A Message to Beginning Graduate Students . . .

At the beginning of every academic year, the head of the department of chemical engineering at Penn State traditionally addresses the new graduate students. While the following paper was originally directed to those students, it has general applicability and is meant to assist all students in making some important decisions about their graduate work.

GRADUATE STUDIES

The Middle Way

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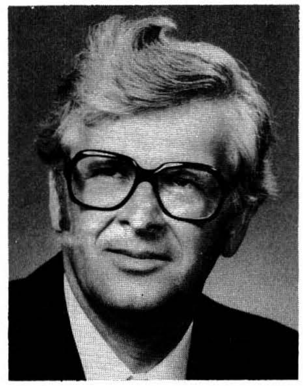

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THE TITLE AND format of this year's seminar were based on one of the central concepts of Buddhism. In my naive interpretation of Buddhism, the Buddha experienced two extreme lifestyles. In his early manhood he emphasized pleasure in the worldly aspects of life. Although he had every material thing that was supposed to ensure happiness, he did not find fulfillment. Consequently, he rejected material things and assumed an austere lifestyle—but this did not seem to fulfill him either. Finally, Buddha concluded that neither of these extremes brought happiness—that it could only be found in the middle way. He did not feel that a person had to abandon worldly pleasures, but on the other hand, he should also not be dominated by them.

In thinking about approaches to different aspects of graduate studies, I concluded that in many cases the middle way is also the most appropriate path between two extremes. I have been able to identify eight different aspects of graduate work where I feel the appropriate approach lies between two extremes. These may not be the only ones, but they do incorporate many important aspects of graduate work.

1. CHOICE OF PROJECT

Right now, you are in the process of choosing a research topic for your MS or PhD program. I have often observed that there are two extreme approaches to this process. Some graduate students have their mind fixed on a specific area of research and will not consider any project outside of that narrow area. On the other hand, some students become interested in a specific research advisor and feel that it is imperative that they work with him. To the first group, I want to make it clear that your choice of a research topic in graduate school *does not* predetermine the path you will take after you have finished your degree. It would be impossible for any of you to determine the areas of my MS or PhD research from my research record and the work that I am now conducting. The purpose of research in graduate school is to learn *how* to conduct research, and you should choose a combination of advisor and topic which you feel is the optimum for reaching that goal. Learning how to conduct research in one area can easily be transferred to conducting research in an unrelated area. Similarly, a faculty member with an outstanding research record may not be the most appropriate individual for you as an advisor. It is very important that you find an individual with whom you can communicate and whose personality and mode of operation are compatible with yours.

2. AREA OF EMPHASIS

Up to this point, your academic career has been straightforward. You took courses which fit into some pre-prescribed course of study. Now you will encounter demands coming from two distinct areas—from course work and from research. The tendency of most new graduate students is to emphasize their course work and neglect their research. This is a natural extreme since you are familiar and comfortable with courses and have a proven record of success. But it is not clear that your proven capabilities in structured courses can be transferred to an area of research. It is extremely unusual for a graduate student to not fulfill the requirements of a degree because of poor performance in his courses. On the other hand, do not over-react and spend all of your time in your research area. Again, the middle way is the obvious appropriate procedure. You must find a way to budget your time and effort in order to maintain progress in both areas.

3. PLANNING OF RESEARCH

In planning a research program, there are two extremes which can dominate. At one extreme are the individuals who are overly preoccupied with choosing a problem which can be solved. The result is often an overly conservative research plan which, even if 100% successful, represents only a perturbation on previous work. To these students, I point out that it is important for research to uncover new knowledge or to answer important questions. At the other extreme we have a few individuals who are concerned only with the impact that their research can have, but who do not adequately consider the probability of attaining their goal. It is desirable to think big and to have confidence, but a cure for cancer is hardly an appropriate goal for a novice researcher. One should work on problems that matter, and there should be a group of investigators who care about the results of your research. Never conduct an experiment or work on a theory if the results will not challenge the current way of thinking about your subject. On the other hand, you have to build on the existing and proven work. The truth is that quantum jumps in the advancement of knowledge are really quite rare.

4. INITIATING RESEARCH

You have all been taught that the first step in initiating a research program is to search the literature in order to become familiar with past work in the area. There is no question—that *is* how one should start. However, I have had graduate students who search and search and read and read and still never get to the point where they feel comfortable enough to do something. Nothing will ever be done if you wait until all possible objections are removed before taking the first step. On the other hand, I occasionally find an individual who will start mixing things together or start writing a computer program for the first idea that enters his head. This kind of graduate student is rather rare, however. Our educational process has a tendency to suppress creativity and forces us to parrot back what we have been taught. One of the most important aspects of conducting research is that you must avoid sequential thinking and work habits. You should start by looking at the past work in the literature, but do not expect to follow a well-defined sequential path. The initial literature will lead to laboratory or theoretical work whose results will lead you back into the literature, and so forth and so forth. You must keep many balls in the air at the same time. Your literature search will be updated as your problem becomes more defined. At the same time, you will be planning experiments and ordering equipment because of time lags involved in developing an experimental program, and you will be working simultaneously on theoretical models which will be modified as experimental results are produced. The inability to lay out a well-defined path for a research project is perhaps the most frustrating and maturing experience that graduate students face in the early stages of their career.

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5-7. APPROACHES TO RESEARCH

My next three examples are all concerned with the approaches to research problems in general. The first case is the conflict between dependence on intution versus dependence on theory or computations based on models. At one extreme are the individuals who proceed on their intution. If something feels right and makes physical sense, they move ahead. It is unusual to find a young, inexperienced researcher following this approach. Most new graduate students fall into the other category—they are dominated by the quantitative results of models. In an interview in *Indus-*

trial and Engineering Chemistry, Dr. Eric Bloch, who recently made a move from management in IBM to direct the National Science Foundation, indicated that engineers who were educated in his generation probably had a better intuitive feel for problems than the group of people coming out of school today. He also feels that the popularity of the computer has contributed to this shift, and I agree with him. Computers can raise a barrier to intuition. Graduate students will even trust a very weak model if the computer is giving them results. Models and computations based on models will become more and more dominant as the cost of computing decreases. However, you must always question the results of a computation and remember that all models are approximations to reality. Never go on to the next step unless the results make physical sense to you. In some cases your intuition will be in error, but do not ignore that intuition until you have thoroughly analyzed the quantitative results and have been able to rectify the apparent discontinuity between it and the model.

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The second area is concerned with the different approaches of wild, unconstrained creativity versus the prolonged process of following a long, hard routine. I have seen some individuals who feel that their creativity is constrained if they have to concentrate on understanding the previous accomplishments in a given area. Remember, only lunatics can be completely original! You will always be building on past work, or at least creating from analogies with other areas. It is unfortunate, but true, that a scientist's norm is about 1% inspiration and 99% perspiration. On the other hand, do not suppress your creativity so much that you are afraid to deviate from the well-trodden path.

My third concern is the apparent dichotomy between an experimental approach versus a theoretical approach. Hinselwood is credited with saying that, "Fluid dynamicists were divided into hydraulic engineers who observed what could not be explained and mathematicians who explained things that could not be observed." I am very disturbed by individuals who say they are either experimentalists or theoreticians. Neither can stand without the other, and even when your emphasis is in one area, you must be cognizant of the developments being made in the other. In one of his early essays, Einstein noted that, "Knowledge cannot spring from experience alone, but only from the comparison of the inventions of the intellect with observed facts." The inventions of the intellect are models and theories, and progress can only be made when these are compared with the observed experimental facts. Even if your natural inclination is to be drawn into one of these extremes, you must force yourself to be at least knowledgeable (if not a contributor) in the other area.

8. CONDUCTING EXPERIMENTAL WORK

The last area deals with the approach to designing and conducting an experimental program. At one extreme, we have individuals who are dominated by the design aspects of the work. I have actually seen researchers create a rigid experimental plan from which they never deviate. An extreme case would be to design experiments on the grid pattern where data would be taken at certain pre-specified temperatures, concentrations, etc. You should have an overall plan, but you should be flexible enough to change that plan as results are produced. When the initial plan was developed, your knowledge was limited—you should continually optimize the path of your research based on the very latest results. Some students conduct experiments for months without analyzing any of the results to see if they are even on the right path. The other extreme is not very typical, but I have seen a few individuals who are so anxious to see results that they jump into the experimental work with essentially no pre-planning or overall plan. They have infinite flexibility and each new result can dictate a change in the path of the work. At one extreme, we have a nice grid pattern with every grid filled in and no deviation, even though trends clearly have been established; at the other extreme, we have a zig-zag string of experiments where the direction of the next experiment is dictated by the most recent results. Neither approach is perfect. You should follow the middle way and have a well-defined overall plan and goals, but at the same time you must have the flexibility to modify the plan.

CONCLUDING REMARKS

From my presentation, you might conclude that there is a middle way in every aspect of graduate work that is the most appropriate approach. Although I have attempted to illustrate that this is certainly true in many instances, there *is* one very important exception. Some students say to themselves, "This is not the best that I can do, but it's good enough." Well, it's *not* good enough. Push yourself—take the time and make the effort to perform at the very highest level of which you are capable. There is no middle way when it comes to the pursuit of excellence. □