, industrial, regulatory, and non-profit environmental organizations. Clearly, their career choices were based on their individual predilections rather than their relationship with me. Or, stated differently, the types of exposure just mentioned were the dominant factors in a career choice rather than the era in which they were graduate students. Last, but far from least, I have enjoyed an ongoing professional relationship with many of my former graduate students, in some cases, for decades after graduation. Again, these relationships have depended more on their individual personalities than on the era in which they were graduate students.

I cannot possibly indicate a preference for one of the periods. I found each exciting, challenging, and refreshingly different. The shared excitement in the undertaking seemed to override all other considerations. Interestingly, some of the most enduring, post-graduation relationships have been with students whose committees I neither chaired nor co-chaired. Students from all three eras meet together at particular professional meetings and rekindle a group identity that I would not have thought possible. Some years ago, I received a pennant from one such meeting (Pensacola Society for Environmental Toxicology and Chemistry meeting in 1987, which I regrettably missed), signed by well over half of my former graduate students. They indicated that I should not miss this particular meeting in the future because I would miss seeing them on an

annual basis. Also, when my former students celebrated my 70th birthday and my formal retirement (which occurred soon thereafter), considerable numbers from all three eras appeared and joined together harmoniously. At my 70th birthday, some of the then current graduate students were taken aback at the age of some of the earliest students, but then they were over two decades apart. In 1968, I shared an office/laboratory with my four graduate students. All indoor work (e.g., protozoan identification, examination of macroinvertebrate samples) was performed there. Eventually, we acquired a computer. At the end of the graduate student era, the graduate students shared office/laboratory space together, although each had a computer. Closing the administrative units was an activity not suited for graduate students, and one I hoped would never be necessary for them. Perhaps the explanation for the closeness is that one should always be undertaking something new and exciting, in which the graduate students can take part and be intellectually stimulated—the nature of the undertaking, as long as it is not disreputable, makes little or no difference.

Although my last graduate student finished in May 1997, I still served, until recently, on graduate committees, advised students, supported a few students on grants, and discussed issues with colleagues in various stages of their professional careers and in many areas of the world. What can I now pass on to students and colleagues? Success is difficult to define. Happiness, as defined by contemporary American culture, is too simplistic and materialistic for my taste. Enjoying one's life is my definition of happiness. As a caveat—no one is entitled to a "free ride" because of family, wealth, or power. Everyone should contribute to societal integrity and the health of the planet. This contribution should be a continuous process, not something postponed to a "convenient" time or special day (i.e., Earth Day). I always asked each new graduate student (and undergraduates seeking advice) how s/he envisioned life at 50 (for the young, a formidable task). Then I would ask—how do you feel about that image? If the feelings were not positive, then I pointed out that something is wrong! I used these questions on myself, which resulted in several career changes and a satisfactory transition to formal retirement but not a cessation of professional activities.

I always tried to help each student determine personal tolerance for risk. Achievements in any field are invariably preceded by various risks. So are major disasters! World-class achievers generally are so involved with their projects that the risks do not paralyze them. Awareness of the risks, as a result of doing their "homework," usually inspires them to exceptional efforts. For most, the price, in energy and time, is unthinkable. For a few, loss of an opportunity is unthinkable. These judgments are a life-long exercise. One can help students and those in early stages of career development to find their own tolerance levels by describing one's own choices, good and bad.

Another essential "leg of the stool" is to associate with people of like mind to the fullest extent possible. Excitement is contagious! Although personal needs undoubtedly vary, I find just one or two highly motivated people that one encounters professionally once or twice a week is satisfactory. In an era of rapid distance communication, trusted colleagues anywhere on the planet will serve very well indeed.

I cannot imagine my career without graduate students! Still, I do not regret their absence in the early part of my career. In retrospect, I was not fully prepared to chair committees due to lack of university experience. Most important, I was not sufficiently well established that students who would fit my research program would seek me out. I cannot choose one period of my professional career over another, but the dismantling of the programs that I had built over decades was a sad period. Easing the impact on students and staff made it bearable.

Although some faculty had more personal relationships with graduate students (e.g., going out with them at the end of the week for a few beers or having them to their home for dinner), I did not feel that such activities were appropriate behavior for me (I do not condemn it for others). I did occasionally go white water canoeing with a group that included one of my graduate students, or I rode with a graduate student to a trout stream where we went our separate ways for a day of solitary trout fishing. Without question, I missed many opportunities for a different relationship with students, but I always believed in limits to the degree to which my private and professional lives should overlap.

Literature Cited

Cairns, J., Jr., D. W. Albaugh, F. Busey, and M. D. Chanay. 1968. The sequential comparison index – a simplified method for non-biologists to estimate differences in biological diversity in stream pollution studies. J. Water Pollut. Control Fed. 40 (9):137-140.

APPENDIX 2

Graduate Committees Chaired or Co-chaired

1968	Sparks, R.E. (MS)	Some Effects of Neutral Mixture of Calcium Oxide and Sulfuric Acid on Channel Catfish Ictalurus punctatus (Rafinesque)
1970	Dickson, K.L. (PhD)	Development and Evaluation of Methodology for the Instream Assessment of the Effects of Water Pollution Upon Macroinvertebrate Organisms
1971	Lorton, E.U. (MS)	The Effects of Thermal Stress on Protozoan Community Structure
	Sparks, R.E. (PhD)	Using the Respiratory and Cardiac Responses of Bluegill Sunfish (Lepomis macrochirus) to Monitor Zinc Concentrations in Water
	Waller, W.T. (PhD	The Use of Fish Movement Patterns to Monitor Zinc Continuously in Water
1972	Lanza, G.R. (PhD)	Effects of Thermal Stress on Microorganisms
	Ruthven, J.A. (PhD)	The Response of Freshwater Protozoan Communities to Concentrations of Various Toxicants Particularly the Heavy Metals, Zinc and Copper
	Yongue, W.H., Jr. (PhD)	The Structure of Fresh-water Protozoan Communities
1973	Crossman, J.S. (PhD)	Recovery and Restoration of Damaged Ecosystems
	Hales, V.M. (MS)	Biological and Chemical Monitoring of Three Streams in the Area of Blacksburg, Virginia (Co-Chair E.F. Benfield)
	Herricks, E.E. (PhD)	The Recovery of Stream Macrobenthic Communities from the Effects of Acid Mine Drainage
	Morgan, E. (PhD)	The Effects of Stress on Fish Behavior
	Sullivan, G. (MS)	Acute Bioassays for Assessing the Toxicity of Six Heavy Metals and Factors Affecting Zinc Toxicity to the Rotifer, Philodina (near acuticornis) (Co-Chair A.L. Buikema, Jr.)
1974	Camp, F. A. (PhD)	The Application of Algal Growth Potential Techniques to Surfactant and Zinc Toxicity Studies (Co-Chair A.C. Hendricks)
	Dolan, J.M., III (PhD)	Comparative Studies of the Toxic Effects of Three Surfactants on a Fish (Lepomis macrochirus Rafinesque) and a Snail (Goniobasis Lea sp.)
	Hocutt, C. (PhD)	The Effects of Celanese Inc., Narrows, Virginia on Chemical and Biological Water Quality in the New River (Co-Chair K.L. Dickson)
	Smrchek, J.C. (PhD)	The Effects of Various Tertiary Treatment Nutrient Removal Schemes Upon the Productivity of Autotrophic (Periphyton) Communities in Model Laboratory Stream Ecosystems
1975	Gregg, B.C. (PhD)	Effects of Chlorine and Heat on Selected Stream Invertebrates (Co-Chair E.F. Benfield)
	Maciorowski, A.F. (PhD)	Heavy Metal Toxicity to Aquatic Insects (Co-Chair E.F. Benfield)
	Prather, I. (MS)	Computer Interfacing with Biological Systems
	Slocomb, J. (MS)	Laser Holography as a Pollution Monitor (Co-Chair K.L. Dickson)
	Stauffer, J. (PhD)	The Distribution of Fish in Relation to Thermal Outfalls (Co-Chair K.L. Dickson)
	Wrenn, W.B. (PhD)	Remote Monitoring of Fish in a Thermally Enriched Zone (Co-Chair K.L. Dickson)
1976	Lee, D.R. (PhD)	Development of an Invertebrate Bioassay to Screen Petroleum Refinery Effluents Discharged into Freshwater (Co-Chair A.L. Buikema, Jr.)
	Messenger, D.U. (MS)	Variability in Sensitivity in Invertebrates to Various Toxicants
	See, C. (PhD)	Effects of Heavy Metals on the Behavior of the Planarian, Dugesia tigrina (Co-Chair A.L. Buikema, Jr.)

	Trotter, D.M. (PhD)	The Development of an Algal Bioassay Procedure using Stigeoclonium subsecundum and the Demonstration of the Effect of Intermittent Chlorination on an Attached Filamentous Alga (Co-Chair A. C. Hendricks)
	Wright, J.R., Jr. (PhD)	Chemical Limnology, Algal Growth Potential, and Nutrient Limitation Factors of the Upper New River, Virginia, and Predictions Concerning Trophic Status for the Proposed Blue Ridge Reservoirs (Co-Chair E.F. Benfield)
1977	Clark, J. (MS)	Evaluation of Methods to Estimate Aufwuchs Biomass (Co-Chair K.L. Dickson)
	Klarberg, D.P. (PhD)	Investigations of the Macrobenthos and Physiochemistry of the Upper New River Basin
	Larrick, S.R. (MS)	Behavioral Avoidance by Fish of Residual Chlorine in Power Plant Discharges (Co-Chair D.S. Cherry)
	McGinniss, M. (MS)	Interactions of Acute Thermal Shock and Simulated Effluents to Daphnia pulex (Co-Chair A.L. Buikema, Jr.)
	Paul, R.W. (PhD)	Leaf Processing and the Effects of Thermal Perturbation on Leaf Degradation in the New River, Virginia (Co-Chair E.F. Benfield)
	Rodgers, J.H., Jr. (PhD)	Aufwuchs Communities of Lotic Systems: Nontaxonomic Structure and Function (Co-Chair K.L. Dickson)
	van der Schalie, W. (PhD)	Use of Minicomputers in Biological Monitoring
1978	King, C.L. (MS)	The Development of a System using the Ventilatory Activity of the Fathead Minnow, Pimephales promelas, to Detect and Predict the Presence of Toxicants (Co-Chair K.L. Dickson)
1979	Giattina, J.D. (MS)	Response of Fish to Chlorinated Effluents under Field and Laboratory Conditions as Determined by Behavioral and Electrophoretic Procedures (Co-Chair D.S. Cherry)
	Honig, R.A. (MS)	Effects of a Simulated Refinery Effluent on Periphyton Communities in Laboratory Streams (Co-Chair A.L. Buikema, Jr.)
	Kuhn, D. (PhD)	A Biological Characterization of Environmental Conditions
	Leslie, M. (MS)	The Use of Biotic Value Allocation in the Assessment of Heated Discharges
	Lubinski, K.S. (PhD)	Monitoring Bluegill Swimming Behavior and the Effects of Sublethal Ammonium Gradients
	Plafkin, J. (PhD)	The Colonization of Artificial Islands by Protozoa in Differing Habitats and Systems
1980	Boatin, H., Jr. (PhD)	Factors Affecting the Endogenous Regulation of Fresh-water Protozoan Communities (Co-Chair W.H. Yongue, Jr.)
	Clark, J.R. (PhD)	Effects of Selected Pollutants on Grazer Utilization of Aufwuchs (Co-Chair D.S. Cherry)
1981	Abbott, T. (PhD)	The Role of Macrobenthos Drifts in the Energetics of Rainbow Trout (Co-Chair A.L. Buikema, Jr.)
	Henebry, M.S. (PhD)	Protozoan Communities, Macrophyte Vegetation and Trophic Status of Northern Michigan Wetland Lakes (Co-Chair W.H. Yongue, Jr.)
1982	Hart, K. (MS)	Effects of Toxicants upon Microbial Colonization Processes
	Lechleitner, R. (MS)	The Resistance of Three Aquatic Insect Detritivores to Fly Ash Constituents (Co-Chair D.S. Cherry)
1984	Doane, T.R. (PhD)	Comparison of Biomonitoring Techniques for Evaluating Effects of Jet Fuel on Bluegill Sunfish (Lepomis macrochirus) (Co-Chair A.L. Buikema, Jr.)
	Peters, G.T. (MS)	Response of Isonychia bicolor to alkaline pH (Co-Chair D.S. Cherry)
	Pratt, J.R. (PhD)	Export of Species from Sources of Differing Maturity and Complexity

1985	Belanger, S.E. (PhD)	Functional and Pathological Responses of Selected Aquatic Organisms to Chrysotile Asbestos (Co-Chair D.S. Cherry)
	Hartwell, S.I. (PhD)	Validation of Laboratory Versus Field Avoidance Behavior of Schooling Fathead Minnows to Heavy Metal Blends Relative to Acute Toxicity During Long Term Exposure (Co-Chair D.S. Cherry)
	Stewart, P.M. (PhD)	Diatom and Protozoan Community Analysis and Colonization on Artificial Substrates in Lentic Habitats
1986	Doherty, F.G. (PhD)	Aquatic Ecology (Co-Chair D.S. Cherry)
	Farris, J. (PhD)	Assimilative Capacity (Co-Chair D.S. Cherry)
1987	Genter, R.B. (PhD)	Species Interactions in Microbial Communities
	McCormick, P.V. (MS)	Patterns of Microbial Community Development in Isolated Aquatic Systems
1988	Clements, W.H. (PhD)	Community Responses of Aquatic Macroinvertebrates to Heavy Metals in Laboratory and Outdoor Experimental Streams (Co-Chair D.S. Cherry)
	Pontasch, K.W. (PhD)	Multispecies Toxicity Tests Using Indigenous Organisms: Predicting the Effects of Hazardous Materials in Streams
1989	Orvos, D.R. (PhD)	Environmental Risk Assessment of Genetically-Engineered Microorganisms
1990	Comeaux, J. (MS)	Transfer of Genetic Information from GEMs to Indigenous Organisms
	Rifici, L.M. (MS)	Investigation into Three Potential Modifying Factors in Larval Fathead Minnow (Pimephales promelas) Growth and Survival (Co-Chair D.S. Cherry)
	Scanferlato, V.S. (PhD)	Environmental Risk Assessment for Toxic Chemicals and Genetically-Engineered Microorganisms: A Microcosm Approach
1991	Atkinson, R. (PhD)	Rehabilitation of Damaged Ecosystems
	Palmer, S. (MS)	Application of Molecular Biology Techniques of the Assessment of Microbial Community Responses to Environmental Perturbations
1993	Hill, S. (MS)	Evaluation of Seed and Seedling Response to Aid Revegetation of Hazardous Chemical Waste Sites
	Arnegard, M. (MS)	Functional Toxicity Tests in Microcosms
1994	Dobbs, M.P. (PhD)	Ecotoxicology (Co-chair D.S. Cherry)
	Holl, K.D. (PhD)	Vegetation and Lepidopteran Succession on Reclaimed Coal Surface Mines
	Sabre, M. (MS)	Restoration of Disturbed Ecosystems Using Wildflowers
1995	Jones, D. (MS)	Macroinvertebrate Richness and Abundance of Accidental Wetlands on Surface Mines
1996	Comeaux J. (PhD)	Effects of Copper on Benthic Communities in Artificial Microcosms
1997	Heckman, J. (PhD)	Restoring Functional Attributes of Damaged Ecosystems

Post-Doctoral Fellows and Research Associates

Fred Benfield	Dennis T. Burton	S. K. Case*	Donald S. Cherry
J. K. T. Eu*	J. M. Fournier*	H. Fujii*	David Gruber
P. F. Lai*	Kenneth W. Thompson	G. F. Westlake	J. R. Pratt
Kay H. Austin	David R. Orvos	Paul V. McCormick	Robert B. Atkinson

*On NSF grant on which S. P. Almeida was co-principal investigator

Sabbaticals

Rodger H. Green, University of Western Ontario
Roger L. Kaesler, University of Kansas
Jin Hongjun, Nanjing University, China
Rex L. Lowe, Bowling Green State University
Shen Yun-fen, National Academy of Sciences, China

APPENDIX 3
paper from former graduate students

The Graduate Student Era: 1985-1991

David R. Orvos[1] and Vjera S. Scanferlato[2]
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[2]via Torino 43/4, 10045 Piossasco TO, Italy

The University Center for Environmental Studies was a somewhat different place in the late 1980s than earlier generations of students may have recalled. While University funding was far less and the total number of graduate students was less, the research conducted continued to span a diverse array of subject areas. Even though money was tight, students were still expected to present results at regional and national meetings. They did! Productivity per student never wavered. John ensured sufficient funding for his students as long as he observed reasonable progress in that student's data collection and analysis efforts. The University Center for Environmental Studies became the University Center for Environmental and Hazardous Materials Studies in 1988 to reflect the broadening of the Center's mission. Students continued to concentrate themselves on the first floor of Derring Hall with Center labs on the first and second floors. The "elder" students who left in 1985-1986 included Paul Stewart, Dick Pratt, Bob Genter, and Paul McCormick. Will Clements was remaining resident of Derring 1027 and was eventually joined by Kurt Pontasch, Keith Sappington, and David Orvos. As always, Bobbie Neiderlehner was there to keep us in line - both personally and professionally!

The graduate student research activities from the mid-1980s to 1991 centered on the examination of anthropogenic stresses to ecosystems. We classify the research into three main categories: environmental risk from release of genetically-engineered microorganisms (primary graduate students were Orvos, Scanferlato, Palmer, and Comeaux), macroinvertebrate responses to xenobiotics (Clements and Pontasch), and wetlands ecology and restoration (Atkinson). Protozoan and ecological impact studies also continued under Bobbie Neiderlehner, Paul McCormick, and Dick Pratt.

The examination of potential effects from genetically engineered microorganisms started in a rather unique location - the kitchen of Clete Sellers in Harrisonburg, Virginia. Sellers was Orvos' Master's advisor at James Madison University and had invited John to give a seminar in the Fall of 1984. After talking with Orvos at length about the promise of biotechnology as well as its potential risks, John proposed the idea of conducting research at Virginia Tech complete with drawings on the back of paper napkins. At the time, Orvos was finishing his fish physiology thesis with a strong interest in cellular and molecular biology but with little knowledge of applied ecology and no knowledge, per se, of risk assessment. However, the initial contact with John Cairns was such that Orvos soon initiated a dialogue that led to his coming to Blacksburg in August, 1985.

At the time, the Center was poorly equipped to initiate such a research program and, with the assistance of Dick Pratt, a Virginia Water Resources Research grant was obtained that provided some equipment and supplies for initial data generation. We soon found out that to conduct GEMs research, it would be useful to have in our possession a GEM! Fortuitously, Orvos' wife, Andrea, was enrolled in a Plant Pathology class whose teaching assistant worked for Dr. George Lacy of Tech's Plant Pathology, Physiology, and Weed Science department. Lacy was willing to work with the UCES and provided his GEM, *Erwinia carotovora*, to us. *Erwinia* is a pectinolytic organism that causes the familiar vegetable soft rot often seen when food is left in the refrigerator too long. Lacy's group had engineered the bacterium so as to remove that part of the genome that produced the pectinolytic proteins. Lacy's excellent track record in phytobacteriology and molecular biology proved to be advantageous to development of GEMs research at Tech. Orvos helped Lacy move into a new state-of-the-art molecular biology laboratory that enabled Orvos and others to pursue GEMs research from both molecular and organismal levels.

Preliminary GEMs research continued through 1986 both at Lacy's and UCES laboratories. Lacy, Cairns, and Orvos wrote several grants. Microcosms were developed using either Pandapos Pond for aquatic microcosms or farming soil for terrestrial microcosms. The fall of 1986 brought two events that would dramatically affect the GEMs research: the awarding of $298,000 in joint research funds by the NSF and EPA

and the awarding of the SETAC Pre-Doctoral Fellowship to Orvos. Funds became available to purchase equipment and supplies as well as fund graduate and undergraduate researchers. Two additional students were brought into George Lacy's lab and three students, Vjera Scanferlato, Sarah Palmer, and Jay Comeaux, eventually joined John's group during 1987-88. While parts of these students research addressed non-GEMs issues, so as to improve their subsequent marketability, all played critical roles at various phases of the project. Scanferlato adapted her microcosm studies to include heavy metal stresses and Palmer published work that examined use of DNA:DNA hybridization in pollutant stress quantification.

The GEMs project progressed very well but did upset many in the biology department and Provost's Office at Tech. "Space wars" and "overhead battles" were common and the administrative headaches of such an interdisciplinary grant often caused the loss of sleep. But, the project moved forward. A post-doctoral research associate, Kay Austin, came on board in the Spring of 1988; Orvos and Lacy traveled to the United Kingdom in 1988 to present a paper; Scanferlato and Orvos took a contingent of four undergraduates to the 1989 American Society for Microbiology meeting to present their results; a total of 13 undergraduates eventually worked on the project; and approximately ten manuscripts resulted from the research. However, the crowning achievement, in John's and our eyes, was when the Virginia Tech Research Office, after surveying all biotechnology-related activities on campus, mentioned that only the Center's research appeared to be seriously competitive with biotechnology-related research programs at other land grant institutions. This, of course, was at a time when Tech administrators continued to decrease the budget and belittle the interdisciplinary approach of the Center. These same administrators were reluctant to acknowledge the findings, but then did find $50,000 to purchase equipment that would benefit all of John's research programs.

Other research areas were as equally important. Macroinvertebrate responses to xenobiotics were investigated by Will Clements and Kurt Pontasch using both field and laboratory approaches. Clements, currently at Colorado State University, came to the Center from Florida State University and examined heavy metal effects on aquatic insects. Clements won the Cunningham Dissertation Fellowship and, after leaving Tech, was awarded the first SETAC Post-Doctoral Fellowship. Pontasch, presently at the University of Northern Iowa, came to Tech from the University of Idaho and examined macroinvertebrate response to complex industrial effluents using a novel paddle-wheel microcosm in his laboratory studies. Both Clements and Pontasch graduated in 1988 and continue to maintain aggressive research programs. Two of Clement's graduate students have been awarded SETAC Pre-Doctoral Fellowships in this decade.

Restoration ecology studies, discussed in more detail elsewhere in this book, took on new vigor at the Center in the Fall of 1988. Rob Atkinson, a friend of Orvos' from James Madison University, took little persuading to come to Tech after visiting in the Spring. Atkinson's long-standing interest in wetlands and desire to explore restoration techniques in non-tidal freshwater systems seemed a natural match to John's desire to once again conduct research in restoration ecology. His career at Tech proved quite successful as his dissertation developed new methods for evaluating created wetlands associated with Section 404 of the Clean Water Act and was the first assessment of nontidal wetland mitigation in Virginia (published in the journal *Wetlands*, a book chapter, and elsewhere). Before graduating, Rob wrote a successful proposal with John to the Department of the Interior to design wetland habitats to be left after surface mining. Atkinson stayed on for another four years conducting that research . He is presently at Christopher Newport University and was recently awarded $700,000 for a study of restoration of an endangered ecosystem: Atlantic White Cedar swamps.

Sarah Palmer is presently on the staff of the University of Arizona where she is responsible for assessing the impact of GEMs releases and rDNA safety issues. Vjera Scanferlato currently lives in Piossasco, Italy where she is involved with a research project investigating bio- and phytoremediation of sediments dredged from the Venice lagoon. David Orvos is at Sweet Briar College and is the founding chairman of the Department of Environmental Studies.

Many professors continue to conduct research in the same general area as they did when they themselves were graduate students. Not John Cairns. If nothing else can be garnished from the text of this chapter, it is John's willingness, actually insistence, on changing research directions "every five years or so" as he would often say that should be clear. We know of no other professor at any time that had the research diversity present in his laboratory as John Cairns did in the late 1980s. While some criticized him for this, those of us who experienced it believed that it strengthened each of us in different ways. All of the students learned from each other, got along well together, and still stay in contact with each other. Who, but John Cairns, would tackle a problem like GEMs or wetlands restoration with minimal equipment and facilities and reap immense success? Who would do it repeatedly throughout his career? How many other advisors put the amount of trust in their students as John did?

One of the superb advantages of working with John was the constant flow of scientists who visited Blacksburg or met with him privately at various national meetings. One of us, Orvos, fondly recalls the pleasure of meeting with and standing in awe of such scientists as Ruth Patrick, Linda Birnbaum, John Harte, Gene Odum, Peter Day, Larry Slobodkin, and Paul Erlich. We doubt if any other program provided so much opportunity for interaction.

Graduate students came and graduates students left Blacksburg throughout this period. The one point of consistency that prevailed was John's right arms: Darla Donald and Bobbie Neiderlehner. We both had the privilege of working with them and firmly believe that if Center students have had success after graduation it was due, in no small part, to the efforts of the both of them to improve our grammar, writing, statistical analysis, lab technique, experimental design, and personalities! We know many others share this belief.

John Cairns, Jr., passed on many things to his students over the years. While his pedagogy, beliefs, and work ethic were passed on to all generations, we both experienced his concern for problems of a personal nature. His insight and understanding into the troubles of Yugoslavia, Scanferlato's home, and the stress of having family there or the health of Orvos' elderly parents meant a great deal to the both of us. Many have seen the professional side of John, but we believe relatively few have seen the personal man.

In finishing this accounting of Center history, one of us, Orvos, reflected on two things. As a scientist for SmithKline Beecham, I was sent in 1995 to a weeklong academic/industrial conference in France which consisted largely of French and U.K. scientists. When they learned that I was educated at the Center, I was left with the distinct impression that the Center had the reputation of being a skyscraper-type structure with a staff of at least dozens, with productivity very appreciated by the Europeans. I did little to dispel the belief, but only wished that someday Virginia Tech would realize what, in fact, they had on that campus. Finally, one of the things that has always impressed me the most about John is his "extended" family. The students, spanning two generations, and now the students of those students. To borrow and modify something I read just this morning in *Chemical & Engineering News* (27 July 1998, page 62), 'As a biologist, one's legacy is not the science that is left behind. It is the students one has trained." If that is an acceptable criteria for success, then John Cairns, Jr., has indeed been among the most successful biologists.