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INSIDE

Bodil Schmidt-Nielsen Award Lecture

Mentoring: A Fun Collaborative Activity

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Introduction

I want to thank Jane F. Reckelhoff and the APS Women in Physiology Committee for recognizing the laboratory with the Bodil Schmidt-Nielsen Mentor Award. My gratitude, of course, extends to those who did the work, the world-wide members of Cimcon, and especially to Tara Haas, who spearheaded the nomination. This award means so much to me simply because it's heart-warming to be recognized by those who know you best, that is, by people who have passed through the lab and who feel that we did well together. The award also completes a circle in a way. Bodil Schmidt-Nielsen and I had a connection through her father and mother, August and Marie Krogh. I had long admired August Krogh's work and had studied his papers closely over the years and even visited his home and laboratory in Copenhagen. It was my custom in the lab to use scientists as role models or as "heroes/heroines" for discussion with people in the lab. I think it's vital that students feel that they are part of an ongoing process, resulting from the combined activity of many scientists who have gone before them; it helps the students to see that they are not just tinkering with the latest fad in the work that they do in the lab. John Pappenheimer and Arthur Guyton occupied this hero's place, as well as August Krogh, whose thoughts and ideas were a pivotal part of what went on my lab for many years. I even had a name plaque made for Professor Krogh and put on the door to the lab. Bodil came through Charlottesville for a seminar and was touched by the plaque enough to refer to it in her book, and we talked a long time about her father and mother and life in Copenhagen.

Brian R. Duling

I must also place my children in a prominent place in the list of people who must be recognized and thanked. Unfortunately for them, I suppose, they were the subjects of my first learning laboratory in mentoring. I worked pretty hard at it and I must admit that I inadvertently made them work harder at being mentored than I think was really necessary, but I had to start on

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someone didn't I? My wife, Marilyn, of course, was always part of the lab and she gave me the freedom and confidence to do the things that we did in the lab and mothered many of the trainees. In preparing this talk, I asked my wife and three girls how I did as a mentor-parent. I won't give you a full report but I guess you could say that I got a good solid B or maybe a B+. What I can tell you is that their trials and tribulations taught me much more than I taught them. I learned and re-learned the hard lesson that in teaching, as in so many parts of being human, "things take time," and I learned that people need both space and time to learn. Moreover, I learned that people learn at their own rate, no matter what I plan; as Bob Gore, one of my closest confidents taught me: "When the student is ready, the teacher will come." As I learned from my trainees, when the teacher comes too early, frustration often arrives on the same train.

I must say that I never really thought of myself as a mentor in the sense that it is so often used today. Rather, I thought of myself as a lab leader and, as the title of the talk indicates, as someone who had the great pleasure of working with a new set of ever-younger collaborators each year, the students and post-docs. There are now many, many books and papers on how to mentor and they are quite detailed in prescribing and proscribing the research environment, and how a mentor might interact with a mentee. The mentoring tomes that came across my desk often seem to me to be too focused on a set of rules for running a profit making organization, rather that operation of a place where a bunch of friends meet to play and study important things. My feeling in bringing in a new person to the lab was that I was looking for someone who was interested in what we do, and who wanted to come and work together on a problem for a while in the spirit of a comment made by Newton. "I do not know what I may appear to the world; but to myself I seem to have been only like a boy playing on the sea-shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me" (Isaac Newton, 1642–1727). Thus, I don't have a formal presentation on how to mentor. Rather, what I'd like to do is reminisce a bit with the members of Cimcon, and share some things that I think might be worth thinking about as one starts and runs a lab.

Finding a Project

Because joining the lab was pretty much a joint decision, the general direction for a project was usually decided before the fact. I recognized that the new trainee had an incredible amount to learn, had to do so in a short time, and that the good science had to follow the hard work of simply learning how to navigate in a new environment using a new language. I don't see science in the linear way that many do, in fact it seems to me that Werner von Braun got it right when he said, "Basic science is the science I'm doing when I don't know what I'm doing." I believe this, and I get a bit impatient with plans for research that involve developing a highly detailed set of research goals and objectives in advance. I suspect that all too often, if you know enough to write the ultimate set of goals and objectives, the problem might be pretty trivial. The mathematicians have a phrase for such things—after thinking—and I feel that the granting agencies are forcing us in a direction that

will cause more and more time to be wasted in this kind of activity.

So, the discussion of a research project for us is more open-ended and typically begins with a discussion of which general area of research the trainee might be interested in, and then we identify a small, starter project, often part of some bigger project or a follow-on from the work of a previous trainee. If all goes well, the small project is chosen, so that it demands that the necessary skills be learned, and at the same time it will allow room for growth. The student has to be given time to mature and to find their own way into "their" project, and perhaps even to a career if all goes well.

The thought process in approaching a project deserves a bit of discussion. Sometimes we get locked into the idea that a project should be a problem that demands a solution, and the solution is the aim of the project. However, it is equally important to recognize that the problem is the trigger or goad that leads to deep thinking, and ultimately, if we are serious, the thought leads us to a new perspective on the origins of the "problem." Occasionally, if we are lucky, we get drawn into some really new science. I have often heard students warned not to go off on a tangent, but at the same time it has to be remembered that, in the land of good science, tangents are sometimes where the gold lies.

My view of a student or postdoc research project is also a bit different than is often the case, because I have always made a great effort to find independent projects for everyone. This has an enormously positive effect on things, because it prevents squabbling, and because it obviates issues with authorship and, most importantly, it fosters independence. In today's multidisciplinary research environment it is often impossible to find really independent projects, but the basic idea is not so much the project as the fact that the trainee learns early on to assume ultimate responsibility for the project. That means, no finger pointing and no excuses: <u>your</u> project is moving or it isn't.

If the project is a collaborative one, we have to make sure that everyone in the lab understands that in a collaboration, it is not adequate to just shrug off a methodological question as someone else's responsibility. The trainee has to learn that, "die Metode ist alles," and he or she, as the director of the project, must assume responsibility for the whole thing. I recognize that in much current biological science, several experts combine to carry out a project and this can easily lead to conflict and misunderstanding. Though it pains me to think about it, it may be vital in a world of complex, joint projects that authorship, timeline, and responsibilities are laid out with the trainee and collaborators, in advance, in writing!

Lab Operations and Rules

So, having a starter project, one of the tasks before the student is to simply learn how the lab operates. Of course, this means immediate immersion in the relevant literature, and such mundane things as: where the reagents are stored, how to talk to the lab manager, and in my old fashioned view, he or she has to learn how any machine being used actually works, and how to test, and calibrate it. How a pH meter works, how one sets up a microscope for Koeller illumination, and how the lab water system operates; all determine the outcome of even the best experimental designs, and these "trivial details" are too often ignored. I have the feeling that details of lab operation can too easily be minimized, especially as we get more and more kits, and the company tech reps spend more and more time "setting up and calibrating the equipment." The result is that the ability to trouble-shoot is diminishing, and it can become more difficult for a new trainee to recognize that the data look that way because of what the machine is doing wrong, not what the biology is saying.

One vital part of the introduction to the lab is the notebook. I have found recently as a result of service on the University Ethics Committee that many trainees are not being taught to use and depend on a lab notebook, and virtually none is instructed in how to acquire, store, and present data from the multiple sources that commonly contribute to today's projects. We all need lab notebooks and it is the mentor's job to check the notebooks occasionally and to look at the original data, not the distilled material presented in a PowerPoint show in lab meeting.

I tend to run a pretty relaxed operation in terms of individual behavior, but I think it's important to identify some absolute limits that are just not transgressed. The idea has to be constantly reinforced that certain rules are <u>rules</u>, and they allow no room for the trainee marching to the "beat of a different drummer," for if the rules aren't followed the best case is that the experiment may fail, and the worst case is that a complicated, joint effort fails. The shared equipment has to be taken care of. The space of each lab member has to be respected. Computers have to be backed up. Messes have to be cleaned up, and, no loud and obnoxious music in the lab. Here, I'd like to say something about work patterns and hours but that is so controversial these days that I'll settle for a quote from Gustave Flaubert that I recently ran across: "Be regular and orderly in your life so that you may be violent and original in your work." I'll leave the definition of regular and orderly to the reader.

It helps enormously to have a strong daily presence in the lab to work with the trainees on these "mundane" issues, someone who commands respect and who knows both the what and the why of the lab. In my very fortunate case, David Damon trained just exactly as many students and post-docs as I did, and later Kathleen Day who not only did the training but brought a whole new batch of skills to the lab.

Lab Meetings

Perhaps as important as anything I can think of with regard to mentoring is to hold regular lab meetings, and to elevate them to a central part of the lab activities. These contribute to both the scientific and social life of the lab. The

lab meetings must be regular, and the PI must always be there, as a demonstration to the lab members that the meetings are important and that they take precedence over all the other demands on our time. Not only is everyone expected to attend, but also material has to be prepared in advance and presented in a formal way. Failure to follow this rule should lead to embarrassment at the least. The technicians and lab managers should be an integral part of the meetings, because this is the place where bad practices in the lab are uncovered, and where the trainees learn the value of the technical staff.

The lab meetings are where the mentee will learn to give and take in the world of science, and every meeting is a time to hone presentation skills and to sharpen thinking. The form of the meetings must change from time to time or they will become routine and lose their edge. My own lab meetings consist of lab progress reports, reading papers in the literature, reviewing manuscripts and grants, and preparation for scientific meetings. I was surprised recently to find that I have copies of material handed out at the lab meetings that go back to 1982! It's worth noting that when we went to PowerPoint presentations I failed to make a similar system for storing the presentations, and I urge all of you to figure out a way to systematically collect and file lab meetings in a digital era as they are great fun to look back over.

"I learned and re-learned the hard lesson that in teaching, as in so many parts of being human, 'things take time,' and I learned that people need both space and time to learn."

The presentations in lab meetings have to be critiqued and critiqued hard. This is where the respect for preparation, truth, and the ability to think on one's feet are learned. Both mentor and mentee must be prepared to challenge and be challenged in lab meetings, and the lab meetings should even be a bit confrontational at times. I know that is not the style currently but confrontation is one part of scientific life, and being prepared for it at home is much easier than learning in public. I once asked a trainee if she was fearful of presenting at Experimental Biology and she looked a bit surprised and asked "Why would I be? If I can get through one of our lab meetings, there is nothing to fear from EB."

At the lab meetings I try to minimize my own thoughts, except where things seem to be especially beautiful, or going badly astray. I try to spend less time in answering questions, and more in framing the problem and defining directions. A direct answer to a specific question

Duling Receives Schmidt-Nielsen Distinguished Mentor and Scientist Award

The APS Women in Physiology Committee hosted a reception at Experimental Biology 2009 to honor Brian R. Duling, Robert M. Berne Professor of Cardiovascular Research Center, Departments of Molecular Physiology and Biological Physics and Biomedical Engineering, University of Virginia Health Sciences Center, who was selected as the sixth recipient of the Bodil M. Schmidt-Nielsen Distinguished Mentor and Scientist Award.

More than 100 trainees, EB awardees, and colleagues gathered to celebrate the award and hear Duling s award lecture entitled, Mentoring: A Fun, Collaborative Activity. The talk is published in this issue of *The Physiologist* and posted on the APS Mentoring web site (http://www.the-aps.org/career). Tara Haas (York University), who coordinated the nomination of Duling for the award, was present to introduce him. The award was presented to Duling by Jane F. Reckelhoff, Chair of the Women in Physiology Committee; and Irving H. Zucker, President of the APS.

Duling received his PhD at the University of Iowa. He did his postdoctoral training at the University of Virginia School of Medicine before being hired as an Instructor and then Assistant Professor in the Department of Physiology. He moved up through the ranks, including terms as Vice-Chair and Acting Chair of the Department, until being named the Robert M. Berne Chair in Cardiovascular Research in 1992 and then Director of the Cardiovascular Research Center in 1993. He continues to hold those positions today, in addition to acting as Interim Associate Dean for Graduate Studies in the School of Medicine.

Duling has successfully mentored 35 postdoctoral fellows, three clinical fellows, and seven predoctoral students. His mentees have gone on to successful and prominent positions (including two chairs, Senior Vice President, Chief Medical Officer, and NIH Program Officer, among others) in a variety of careers: academia, industry, clinical centers, and government with national funding and numerous awards among themselves.

Duling's success as a mentor was fourfold, according to the people writing his supporting recommendation letters. *1*) He provided excellent guidance to trainees holding a range of career aspirations. He gave equal attention to trainees who desired to follow in his career footsteps and to those who aspired to an different career path. By spending the time to understand his trainees' career goals, Duling took steps to best facilitate each trainee's future success. *2*) He constantly demanded the best of his trainees. He stimulated individuals to work harder and think deeper than they thought possible. *3*) Through his enthusiasm in hosting visits from national/international scientists, Duling illustrated to his trainees that science transcends geographical boundaries and that colleagues can become lifelong friends in the journey of scientific discovery. This led to a great diversity in individual strengths of the researchers in his lab and generated a strong international "spirit" within the lab. *4*) He engendered an air of "family" with members of his lab. He maintains continued contact with previous lab members by holding an annual Duling lab dinner at the yearly EB conference.

APS members are encouraged to nominate members for the 2010 Bodil Schmidt-Nielsen Award. For more information, see the APS website (http://www.the-aps.org/awards/society/schmidt-nielsen.htm). Application deadline is September 15, 2009. ™

makes it easy to move on but lets the trainee off the hook and has inherent in it all the mentor's biases. This refusal to answer questions is sometimes incredibly frustrating to the trainee facing a deadline but I believe it is vital to their growth, and often will draw out another lab member's thinking. I, of course, moderate the meetings to some extent, and try to get the presenter to extend their thinking and to push back when someone is pressing too hard; criticism is OK, bullying is not. The lab meetings should be a time to challenge, challenge, challenge, but only in regard to the science, never the person or the idea.

"Keep an open door for the lab members, and make everyone in the lab knows the meaning of it: if you need me I am here; you are all important, and we are working together on these projects...."

Some lessons learned at good lab meetings:

distrust your own data more than you distrust the data of others;

your data are your responsibility in every way;

what we are doing here is more than making papers; we are trying to find truth;

never get angry, or at least don't show it: keep thinking;

be ready and enthusiastic to receive the suggestions of others, even when it means a lot of work.

To paraphrase the Newton quote mentioned above, in the lab meeting you should learn that every shiny thing you pick up at the sea-shore of science is not a treasure—there will be some rotten fish as well.

The White-Board Sessions

After a lab meeting I often have a follow up in the form of a one-on-one "debriefing" at the white board, usually with the presenter, but not always. Here is where the real excitement for the mentor occurs. These might take different forms: helping to solve a technical problem that applied specifically to the trainee's project, trying to think more deeply about an unresolved equation, and yes, occasionally discussion of a job poorly done. I try never to use the time in lab meeting itself for criticism of anything but the science.

Most often the white-board discussion becomes the place where the real mind work gets done. This is the time for you to think together with your younger colleagues—it's a time when you get to know him or her and they get to know you. To me it's like dancing or even better, jazz. Two people focused on something important to both, one leading and one following, and then the leader and follower change places. A part that I especially like is that, when it works, the discussion and interaction are intense and often so consuming that when we finish, neither mentee nor mentor can even be sure whose idea the outcome is.

Two particular events that I recall might emphasize the point. I had been working for many years on a problem that just wouldn't yield a reasonable solution—the glycocalyx for those of you who followed that odyssey. After many years and many trainees, a post-doc and I got to a place where we seemed to have finally discovered an approach that could move us forward, and with a sense of great frustration and, honestly, me close to tears, I fell into my chair and told him that I didn't care what the outcome was; prove the hypothesis or disprove it, but don't make me think about it anymore. He did as asked.

Another memorable white board meeting followed a presentation that showed pretty clearly that a project that a post-doc and I had been working on for a long time had just come to a completely dead end. We spent several hours together, first allowing ourselves to abandon what we had been doing, and then thinking of the next move, and that discussion resulted in several years of some of the most exciting work I have had in the lab.

Review of Drafts of Written Work

One of the most important parts of the work that mentors do is to review and critique written work. The students and post-docs enter the process with hugely diverse levels of training in writing and analysis, and by the time the mentor gets a manuscript, the trainee will have put heart and soul into the work, and will likely both be in love with the creation, and a bit ill at ease in presenting it for scrutiny. Here is where trust must exist, as it is the responsibility of the mentor to both review the manuscrfipt, with as much scientific rigor as possible, and to edit the prose. I sometimes call the process "killing the babies," as that is almost the feeling that the trainee has. I learned from my daughters that if I give myself free reign in the process, it is discouraging, and it is just too much to endure. Only after a lot of trial and error did they come up with the idea of "just as much editing as needed for this round," i.e., give me a level 1, 2, or 3 level critique. One can begin with some thoughts on the strength of the work, and then some comments on structure and perhaps some thoughts on order of presentation. This is enough to start another draft. The two of you can then go through an evolutionary process where parts of the manuscripts are getting better as the others are getting adjusted or completely re-written.

The beauty of the process is that both the mentor and the mentee are learning all the time and it's not just the English that's getting better, it's the science, and from the process new experiments often emerge. In the middle of one of these interactions, I once came running out of the office waving a manuscript and emoting to the poor post-doc how great it was now. With typical understatement for him, the fellow said "Well, I supposed you would like it; you wrote it." Ultimately, the grinding work begins and the red pen goes to work in earnest, dealing with the fine details. Once, at the end of this phase of the process, I dropped off a manuscript with a student as I went out of town for a while, and in a few days I received a Federal Express package containing nothing but a nice, new red pen.

The process is sometimes painful for both parties but essential, and even when this part is done, the manuscript should be passed on to another reader. Hopefully, he or she won't undo most of what the wise mentor has suggested!

Open Door

Keep an open door for the lab members, and make sure everyone in the lab knows the meaning of it: if you need me I am here; you are all important, and we are working together on these projects, and probably I am just working on politics anyway (see below). The open door should swing both ways. Stop at the bench of everyone in the lab as often as

you can, even if you are not doing the bench work yourself. You will see their work habits and it will help you to understand the problems that they confront.

Politics and Sociology

The mentor's influence on the mentee's perception of this aspect of our work is critical. Trainees must be taught that politics and people skills are part of a scientist's life and they have to be warned that politics is real, and will determine parts of their future. At the same time, the trainee has to see that the science is the goal that will stand up over time. Thus, it's important to emphasize and re-emphasize that one must maintain a clear distinction between what is the science and what is the politics. As with everything, mentees will differ hugely in their ability and interest in people-things (i.e., politics). I find that many of the current generation of students are more receptive to politics than previously, though they are likely to call it networking. Some trainees will find it easy to work with others, both on science and politics; many will be frightened, and some will be put off by even the idea that politics could be critical to their science. The mentor's job is to allow each the latitude to express their feelings and, at the same time, to encourage them to find their own way. I have often shared the idea of "Quintons" with trainees, an idea that I made up to reflect the fact that there is an emotional cost to doing both science and politics and the coin of the realm we called Quintons. An important part of the thinking is that the Quinton cost per activity is different for different people. Some people find the politics painless and for some it is exquisite pain. Ultimately, your energy (i.e., Quintons) is all that you have, and it is your decision as to whether you use it for politics or science. So, recognize that some energy has to be used on both science and politics, and be sure that you put your energy where it will do you the most good.

The Rat Race

As much as you possibly can, keep the lab and the trainees out of the rat race, but help them to learn how to cope with it as their careers develop. We are all under great pressure to publish as much as we can, to get as many grants submitted as possible, to present large numbers of abstracts, and to implement the next new technique as quickly as we can find the money for it. At times speed is necessary but it easily becomes a way of life, a treadmill. Some advance thought and planning about it may alleviate some of the pressures. If you submit a grant, do it with the idea that you are expecting to get it. If you just throw them out the door at the study section, that will be reflected in the product and the funding likelihood will decrease. There is an old story about duck hunting that is relevant. It turns out that if you are looking up at a flock of ducks and just pull the trigger, you will never get a duck. On the other hand, if you take careful aim, you may get one. With your grant and paper submissions, take aim and act and feel as though you will get the grant or have the paper accepted, no matter what.

Don't try to publish little "throw away" papers just to get the finding out there or to pad the CV a bit. Do the cost accounting and I think you will find that it costs an amazing amount of money. I estimated several years ago that a four-page paper might cost $50,000 in real dollars, if you add in technical time, your hourly wage, and the cost of a trainee's time. More important is the amount of your time that will ultimately be spent with galleys, etc., and realistically, a throw-away won't get you many points with whomever is counting. Publish what you think will contribute and make it as good as you can. Save what won't contribute today until there is a place for it tomorrow.

With things like the Federal Incentive money on the horizon, a request for a new gadget is almost irresistible. A new device in the lab often offers some new advantage, but at the same time, it will require care and maintenance. What exactly are you expecting to get from the device, and do you really need the data that might come from obtaining it?

On a related side note, there is the tendency to have large numbers of projects going at once for "safety." This is especially common, and especially risky for a trainee. Some of this is probably necessary but do not forget that a good project is a demanding one. The old adage, "Problems worthy of attack, prove their worth by fighting back," (Piet Hein) applies here. I still think that it's worthwhile to start with a small problem and let it seek its own direction to a big problem, and hopefully to a lifelong pursuit.

Think hard about your collaborations. Do you really need what the collaboration offers? Will both your research project and that of the collaborator gain in the association? Every interaction takes time and Quintons from both you and your collaborator. Any serious collaboration involves hours of discussion and the possible project outcome should be worth the time invested. Collaboration for the sake of collaboration is rarely worth it.

Do not get seduced into too many committees. With these being a great ego boost and also a real part of an academic CV, it is all too easy to say yes when invited. I think a good rule is to first see if you have "enough committees" on your CV, and then ask whether the service on this or that committee is likely to take advantage of your skills and give you information you need for your career?

APS President Irving Zucker presents Brian Duling with the Bodil Schmidt-Nielsen Distinguished Mentor Award, along with Tara Haas, who nominated Brian and was one of his mentees, and Jane Reckelhoff, chair of the APS Women's Committee.

I just want to touch briefly on the dreaded subject of Career. I needn't spend much time here as all the mentoring books go on and on with all the fine details of building a career. I think it's worth adding a bit of idealism to all the current discussion, however. A researcher in the biomedical sciences, particularly Physiology has a golden opportunity, especially now. If you find your way to a good teaching or research position, you have available equipment and ideas that were undreamed of even 20 years ago. We have an enormous database of information collected during the current discovery phase of research that is just looking for someone with the training to assemble the pieces into a coherent whole. The people who will do this will fundamentally change biology and medicine and they are the young physiologists in training today. If you think you want to stay in it for the challenge and the science, you can now do so in academics, government, or industry and have someone pay you for finding those pebbles and shells and getting all the joy that goes along with it.

Respect

For the mentoring process to work well it is vital that both mentor and mentee respect one another. Lack of respect gets translated into distrust, and then anger, and the problem goes to counseling, not mentoring. It took me a while to understand that respect implies the recognition of differences. Where the science is concerned there is no latitude; it has to be as good as possible. However, that doesn't mean that everyone gets to good science by the same path. A part of good mentoring is to be able to recognize that the trainees will attack problems differently. One has to be sensitive to the fact that people have different ways of working and allow them to express their individuality. Some will have to be alone and quiet, some have to sing or whistle while they work, some are slobs and some are neatnicks. Some of the trainees will want you to provide constant guidance and approval. Some will be in the office hourly to tell you what is going on, and some will have to have the knowledge that they have gained dragged out of them. Some need very badly to confront everyone, including you, and want to argue and fight and some are crushed by confrontation.

The job of the mentor is to try to tell the differences, and where possible, allow the trainee full expression, so the trainee can find his or her own way of working once they leave the lab. If you insist that the trainee "follow in your foot-steps," when you are not there it may be very hard to keep going. By the way, that includes career path. Many good trainees will wind up doing much different things than you do and be in academics, industry, and administration and if you and they are lucky and have worked hard at the training process that training will work in many venues.

Race, and gender differences are a subject of great discussion in mentoring manuals, and are, to some extent, inescapable. We have to all recognize that they are there and be ready to deal with them directly or not. These can be subtle and all of us have to learn not just the big things but the little ones as well. I tried to ask people who were leaving the lab how the experience had been and whether there were bothersome things that occurred of any kind, including race and gender. I asked one young woman such a question, and after a bit of hesitation she allowed that there had been one problem, in that I always asked my male technician to do the soldering and she was perfectly capable of doing it herself. So much for unconscious bias.

Regardless, of differences in perception, ability, or behavior, everyone in the lab gets the same respect, and that just has to be true. The mentor has to keep trying to learn to work differently with different people. Favoritism or negativism is incredibly destructive. Thus, the mentor will have to adapt to diverse styles, but everyone gets subjected to the same rigor in the inspection and analysis of the data and the presentation of the science. Ultimately, differing abilities or characteristics just don't count; it's the science that counts and everyone has to learn their own path.

Thoughts of the Past and for the Future

So let me close this with two things. First, I'd like to make an apology to my lab mates for the things that I know I didn't do well enough and second summarize some thoughts for the future.

I'm sorry that I:

too often emphasized criticism and didn't give enough encouragement.

sometimes didn't sell all the post-docs and graduate students hard enough to the outside world.

failed to heed the old adage "Listen or your tongue will make you deaf."

got angry way too often.

I am, however, joyous that we did some things together pretty well. I thank you, and I encourage you to continue to:

pick a big problem;

follow the science where it leads you;

get grants only to do the science that excites you;

work on the hard parts of a problem (but don't tell the study section until they are mostly done);

develop new methods necessary to answer a question, not because they are cool. Cool is good however;

publish only as often as your data warrant;

when you get to the end of a problem let it go;

always tell the truth, especially to yourself;

give your imagination and emotions full range. Judge and respond after;

be more skeptical of your own data than the other person's;

remember that everything we do is not good or successful. Do not be afraid to be wrong;

have as much fun as possible;

Climb trees whenever you can, and sing or whistle a lot.

Finally remember:

"What lies behind us, and what lies before us, are small matters compared to what lies within us" (Emerson). ™