Award Address

Academic Excellence—The Role of Research[1]

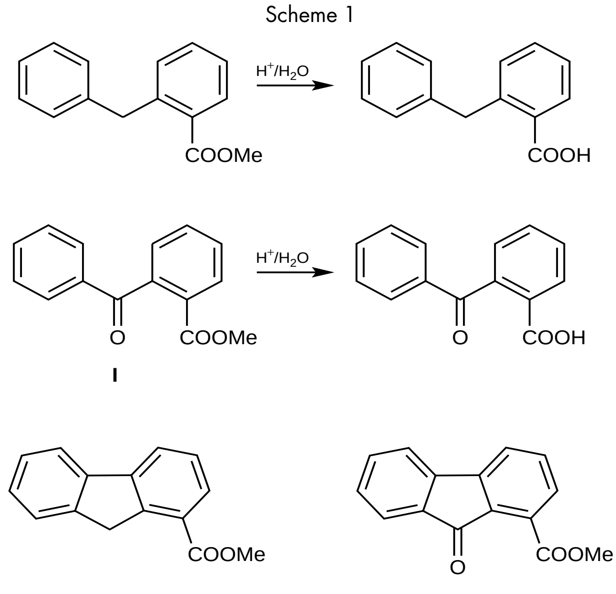
2002 George C. Pimentel Award, sponsored by Dow Chemical Co.

by Michael P. Doyle

On special occasions such as this, the acknowledgment of persons and circumstances that have brought me to this stage is most appropriate. They are many, and I will not be able to recognize them here with sufficient gratitude. George Pimentel is a hero whose dedication to research and education have inspired my activities. My career choice to be a chemist, however, I owe to Brother Bernard of De La Salle High School in Minneapolis, MN. He provided the attraction that drew me into this rewarding discipline. Although only five feet in height, he rose much higher in his enthusiasm for the intricacies of the discipline. In college organic chemistry, the first edition of Morrison and Boyd's book captured my mind, perhaps because that was the first college chemistry course in which I excelled. But it was the process of discovery that held me captive from the first weeks of Qualitative Organic Analysis with the textbook of Shriner, Fuson, and Curtin.

For me the first experiences in research were not what you might have expected. Like many students in the 1960s I enrolled for a Senior Year Independent Project. I don't believe that it was actually called research, and those of you old enough to remember will recall that you really couldn't engage in this activity before your senior year. In any case, I accepted a project to determine the rate of hydrolysis of a methyl ester of 2-benzoylbenzoic acid (I) and compare the rate constant to that of the methyl ester of fluorenone-2-carboxylic acid (II) (Scheme 1). The purpose was to evaluate if there was anchimeric assistance enhancement in the hydrolysis of the fluorenone-2-carboxylic ester due to the fixed geometry of its carbonyl group. The rate constant for the hydrolysis of the methyl ester of 2-benzoylbenzoic acid had been previously determined by Professor Joseph Bunnett's research group at University of California, Santa Cruz, and Dr. Richard Morath with whom I was working had been a postdoc with Joe before beginning his academic appointment. Well, the rate constant that I obtained for the standard system was different from the published number!

Scheme 1



I went to the professor's office to inform him of the result, and you can imagine his reaction. "Go back to the lab and repeat the measurement!" I did that and obtained the same number. Disappointment was evident, but I was allowed to continue. Five years later, in my first year as a faculty member, I met Professor Morath at the spring ACS meeting in Minneapolis. He asked if I remembered the problem, and did I wish to know the outcome. The year after I graduated, he informed me, he "put a *really good student* on that project," and, to his surprise, that student obtained the same number that I had determined. Since that number was different from the published value, he "dropped the project." But had I seen the latest *Journal of Organic Chemistry*? In it an article by Bunnett reported in a footnote a correction to the previously reported value for hydrolysis of the methyl ester of 2-benzoylbenzoic acid, and to his surprise, the new value was identical to the one that I had determined five years previously.

I'm fairly certain that I didn't really understand the meaning of research then, but if my limited experience is an indicator, research was defined, as it is in most colleges and universities today, as investigation or experimentation aimed at the discovery and interpretation of new information. I read this in my role today as efforts undertaken in the advance of science. Still there are those who confuse research with personal discovery. Students who measure the amount of vitamin C in orange juice are not performing research, even though they are discovering this information for themselves for the first time. Similarly, the person who is repeating a procedure previously performed by someone else is not conducting research, even though the material being prepared is essential for the subsequent study that will provide new materials.

I did not invent research with undergraduate students; I merely developed and promoted a process that had existed for many years. Although I do not have concrete evidence for this, I believe that these engagements began with faculty dedicated to the advance of science in environments with limited or no access to graduate students. That was my position when I began my career at Hope College.

Award Address

In my first years as a faculty member I was teaching, as were my colleagues, twelve contact hours—two lecture and two laboratory courses. I was also designated as the person in charge of the NMR, which allowed me to occupy the air-conditioned office in which it resided. My laboratory was a small alcove on the second floor, and delivery of gas tanks without an elevator in the building meant that they were carried up the stairs by hand. But what I had that faculty today don't seem to have, at least to the same degree, was time, despite having what are seen today as high teaching loads. In my first two years I lived in a rented house that was a mere ten-minute walk from my office, so that I could even go home for lunch with family. My wife Janice supported my professional efforts by handling virtually all of the important personal operations of home and finance. I was not assigned to any time-consuming committee until my sixth academic year. As important from a professional standpoint, however, an environment existed in the chemistry department at Hope College that promoted research with undergraduate students as a primary expectation of its faculty. There were discussions through the year, which placed alarm into all young faculty, that if half of the faculty did not have external support we would not prosper.

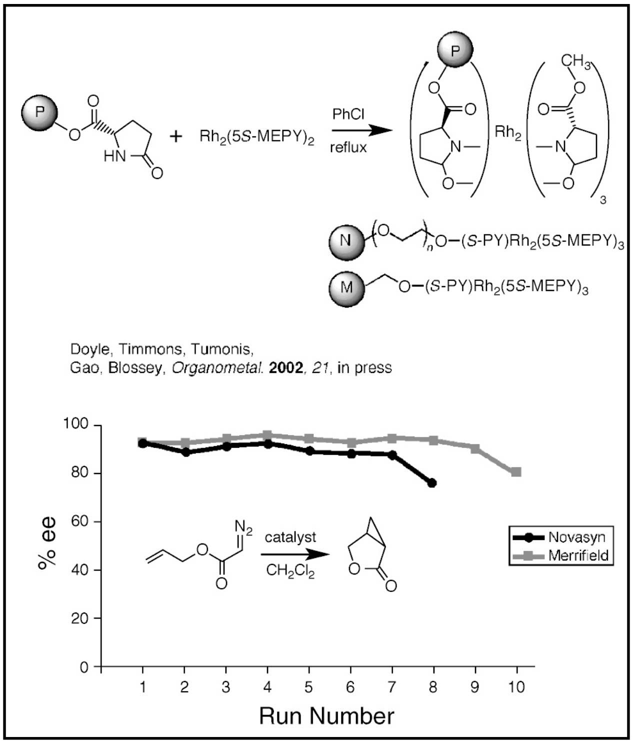
Hope College had an endowment of less than $10 M when I began. Tuition provided about 85 percent of operating cost, and that meant salaries were low. Then, faculty were retained by promoting them rapidly so that their departure was made more difficult. However, despite financial constraints, I had start-up funds from Hope, and Hope never refused to provide matching costs for instrumentation. Department budgets were significant and allowed flexibility in support for teaching and research, including gases and solvents for research. One of the characteristics of those departments that have been regarded as centers of excellence over the past half-century is that there are virtually no restrictions on professional development—supportive start-up packages, provision of matching costs for instrumentation, flexible department budgets, a sabbatical leave program, are among the benefits. In such environments there is little need to discuss indirect cost return.

In the 1960s and through the 1970s there were few funding programs that were specifically earmarked for faculty at undergraduate institutions. In federal programs all faculty were in one category. However, there was relatively more funding available, and access to this funding depended then, as it does now, on personal initiative and productivity. It's difficult to receive a grant if you don't submit a proposal. I recall that at Hope I was expected to submit a proposal prior to my arrival (ACS Petroleum Research Fund was my choice) and one during my first year (to Research Corporation). Also incumbent on the new faculty member was the writing of a department proposal for instrumentation (to the National Science Foundation). And although we were told that such things were not possible, we did apply for and receive funding from the National Science Foundation for research before there were separate programs designated for faculty at undergraduate institutions.

In all due respect to the compliments that I have received this morning, it was the students who made this happen. Where would I be if Wendell Wierenga had not, during my second academic year, in only twelve months including a summer, undertaken research with me prior to attending graduate school? That research resulted in one communication in *JACS*, two subsequent full papers in the same journal, and four additional publications from research that he began in that year *(1)*. Wendell could come into the lab during the ten minutes between classes and set up chemical reactions. He was the model of efficiency.

And what about the enthusiastic Melissa Vasbinder who began her work with me after her junior year in high school? Highly recommended by her high school teacher, who had herself participated in a research program designed for high school teachers, Melissa came to the lab after her last day of exams. She asked, "What will I do today?" To which I answered, "Today, Melissa, you will make methanesulfonyl azide, and this requires that you mix methanesulfonyl chloride with sodium azide *(2)*. If, somehow, water enters the flask, its reaction with methanesulfonyl chloride will produce hydrogen chloride, and that, in turn, will produce hydrazoic acid which is a poison. If you breathe that you will die!" Well, Melissa made methanesulfonyl azide that day without difficulty in yields that were comparable to those of experienced undergraduate students. That afternoon she asked, "What will I do tomorrow?" "Tomorrow," I replied, "you will make an ether by a time-honored procedure (the Williamson ether synthesis) that involves

Scheme 2



adding sodium to benzyl alcohol (*3*). Reaction between the two produces hydrogen gas. If somehow you ignite that hydrogen by a spark or a flame, an explosion will occur and you could die!" A bit nervous, Melissa completed the reaction in record time with a yield of distilled product at least as high as has been reached before. Later that day she came to my office; highly excited, she said, "Gee, I love chemistry!"

After moving to the University of Arizona, a grand partnership was undertaken with sophomore Jennifer Tumonis, postdoctoral associate Daren Timmons, and myself, and with Erich Blossey at Rollins College, to attach dirhodium catalysts to polymers and investigate their reactivity and selectivity (Scheme 2) *(4)*. Here Jennifer had to learn how to make these polymer-bound catalysts and to use them in reactions with diazo esters, as well as become accomplished with GC, NMR, and IR methods. Within a year we had interesting results, including many of Jennifer's, ready for publication. (In addition to reporting regularly at our weekly research group meetings, Jennifer also takes part as a reporter at our literature meetings, and she asks thoughtful questions and makes insightful comments as well as anyone in my research group.)

But you didn't come here solely to learn about the talented people who have worked with me and made me look good. Many of you are interested in the results of a study entitled "Academic Excellence: The Role of Research in the Natural Sciences at Undergraduate Institutions." Funded by The Camille & Henry Dreyfus Foundation, The Keck Foundation, the M. J. Murdock Charitable Trust, the Welch Foundation, and Research Corporation, this study examined the environment for research at predominantly undergraduate institutions, particularly the 136 private and public institutions that participated in the study *(5)*. Multiple sources were used for the study in addition to the institutional surveys, especially data from the National Science Foundation.

There were expected outcomes and many surprises. One of the surprises was

Award Address

the observation made that forty percent of institutions whose natural science faculty received research awards funded by the combined total of Research Corporation (CCSA), PRF (Type B), NSF (RUI), and NIH (AREA) had only one or two of these awards over the fifteen-year period 1986-2000 (Figure 1). This accounting is for all natural science faculty in these institutions. There was no continuum in fundable research activity at these institutions or, in the words of my distinguished colleague, John P. Schaefer, support for these institutions was "money down the rat hole."

Anecdotal information has often presented a scenario in which faculty from predominantly undergraduate institutions are not competitive for limited funding of research. However, total grant dollars for research increased from 1986 to 2000 with awards made at a greater than thirty percent success rate. By comparison, the number of proposals submitted during that same period, from a faculty that expanded by more than twenty percent, remained relatively constant (Figure 2). One sometimes forgets that the NSF-RUI program began only in 1984, and the NIH-AREA program was begun two years later. The conclusion that can be drawn from this is that research at predominantly undergraduate institutions has not been limited by the availability of external funding. Proposal submission appears to be the rate-limiting step.

Some may be surprised to learn that faculty at predominantly undergraduate institutions published their research at a rate of 0.54 publications per faculty per year during the 1990s. Is that number high or low? The variation between public and private, baccalaureate and advanced degree-granting institutions is insignificant. Here a comparison to other institutions is useful: the average number of peer-reviewed publications per graduate student and postdoctoral associate per year in Ph.D.-granting chemistry departments is 1.00. And that number of publications divided by the number of graduate faculty is 3.75. For M.S.-

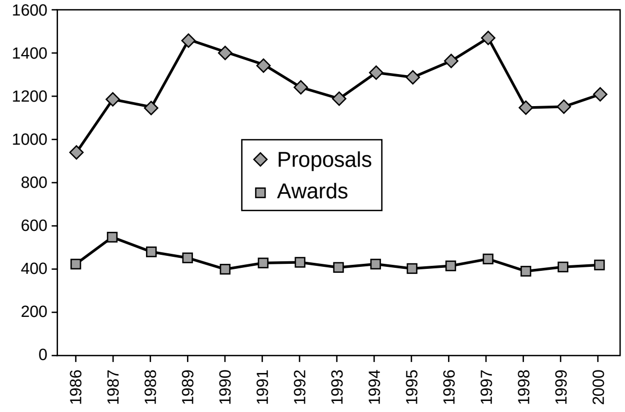


Figure 2. Research Corporation (CCSA), PRF (Type B), NSF (RUI Research) and NIH (AREA) Combined Activity 1986–2000.

granting departments the number of publications per graduate student and postdoc is 0.96, and the number per faculty member per year is 1.18. So the number 0.54 is actually pretty good in view of the greater teaching loads, resource availability, and personnel available to faculty at predominantly undergraduate institutions.

The survey received more than 2,900 faculty responses, fully two-thirds of all natural science faculty at these institutions. To our surprise, faculty reported that only one quarter of their peer-reviewed publications had undergraduate student coauthors (Figure 3). This could be due to underreporting, but my own review of the data suggests that this fraction is close to the actual value. How many institutions have instituted pre-tenure sabbaticals just so faculty could engage in research, often outside of the resident college? And as we have already published *(6),* the fraction does not improve when the selection of faculty surveyed is limited

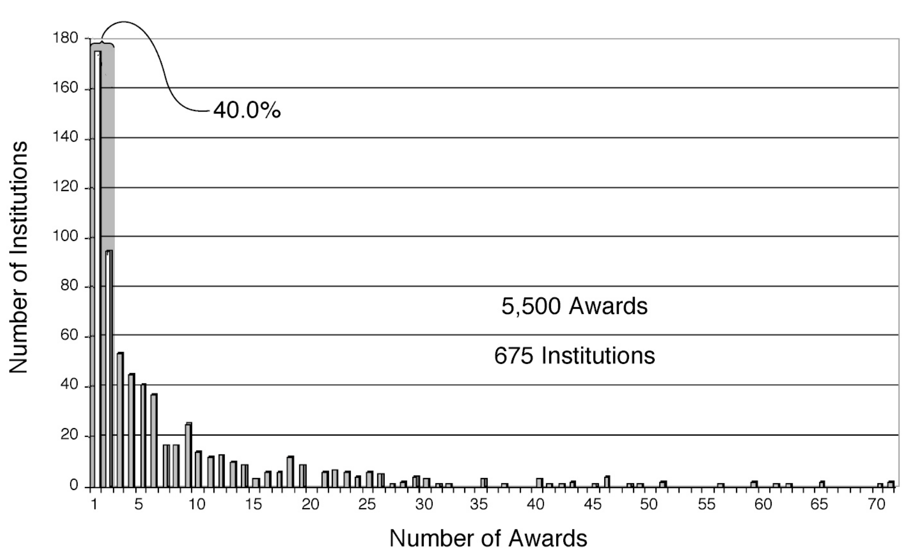


Figure 1. Total Frequency of Research Corporation (CCSA), PRF (Type B), NSF (RUI Research) and NIH (AREA) Awards 1986–2000.

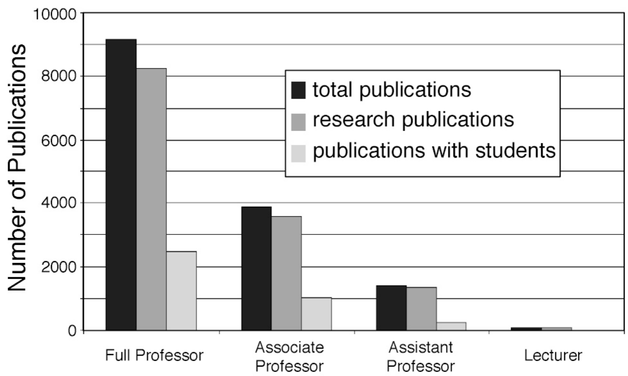


Figure 3. Publications by Rank from All Institutions 1991–2000.

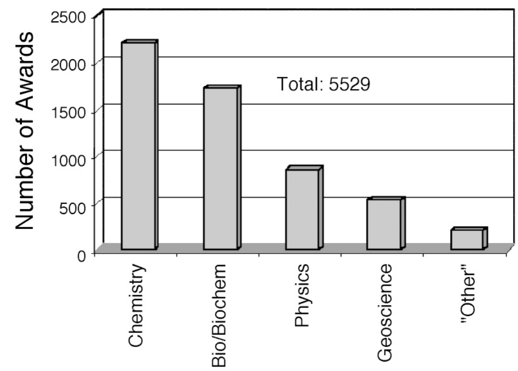


Figure 4. Disciplinary Funding from NSF, Research Corporation, NIH and PRF 1986–2000.

to those who have three publications per year, rather than the average 0.5, or to those who had $1,000,000 or more in external funding during the ten year period (1991–2000) of the survey. Publishing research with undergraduate students is not as common as we expected.

How do chemists measure in these surveys? Surprisingly, those at predominantly undergraduate institutions operated their research programs during the 1990s at lower levels of external funding through individual research awards ($13 K per faculty member per year) than did their colleagues in biology, physics, or the geosciences *(7)*. Their publication rate in peer-reviewed journals was, however, comparable to faculty in other departments. This fact was curious because the resources available to chemists (NSF, NIH, Research Corporation, PRF) has always seemed to be greater than the resources available to faculty in other departments (Figure 4).

Award Address

Is this because chemists can get along with fewer dollars? Or are fewer chemists becoming involved or remaining involved in research? I suspect the latter is a more accurate assessment.

Several of the 2,900 faculty respondents criticized us for not requesting student presentations at local, regional, and national meetings. They believed that these presentations were measures of research activities in their operations or those of their institution. We did not. Such presentations may be reflections of activity, and they have real educational value, but they do not serve directly in the advancement of science. Taking a problem to a publishable conclusion is what leads to the advance of the science. Having students present their work at meetings such as this one is a grand educational opportunity that builds enthusiasm for the science and perhaps a career in science, but only the completed work that is published can impact future science and, I must add, convince foundations of the advisability of future support. “Publish or perish” this may be, but the advantages for the science and for career opportunities for the faculty member and students far outweigh any disadvantage.

We have observed an increase in foundation support for departmental and multi-departmental programs. The most obvious one has been institutional support from the Howard Hughes Medical Institute with over $100 M in support for predominantly undergraduate institutions during the 1990s. Research Corporation has done the same, but on a more modest scale. Whether or not increased institutional support will have a more dramatic effect on research in undergraduate institutions than individual research awards remains to be seen. Accountability is a term that is not easily assessed in either a block grant or an individual grant program, but efforts are underway to determine over a long term reasonable measures of success.

What has this study accomplished? I believe that, first and foremost, the availability of this information makes possible better-informed decisions by faculty, by administrators of colleges and universities, and by foundations. It also diminishes misconceptions, such as those related to the availability of funding for research. Critical questions can be addressed. How do we fit into the mainstream? Where do we excel? What deficiencies are in need of correction? What was surprising to all of us who worked on the study was the paucity of post-secondary-education-related information from the American Chemical Society relative to that from the American Institute of Physics.

“Academic excellence!” What is the role of research? Research, especially in this age of heightened awareness of technology transfer, is just such an enterprise—the transfer of knowledge, technique, enthusiasm, and vision in the solution of scientific problems to students. The student grows in understanding and ability and may follow in the mentor’s footsteps. The mentor creates an environment that fosters professionalism and the joy of discovery—and by publishing results and receiving awards provides heightened opportunities for those who partake in the enterprise.

Acknowledgments

To the 119 undergraduate students who have published papers with me in peer-reviewed journals, nearly half of the students contributing to two or more. To those postdoctoral associates who assisted in these ventures. To the agencies and foundations that provided the funding for the research undertaken. To those individuals who gave to me their personal attention and support, especially during the early years of my career.

Notes

1. This article is based on the award address for the year 2002 George C. Pimentel Award in Chemical Education, sponsored by Dow Chemical Company. The address was presented at the American Chemical Society Meeting in Orlando, FL, on Tuesday, April 9, 2002.

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