

CUR Dialogue Address - February 24, 2003

I'd like to thank the organizers of this conference for inviting me to give the opening plenary address. I should point out that someone important from the National Science Foundation was supposed to talk in this slot, but when she had to cancel her appearance because of a conflict that arose, I was brought in off the bench and am serving today as CUR's "relief plenary speaker".

My goal today is quite simple. I want you to leave this conference with the intent of submitting a proposal to an external funding agency in the near future, and I want you to follow through on that intent. It's my contention that if you have interest in doing research with undergraduates, and if it costs money to do your research, then you ought to submit grant proposals in an effort to support your work.

If you were to examine the funding rates of almost all programs that accept proposals and provide support for undergraduate research, you would find a success rate of about 1 in 3. I'll state at the outset that I think these are exceptionally good odds, ones that should encourage us rather than discourage us from submitting grant proposals. I'd also point out that these odds apply to people at graduate institutions as well, so that no one should feel as if one group has some competitive advantage over the other. In fact, if anything, getting grants at an undergraduate institution might have certain advantages over getting grants at a graduate institution. I should also point out that as much as you might think that my receipt of more than 1.5 million dollars in funded grants is pretty good for someone at an undergraduate institution, a larger number would be the dollar amount of grants I've submitted that have been rejected.

As I look back on my career, I realize that, in many ways, I was the beneficiary of a fortunate set of occurrences that contributed to my own willingness to submit proposals to granting organizations. Although I add that, at the time, I hardly would have considered some of them fortunate occurrences. First of all, I realized some years after completing my Ph.D. degree that I actually had a wonderful thesis mentor. He had this attitude that, if someone was giving away a dollar, you had an obligation to be in line

asking for it. He, along with a long-term post-doctoral associate in our group, wrote many proposals to granting organizations in an effort to support our work. Many of these were successful, but some weren't. Everyone in the group knew about what was being submitted, and we were informed when proposals were successful and when they weren't. But I still remember quite vividly the day in my third year of graduate school when my Ph.D. advisor came into my lab and said "Tom, the NSF grant you're working on is going to end soon, and if you want to continue to be supported on a research assistanceship rather than teaching assistanceship, you'll need to write a successful renewal request." He then handed me a complete copy of the proposal that had been supporting my work, and informed me that he would need a draft of the renewal proposal within a month. Now I have to admit that I thought this was yet another example of what a jerk he was, and just another of his unreasonable requests. And when I gave him a draft a month later and he put more ink on the paper than I had, I thought he was an even bigger jerk. But six months later when the proposal was funded, I still remember him coming in to the lab beaming, vigorously shaking my hand, and congratulating me on the excellent job I had done writing the proposal. Even though I still think more of the ideas and writing ended up being his rather than mine, he never relented on his insistence that my contribution to the effort was the crucial part in our securing funding. In addition to a sense of accomplishment that I might actually be able to write successful proposals, I also learned first hand that there were organizations out there that you could ask for money, and that they might actually give you some. I still have to say that I'm a bit in awe of this system we have where you ask people for money, tell them why, and then they sometimes give it to you. I mean, is this a great country or what?

The other thing that in hindsight may have been fortunate was my situation when I started at Bates. We all know the importance in the sciences of start-up funds, and my start-up package at Bates was a generous "publish one for the College speech" from my Dean, with the reminder that they were behind me 100% so long as I didn't need any financial resources from the college. With 180 square feet of dedicated lab space, none of the equipment I needed to undertake my research, no money, and seven-course equivalents a year of teaching requirements (the teaching load at Bates is now five course equivalents a year), I was under the impression that I had found this wonderful faculty position that would allow me to do lots of research. I had no choice but to submit grant proposals if I expected to get anything done in my early years at Bates. I am proud of the nine equipment grants I've gotten at Bates, five through instructional programs and four through research grants.

And the college ultimately did show their commitment to me by agreeing to match each of those grants.

But perhaps the greatest endorsement of all came from a program officer at NSF by the name of John Hunt. John is now one of my heroes. Early in my career I needed a high quality fluorescence spectrophotometer and so submitted a research equipment proposal to NSF. John was the program officer who handled my request. Six months went by with no response on whether I'd receive funding. But I was a nobody from nowheresville so I sat quietly by. When this stretched to eight months I finally had the gumption to call Dr. Hunt and ask whether he could tell me what was going on with my proposal. He said something about the slow pace of considering proposals, having to weigh decisions relative to proposal rankings, uncertainty in knowing exactly how much money he had, etc. It all struck me as a bit evasive, but who was I to question a program officer at NSF. He had the power, I did not. And he said I ought to know soon. Two months later when I still hadn't heard, I decided to throw caution to the wind and call Dr. Hunt again. And this time he leveled with me. It turns out that he wanted to fund my proposal, but that he had higher priority ones in the round it got considered in, many of which were for big ticket items from research universities. Since mine was a relatively small request, he had hoped that he'd have what essentially amounted to pocket change left over that he could use to fund it. When he didn't, rather than reject the proposal, which is what he should have done, he hung onto it for the next round of funding in the hopes that he might have some pocket change this time. And sure enough, two months later, and a year after I had submitted the proposal, he called to say he could fund my request. About thirteen years later, when serving as CUR's president and while visiting people at NSF with the other CUR officers, we had a visit with a John Hunt. I have to admit that I didn't place the name, but early in the meeting John looked at me and said "remember me, I'm the person who funded your fluorescence instrument". I couldn't believe he remembered me. While I think many of us tend to think that people at granting agencies are not inclined to value and support people at undergraduate institutions, my experience with John and many other program officers over the years suggests just the opposite conclusion.

I also had some other fortunate occurrences during my job search. For one thing, I interviewed at Oberlin College, an institution with a rich track record of successful undergraduate research. I remember quite vividly a conversation with Norman Craig during my interview as we were walking back to the chemistry building from lunch. He said "no matter where you end up next year (which made me think, at the time, well I guess it won't be at Oberlin - although I did get an offer), you should make sure you submit a research proposal before you actually start your position." Now it turns out that the University of Colorado, where I was a Ph.D. student, had recently gone through the process of hiring a new analytical chemist. I had talked with him prior to sending out job applications and beginning the interview process, and I learned from him that grant programs existed at places called Research Corporation and the Petroleum Research Fund, and that they had specific programs for faculty members at undergraduate institutions. Norm Craig, who subsequently received the American Chemical Society's award for undergraduate research, was clearly this revered person at Oberlin with the obvious respect and admiration of all of his colleagues. It seemed imperative to me that if Norm Craig was giving advice, then I had better follow it. So when I interviewed at Bates about a week later, I mentioned my intent to submit proposals to Research Corporation and PRF once I had secured a position.

And I followed through on this, submitting proposals to both Research Corporation and PRF in the spring preceding my start at Bates. Both of these proposals described work I wanted to undertake on the use of luminescent lanthanide ions as detection chromophores in liquid chromatography. In October of my first term at Bates I learned that my Research Corporation proposal had been funded (so I now had a whopping \$10,000, and possessed a feeling like I had just won the megabucks lottery?). I had money to buy a fluorescence detector for liquid chromatography. Now all I needed was a liquid chromatograph (an NSF instructional equipment grant soon after provided that). About two weeks after receipt of the good news from Research Corporation I learned that my PRF proposal had been denied. The reviews of my PRF grant brought home to me some of the harsh realities of grant writing. In one of the reviews was a statement that read something to the effect of "it would be useful if the PI knew something about fluorescence, and only when he knows something about fluorescence should he

consider sending in another proposal having to do with a fluorescent detection method." The nerve of this reviewer; holding back to spare my feelings. Come on, just tell me what you really think. But when I examined the particular statement that set off this reviewer, I realized that there was a rather egregious error that I had in my understanding of one aspect of fluorescence. The result was that I went back and read a lot about fluorescence and learned lots of subtleties about the field that I had not learned in either my undergraduate or graduate education. Another result is that I'm now more thorough and careful now when submitting grant applications.

Now you might wonder how the same idea and essentially the same proposal that was rejected by PRF could have been funded by Research Corporation. Well it turned out that the application to Research Corporation had to be a lot shorter than the one for PRF, and the erroneous statement ended up on the cutting room floor in the revisions that I had to do to shorten the proposal.

What I did have at that time was a good idea for research. How do I know that? Well, I did get a grant funded, so that was one sign. The other thing was that we were able to publish the first paper that described the use of lanthanide ions for detection in liquid chromatography. And finally, after our pioneering paper was published, several laboratories around the world began to do work on lanthanide luminescence detection in liquid chromatography. Unfortunately, all of these laboratories had combinations of graduate students, post-docs, and technicians, so that the work that I might have undertaken over the ensuing 10-15 years was all done within the next five years, and the dozen or so papers I might have had with my students on lanthanide luminescence only turned into five. The most significant research group that picked up on our work was that of Professors Roland Frei and Nel Velthorst at the Free University in Amsterdam. At that time, they were the premiere laboratory in the world doing post-column luminescence detection in liquid chromatography, and there was no way I could compete against them. So I wrote to them, describing my situation at Bates, pointing out that I would never be a threat to their work, and asking if we could at least talk to each other so that I might retain some distinct niche in the field. They wrote back and invited me to visit their lab, and so my family and I spent three weeks one summer at the Free University. Eventually, four Dutch undergraduate students visiting my lab to conduct a summer of research, and two Bates students spent a summer doing research in the Netherlands. They also invited me back as a distinguished guest to serve as the outside examiner on one of their Ph.D. student's thesis defense

(although, in the system over there, I was referred to as the "opponent").

I've been especially fortunate over the past dozen or so years to have support for my research on chiral NMR shift reagents through the NSF-RUI program. I believe that my recent success with NSF-RUI grants has caused some people to think that it's been easy for me to get funding, and that essentially all my grants have been funded. Now it turns out that after getting my initial Research Corporation grant and a subsequent renewal for my work on lanthanide luminescence, and a grant from PRF that I renewed to pursue another area of research on the development of selective pre-column adsorbents for use in gas chromatography, I became convinced that what I really needed was a big grant from someone like NSF. After all, I had seen the process of writing an NSF grant in graduate school and had seen the types of budget categories that could be requested through NSF that could never be supported through the smaller grants from Research Corporation and PRF. I became determined that I had to have one of these. So in my first six years at Bates I submitted four grants to NSF to support ideas I had for work on lanthanide NMR shift reagents. All four were rejected. The ratings were always close to what might be needed to secure funding, but not quite there. I guess I didn't learn from the first rejection, and with dogged determination kept slamming my head into a brick wall.

After letting my Research Corporation and PRF grants lapse to take a year-long sabbatical leave at Duke University, I was in need of funding again and decided to try the NIH-AREA program for work I still wanted to do on lanthanide luminescence detection. I chose NIH since many of the compounds that were potential candidates for detection were of biological significance. Now, at that time, I had no familiarity with NIH and how their rating system worked. In those days, and I suspect they still do it this way, you got your proposal rating before you learned if it was funded. So one day it showed up in my mailbox at Duke, and on a scale of 100 to 500, with 100 being the best possible score, I got a score of 250. Now I thought, 250 is closer to 100 than 500, so perhaps I'd get funded. That was until a few days later when I spoke with one of the faculty members at Duke who informed me that on the regular grant programs at NIH, you had no hope unless your rating was well under 150. But this was the AREA program, so maybe it was different. But a few weeks later I learned that my proposal

had been rejected. But that didn't bother me too much since I knew that the NIH proposal was a long shot, and I had another proposal pending with PRF to continue my work on selective sorbents. And sure enough, the very day after I got my rejection from NIH I received my letter from PRF informing me that my proposal on selective sorbents had been rejected. What? This was impossible!

Reading the reviewer comments on my NIH proposal made me realize that there were ways that I could clearly improve it, so I rewrote and resubmitted the proposal to NIH. And I did do better - I got a score of 230. Let's see, if I could keep improving 20 points per try, another five years and I might get funding.

My track record on big research grants was now 0 for 6. If nothing else, I was consistent.

I also wrote and resubmitted my PRF grant based on the reviewers comments and a useful discussion with the program officer (I was finally learning of the value of talking with program officers, and finally overcoming my reluctance as a nobody from nowheresville to call them and ask questions). And my PRF resubmission was rejected too.

At this point, I had a revelation. It turns out in Massachusetts there is a coastal town named Marblehead. And of course, Marblehead faces east and looks out over the Atlantic Ocean. And as they say in the morning, and I am prone to say when I get these revelations:

"Dawn breaks over Marblehead"

What finally hit me was that both these proposals basically contained crappy ideas. I and other labs had basically exhausted the important work on lanthanide luminescence detection in liquid chromatography to the point that NIH was not going to fund what I wanted to do. And the work on selective sorbents had reached a dead end as well. Sure, I could have continued to turn the crank and get out some publications, but the primary ideas in the proposals were no longer considered significant enough to warrant funding.

Fortunately, though, I still had this interest in NMR shift reagents, even though I had never gotten an external grant to support this work. What I had been able to do was stay

active in this area through the use of modest funding programs at Bates. During my sabbatical leave at Duke, while reviewing a proposal for Research Corporation having to do with NMR investigations of the host-guest chemistry of compounds known as cyclodextrins, I began to wonder if it would be possible to couple lanthanide ions to cyclodextrins and use them to better distinguish the NMR spectra of compounds that are known as enantiomers. The more I thought about this idea the more I liked it. So I read the literature, talked with organic chemists at Duke who knew a lot more about synthesis than I did, and became convinced that it was both possible and interesting. So I submitted a proposal to Research Corporation and it was funded. And as I talked about this work with other colleagues, they all said things like "hey, that's a good idea." So what the heck, I submitted an NSF proposal on the more general use of coupling lanthanide ions to chiral solvating agents, and guess what - it was funded! FINALLY, an NSF research grant. And better yet, I've been able to renew the support through three cycles now, and I've received two grants to purchase high-field NMR spectrometers.

So, some take home notes from my own personal experience over the past 22 years of proposal writing.

1. Send in proposals. It takes money to do research. There are places out there that give you money for the asking, provided you have a good idea and provided you do a good job describing it. More money allows you to do the work better. Sure I'd love a Hyundai, but I'd much rather be driving a Mercedes. External grants allow for more of what I call "Mercedes moments" in the lab. There is just no excuse for faculty members who are interested in doing research not to send in proposals to seek support for doing the work. Too many faculty members at undergraduate institutions convince themselves that it's hopeless and they will never get funding. I believe my record is a testament to the value of never giving up. Too many faculty members also convince themselves that they don't have time to write the proposal and then administer the grant. I say that if it's important enough, that you'll find the time. Don't make excuses. Write the proposals.

2. When you don't get funded, assume it's your fault. Sure, I know, all those reviewers are a bunch of jerks who are out to get you, who don't understand your work, and who don't appreciate the value of research done at undergraduate institutions. But you know what, that's not the reason why you didn't get the proposal funded. You didn't get it

funded either because your ideas weren't good enough, or you didn't do a good enough job explaining your ideas and making the case that they were important enough to fund, and that you knew how to make the project successful. As I speak to you right now I have a renewal request pending for my NSF grant. I am sitting on eggshells because I do not know whether it will be funded. No matter what successes I have had in the past, there is no such thing as a sure thing when it comes to submitting proposals. I know this, in part, because I have also been so unsuccessful in the past. I really don't believe the reviewers can find fault with my track record. If my NSF grant is not funded, it means that my ideas were not deemed important enough, or I did not do a good enough job convincing the reviewers that they were important enough. I also have a proposal pending with the distinguished teaching-scholar program of the NSF, a submission that's probably as likely to succeed as my trying to light a candle outside in a hurricane. But I truly think I stand some chance of receiving one of these, as I've always thought with every proposal I've ever submitted, and of course you can't get a grant unless you submit a proposal.

3. Read the reviews carefully, and if in doubt, have one or more colleagues read the reviews carefully, then try to figure out whether the reviewers think that you have a good idea that needs repackaging, or a bad idea that will never merit funding. If it's a good idea that needs repackaging, repackage and resubmit. It's important to find honest colleagues who will level with you.

4. Listen to honest colleagues. Last summer a younger faculty member who I hardly knew, but who I was in contact with through a professional activity, asked me if I would read over a draft of a proposal and provide feedback on it. I said I would. I read it over and found it lacking in some substantive ways. So I sent this individual two full pages of comments (single-spaced, 12 point font) on the draft. This fall PRF sent me the proposal to review. Now on PRF proposals you can list possible reviewers. I wasn't listed, but PRF knows of me (of course they do, they've rejected enough of my proposals to know of me). I debated whether I should review it having provided comments on an earlier draft. And then I decided, who better to review it? I could see how well my criticisms were addressed, and be in a position to better advocate support of the proposal. But guess what, the PI had not addressed any of my substantive criticisms. There were a few superficial changes from the draft I had seen, but it was virtually identical to what I had seen in the summer. I suspect that the PI will recognize my review - since I already had it written. I also suspect that the grant was not funded. If you ask for

advice from colleagues who you think can provide good advice, you ought to listen to it.

5. Finally, never give up looking for good ideas. A good idea is the most important criteria for getting a proposal funded, but unfortunately, it's often the hardest thing to come up with. We can never spend enough time looking for good ideas. In fact, I think that the greatest challenge that those of us at undergraduate institutions face, and something that CUR needs to devote a lot more time and energy to in the future, is how we foster ways for faculty members at undergraduate institutions to keep abreast of the rapid changes within our disciplines, such that we can continue to generate new ideas that funding agencies want to fund.

I appreciate the chance to talk with you today and your patience in hearing me out, and hope that you find the rest of this conference worthwhile.