The 1991 Noranda Award Lecture: Reflections on Career Choices

Ajit J. Thakkar

June 1991

This precis of my 1991 Noranda award lecture is a survey of career choices I have faced and will face again. I consider choices of material to study, research topics, and teaching methods, and ways to discharge our social responsibilities.

1 Preparation for a career in theoretical chemistry

I have no suggestions regarding the decision to pursue a career in theoretical chemistry because I believe that such choices are rarely, if ever, made by a conscious rational process. Instead, chance events push us to choose one of several attractive paths. I chose chemistry over mathematics and physics largely because of Jim McCowan's outstanding freshman course and his encouragement.

If you have decided to be a theoretical chemist, then you will need skills besides chemistry. a) Study physics and mathematics as taught by engineers whose pragmatic approach is quite appropriate for the messy problems that chemists have to deal with. b) Learn a computer programming language, preferably Fortran. There is no better way to understand a complex mathematical formalism than to program it for a simple case. Moreover, it will be useful as a research technique. c) Learn to speak well and to write well. This is essential in every work environment. d) Browse through current issues of the Journal of Chemical Physics and similar journals. This will help you to get a sense of what the subject is all about even if you do not understand everything you read. e) Broaden your horizons by taking courses in the fine arts or social sciences, by traveling or by taking a job for a year. The best scientists are the ones with the widest interests.

2 Choice of research topics

Painful experience has taught me to watch out for some traps in the choice of research topics. An insidious one is calculism [1]: the belief that theoretical chemistry (or physics) should aspire to a seamless transhuman domain of mathematical discipline. I found calculism beguiling early in my career, but it is now apparent to me that calculism is a folly that often stems from intellectual insecurity. 'Sleepwalking', or doing good work in pursuit of illusory goals, is another trap that is difficult to avoid especially since fashionable research may be of this type.

Other traps include 'negative' and 'improvement' research in which the goal is to prove something wrong or to improve it, regardless of whether it is significant in the first place. Such opportunistic research can lead to quick papers but is destined to oblivion. A somewhat similar trap is 'tool-driven' research that is characterized by the search for problems to solve with a method that one has already developed. The best research is driven by important scientific issues that are tackled with all the tools available.

I can offer only a little constructive advice toward choosing a research topic. a) Choose tomorrow's hot topic today. Then you will be an expert by the time the field becomes fashionable. Unfortunately, there are no good methods for finding such topics. b) Be aware of where you are placing yourself on the 'Musician - Conductor - Business Manager' scale. At the beginning of one's career, there is little choice but to do all of one's own research. However, in the later stages of one's career, there is a real danger of getting so managerial that one loses touch with frontier research. Even a science manager cannot afford that. c) Make peace with yourself. By this I mean, at some stage we all have to stop trying to do research that is beyond our capabilities, accept our individual limitations, and make the best of our abilities. Science needs good collectors, classifiers, tidiers-up, artists, mystics and detectives as well as explorers.

3 Continuing education

Scientists constantly need to update their skills. Common sense tells us that improving our most used techniques will lead to the greatest payoff. This may imply mundane activities like learning how to use a word processor more efficiently. Browsing the literature, especially in areas outside one's own current research interests, is a splendid way to update and broaden one's knowledge. Collaboration with other scientists is another rewarding way to continue your education.

A lot can be learned from referee reports. There is a paranoid tendency in many of us to perceive a referee report on our manuscript as a personal attack. Some reports are undoubtedly a bit caustic, but they almost invariably contain useful and constructive suggestions. Reluctance to concede some of the referee's points often arises from a reluctance to reopen a subject one has mentally closed. In hindsight, I can honestly say that almost all the referee reports I received helped improve my papers.

Refereeing papers written by others is an excellent occasion to learn. The benefits are greatest when the manuscript is in an area somewhat different from your own.

4 Teaching

I once read somewhere, perhaps in one of Richard Feynman's books, that teaching and research are multiplicative, and not additive, factors in one's stature as a scientist. This means that you are a zero as a scientist if you cannot teach. Of course, it also means that you are a zero as a scientist if you do not do research. This principle should be held firmly in mind by promotion and tenure committees.

I use a few rules of thumb in my undergraduate teaching. The 80/80 rule is: structure your lecture so that 80% of it can be understood by 80% of your audience. Since science is cumulative, this comprehension rate is possible only if students study steadily throughout the course. I use frequent testing to encourage students to keep abreast of the course and to provide them with feedback.

The 80/20 rule says that 80% of the work of a scientist is done with 20% of the tools available. Hence, I emphasize the most important operational skills. The exhilaration of being able to do something provides students with confidence, and often the motivation to understand the theory taught later. Finally, remember that the best students need to be challenged by inclusion of difficult material.

5 Social responsibilities

It is not possible to avoid taking a stance on social, ethical or public issues because refusal to say or do anything is tantamount to support of the status quo. Theoretical chemists are not confronted with difficult decisions about possible military or medical implications of their research. However, they must make many other choices involving social issues.

One has to decide whether to accept funding from the military- industrial complex. Funding can be of an indirect sort. For example, an arms manufacturer who wanted to establish a factory in our community offered a research chair for our department. I felt that acceptance of the chair would be implicit approval of the manufacturer's activities. It so happened that no action was required on my part because the manufacturer chose not to locate in our community. However, if the factory had come to our community, I do not know whether I would have had the resolve to oppose vigorously the establishment of the research chair in our department.

There are other situations in which we have to resist being co-opted into systems we oppose. For example, many of us are opposed to the endless introduction of new research journals by commercial publishers. A common ploy of such publishers is to invite us to serve on the editorial board of a new journal that we think should not exist at all. Another common device to lure us to publish in marginal journals is an invitation to contribute an article to a special issue in honor of one of our colleagues or mentors.

There are inequities in the representation of various minority groups in our profession. If each of us actively encouraged students from minority groups, there would be a realistic chance of rectifying the imbalances. Chemistry has a very poor public image that is partly a consequence of major accidents involving chemicals. It was damaged further by the unprofessional manner in which the dramas of 'polywater' and 'cold fusion' unfolded. It is our responsibility to try to improve our image by speaking to high school students, to community groups, and to radio and television audiences. Many more of us should write newspaper and magazine articles on chemistry for the layperson. George Gamow is an outstanding role model. His research was of the highest calibre and he wrote the marvelous 'Mr. Tompkins' books that inspired so many of us in our youth. It is also our responsibility to educate our colleagues by writing review articles even though they do not help draw in grants.

If you have read this article, then I have fulfilled my objective to raise your consciousness about your responsibilities and the difficulties in making some choices, both academic and ethical, during a career in chemistry.

References

[1] M. Halpern. Binding Time: Six Studies in Programming Technique and Milieu. Ablex Press, 1990.