# WHERE IS CHEMISTRY GOING?

*Caltech's chemistry division has just concluded a weekly series of seminars on Chemistry and Society. The series was conceived, organized, and supervised by chemistry graduate student Bill Beranek (E&S, February 1971) "to stimulate introspection among graduate students and the faculty in chemistry." A more general goal was to increase communication between the scientific community and the rest of the world.*

*The nine seminars covered four broad areas: the effect of chemistry on society, the structure of chemistry, the responsibility of the chemist, and the future of chemistry as a discipline. The final meeting of the series—a panel discussion of the future of chemistry—appears here in condensed form.*

*Moderator of the panel discussion was C. J. Pings, Caltech professor of chemical engineering and chemical physics, executive officer for chemical engineering, vice provost of the Institute, and dean of graduate studies.*

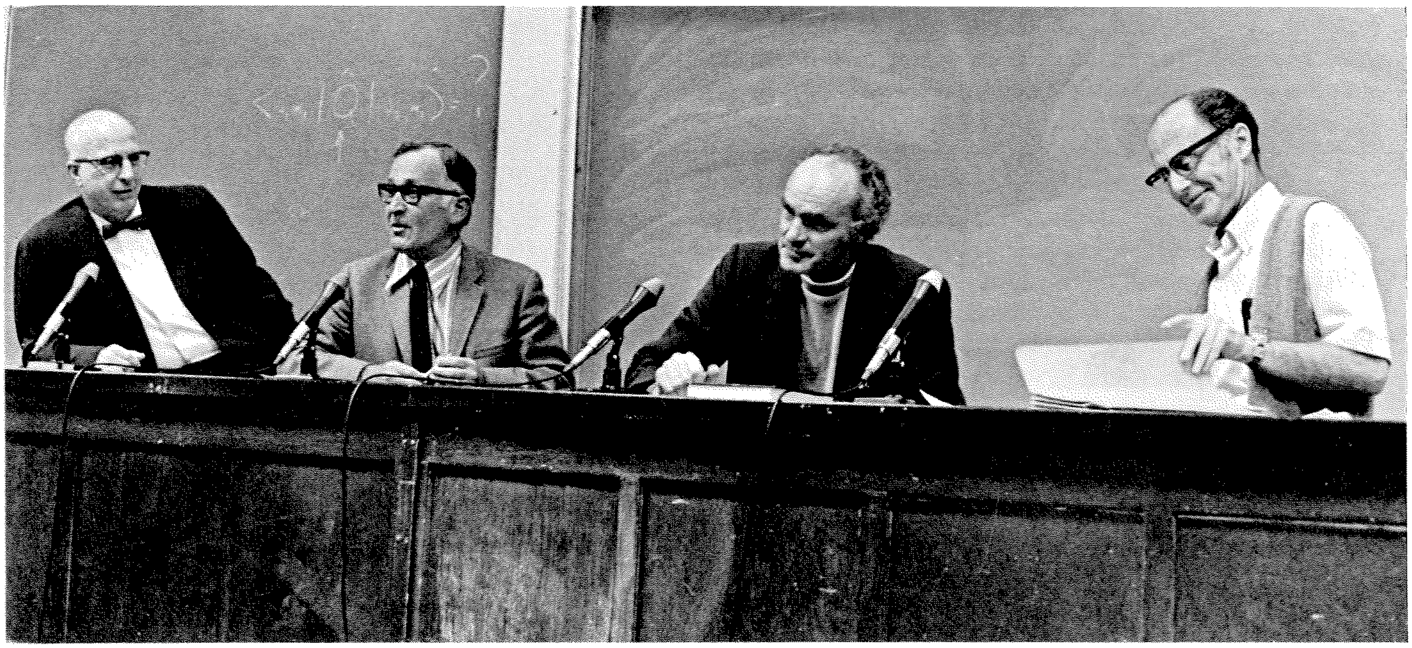
**Pings:** We are here today to discuss the future of chemistry—if any. We have a panel of eminent speakers—two local experts, and another flown in at great expense from the Midwest. All are well known in their field. All are members of the National Academy of Sciences. They will speak in the following order: George Hammond, Norman Davidson, and Harry Drickamer. Hammond is chairman of the division of chemistry and chemical engineering at the Institute and Arthur Amos Noyes Professor of Chemistry. Davidson is professor of chemistry and executive officer for chemistry. Drickamer is professor of chemical engineering and physical chemistry at the University of Illinois. He is in residence at Caltech this week as the fourth Lacey Lecturer.

**Hammond:** I'll try to speak briefly (though this is not one of my habits) about what I really think is likely to happen in chemistry in the fairly near future.

My first prediction is that in the forseeable future it's quite possible that the traditional disciplines of science will have been mixed up, redefined, and done differently so that chemistry won't exist explicitly, nor will physics. However, I don't think this is going to happen in a big hurry, and so over the short range of the next couple of decades, chemistry is likely to still stay chemistry.

Other predictions have to do with areas of activity within chemistry or chemical science. First, in research: I think the style of research done in various kinds of chemistry probably will change quite a lot within the next 10 years. I think some switch in orientation will occur; there'll be more emphasis on doing new things, answering new questions, than there will be in solving very much better some of the questions that have been classic for the past 20 years. I even think that it's likely that there will be an increased movement toward obliteration and slow redefinition of the subdisciplines within chemistry.

Second area, chemical industry: I think there will be quite a lot of change in the chemical industry. By and large, the missions of industrial chemistry have not changed a whole lot during a period when there's been great development. People keep saying, "What we need is a new nylon," and so they invent nylon over and over again. And I think that this kind of philosophy will slowly either sink into the traditional chemical industry or else we will probably see arising slowly new chemically based industries. If so, then the traditional chemical industries, like Monsanto and DuPont will, in fact, become commodity industries, and the real action—new industrial chemistry—will be the newcomers. I don't know how this will happen, but somehow I believe it will occur. And probably this new industry will place less emphasis on turnover of masses of material, a characteristic of common thinking in the industry in the recent past. Questions such as "How much of a thing can you produce?" "How many pounds of it can you sell?" will be asked less often, simply because this would be consistent with other societal trends. The society is beginning to worry about masses of stuff that we pass through our hands and process.

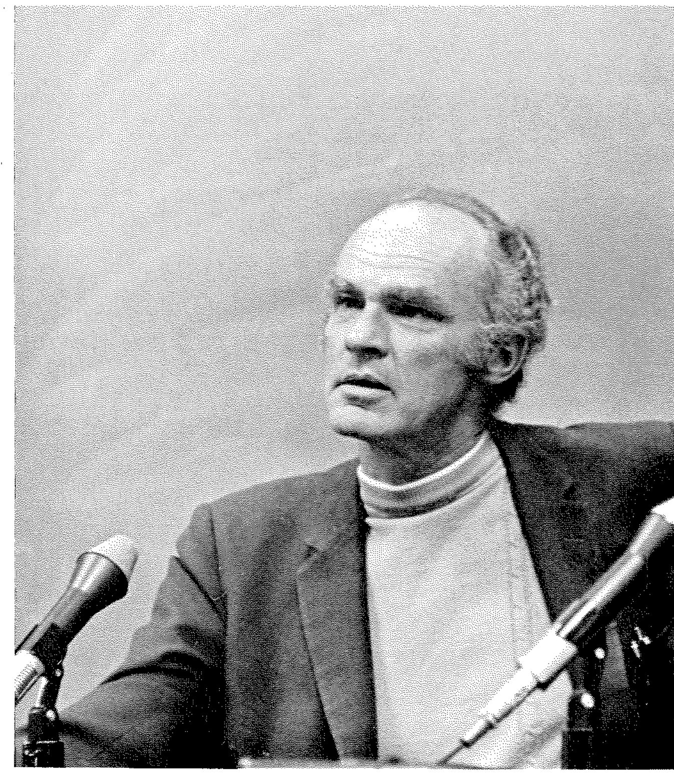


*The panel: Drickamer Pings Hammond Davidson*

Government research is a separate area on which I'd like to comment directly because I see some interesting prospects. I think that, in one form or another, chemical scientific research is likely to become relatively more important in government's own laboratories, simply because the big things—in volume of dollars and also in the numbers of people involved—in the near past, have been in militarily related research, in aerospace industry, and to some extent in development of new electronics industry to support the military and aerospace. These things are going to be decreased. I think that it's almost inevitable that the government itself is going to stay in scientific research and that, as one kind of thing is turned down, another is likely to be turned up. I don't think it's all going to go into health-related science, because that's already fairly large. So I believe that among the likely candidates will be some kind of chemically related science. It's likely that this will start out sounding as though it's all environmental and turn into other things as people branch out from that.

The fourth area is education. I think there's going to be tremendous change in chemical education during the next 10 years, because chemical education is a part of a very large system—general education—which is undergoing enormous changes. There will be less emphasis on the goodness of great numbers of students, in chemistry and elsewhere, because as a nation we face the fact that we've probably overproduced intellectual snobs. Thirty years ago that type had a unique and valuable role, but now we have more of them than are needed at the moment, and this is something that the nation as a whole will struggle with. There will be many experiments with new styles in education. For example, we may even discover that the lecture, which is, of course, a heritage from the time before there were books, is not necessarily the greatest way on earth to communicate. Chemical education will surely be a part of the changes.

I have one last thing to say which is *not* a prediction, but simply a hope. I wish I could predict that the style of chemical science would become a lot more realistic in self-appraisal. What we need is a good deal less sacredness in our view of ourselves and how we want people to view us. We need a good deal less feeling of total responsibility for everything that happens in the world.



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GEORGE HAMMOND

**Davidson:** I started to write out this 10-minute speech (I never do it for a 40-minute lecture), and it sounded terrible. One paragraph was platitudinous, and the next one struck me as likely to get me lynched. So instead, I've written out a series of more or less disconnected statements. I'm still likely to emit platitudes or say things that are going to get me lynched, but you're not going to criticize me for not having an organized, systematic presentation. It's not *supposed* to be organized and systematic.

My first topic is the economic future of chemistry. The economic future of chemistry is very bleak right now. Essentially, the chemical industry isn't hiring anybody, and the signals I pick up are that the probable rate of hiring in the chemical industries over the next decade or something like that is going to be less than half—perhaps a half to a quarter—of what it has been in the great years. Government support: The latest signals are that it's going to be up some from the recent bleak years, and just how that affects the over-all picture I don't know. Teaching: Teaching is bleak.

The main point of this table from the Cartter report (page 7) is that the demand for new faculty with PhD's is going to remain essentially constant through 1980, and then because of population trends or something, the demand becomes negative. Now, according to George Hammond, half of the teachers are going to be female, and the demand for male teachers is accordingly cut in half. I suppose the

Estimates by Allan M. Cartter of New Faculty Required to Maintain Quality, and New Doctorates Available: Actual and Projected 1965-1985

| Chemistry | New Faculty with PhD Needed | New PhD's |
|---|---|---|
| 1965 | 505 | 1,439 |
| 1970 | 492 | 2,030 |
| 1975 | 578 | 2,290 |
| 1980 | 475 | 2,888 |
| 1985 | –37 | Not Available |

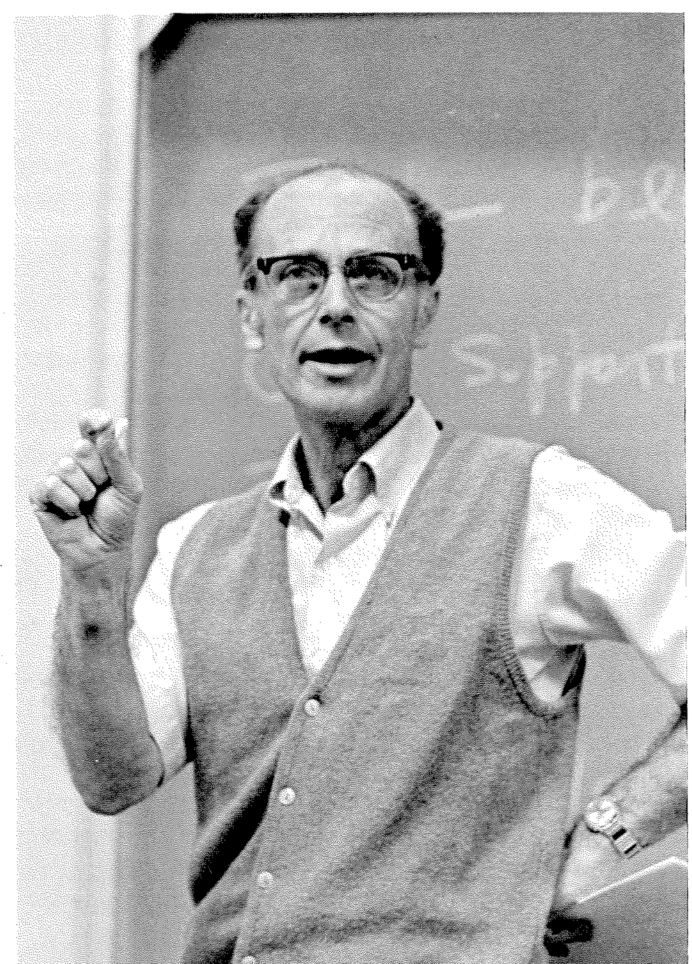
prediction of the constancy in the demand for faculty for the next 10 years can be regarded as modestly encouraging. I am uncertain about the future of biomedical research; there's serious talk of an additional $100 million a year for cancer and for other things. If that happens, it's likely to have an impact and make the opportunities in chemical biology somewhat better than the opportunities in straight chemistry, but I don't know how much. So if there's going to be any substantial expansion in opportunities for chemistry, it has to be new outlets.

My next statements are about the intellectual future of chemistry. The prospects are medium, neither terribly bright nor terribly dull. Chemistry is, in my view, a mature science. Molecular biologists have a tendency to think it is basically dead. Gunther Stent in his "Golden Age of Chemistry" lecture here a few years ago said that organic chemistry is dead, but my friend Jack Roberts says that there's more new stuff happening now than happened 10 years ago. I think he was thinking of things like the whole development or understanding of the mechanism of electro-cyclic reactions—his understanding, not mine, you understand—the synthesis of novel molecular structures usually strained or otherwise unstable, the development of the chemistry of the interaction of $\pi$-electron systems with transition metals, the developments in nuclear magnetic resonance, chromatography, X-ray structure analysis, and lots of other wonderful things. Well, these are all great advances, but they don't strike me as conceptual revolutions. Supposing I'd retired from the field in 1940 when I got a PhD; how hard would it be for me now to assimilate these ideas? I think most of them were things we talked and thought about in a primitive way then.

I'd like to say explicitly that in my opinion the purpose of modern science is to be useful. At one time science was a great intellectual liberating force; it liberated us from superstition in the guise of religion, it enlarged our vision of the nature of the physical world and of man, and then—in the cases of quantum mechanics and relativity—it enlarged our understanding of how we interpret the world. But I think that's practically over.

Chemistry is too myopic, too parochial, and too stereotyped. We all have too much of a tendency to do the same thing we learned to do in grad school.

NORMAN DAVIDSON

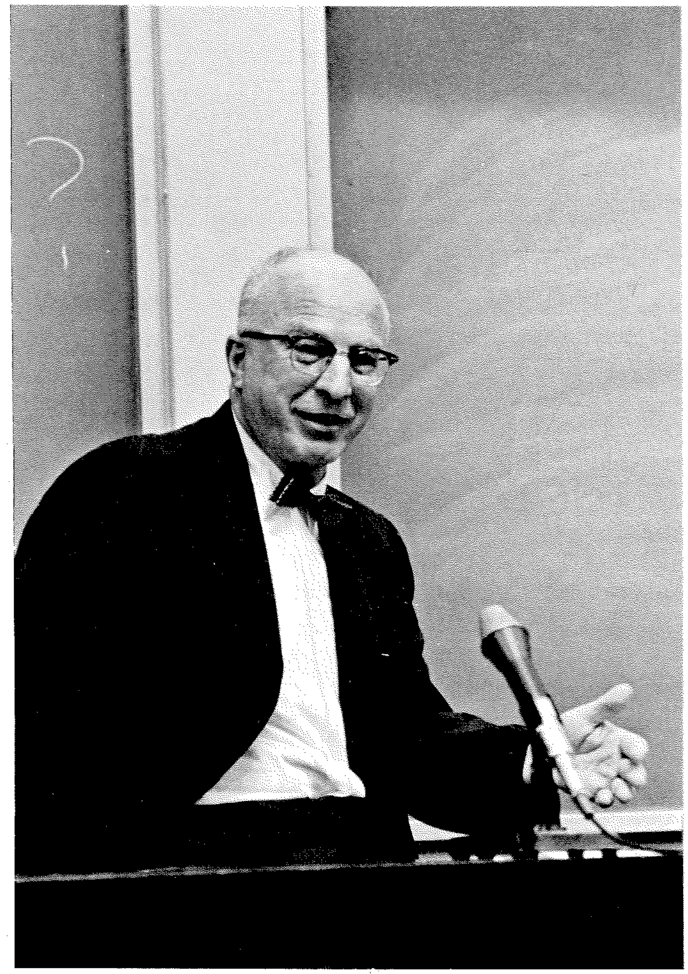


The only part of modern science that can lay a similiar claim now is behavioral biology or psychology. We don't understand man as a thinking, feeling being. The rest of natural science will lead to no conceptual revolutions. And a social justification, as distinct from the motivation of the individual scientist, will have to be related to its usefulness.

In this context basic research is clearly important, because it is basic. It gives us a base from which to attack and solve a number of problems. There is going to be both intellectual and financial pressure to select those areas of basic research that are likely to be relevant. The practical problems we face concern complex systems. My own feeling is that we're going to decide that the useful kind of basic research is to study models just one level of complexity down. Research on very simple systems is going to have a hard time justifying itself because the usefulness of such research for predicting the behavior of complex systems is limited.

I have a special note here to make the prediction that analytical chemistry is going to have a bright future. Even though analytical chemistry as such tends to be in disrepute among the more intellectual members of our profession, there is a good case for the proposition that a major fraction of progress in chemistry is due to progress in analysis. When I first started working in gas-phase chemical kinetics around 1946, it was really a speculative morass. People put things in tubes and they heated them up and watched the pressure change. Then they elaborated mechanisms, but the mechanisms involved reactions which were not in fact occurring. And gas-phase chemical kinetics didn't take off until it was possible to analyze reaction products by gas chromatography and mass spectrophotometry. Similar things have happened in chemical biology. The ability to sequence nucleic acids and proteins has increased the power of our research a great deal.

Under the title "The Trouble with Chemistry" I'd like to express some of my beliefs—and you can call them prejudices—about what's wrong with chemistry at the present: It's too myopic, too parochial, and too stereotyped. We all have too much of a tendency to do the same thing we learned to do in grad school. When I visit other departments of chemistry, I find them all talking about trying to build chemistry departments just like Caltech, Stanford, or Berkeley. Why shouldn't a department strive to excel in medicinal chemistry or in the chemistry of the solid state, or in polymers, and try to do it in such a way that students are educated in



So, I think if we are going to try to encourage young people to do this innovative work, it behooves us who are somewhat established to move out and be exploratory—not in deference to the young people but for our own self-respect.

HARRY DRICKAMER

understanding chemical bonding, non-bonding interactions, chemical dynamics? I think that if we did that we'd have a broader and more diversified and more interesting profession. But other schools hire guys who get their PhD's at Stanford, Caltech, and Berkeley, and unless they are willing to be adventuresome, I don't see much diversification ahead. I think chemistry has a reasonably bright future, but the more innovative and adventuresome we are, the brighter.

**Drickamer:** I'm somewhat appalled. I felt that surely by now one of these fellows would have gotten lynched and broken up the meeting, so I wasn't really prepared.
**Davidson:** We reserve lynching for guests.
**Drickamer:** I'm going to start from the position that a fair number of people feel that there are a fair number of problems within the present situation in chemistry. After all, when things are really booming, one doesn't hold meetings on "Whither Chemistry." At that point you're so busy turning out new ideas and new results that you don't have time for such meetings.

As I see the situation, back in the late 1940's there was considerable dissatisfaction among chemical engineers as well as physical and organic chemists vis-à-vis the relative excitement of nuclear physics in the late thirties and solid state physics in the late forties. Physical chemists felt that their approach to problems was both unsophisticated and sterile in the sense that thermodynamics had been fairly well milked and macroscopic measurements of kinetics weren't getting any further. There was the feeling among both physical and organic chemists that the semi-routine sort of synthesis—which we referred to as sticking another ethyl group in the beta position—didn't have the kind of sophistication and fertility that physics had. I think we did something about it: In physical chemistry we introduced the ideas of quantum mechanics and group theory from the theoretical standpoint and the instrumentation of physics in spectroscopy. In chemical engineering, we introduced primarily sophisticated applied mathematics, and the experimental techniques of fluid mechanics to study transport and moving systems rather than just making thermodynamic measurements. In organic chemistry, we introduced both the instrumentation of physical chemistry and physics and also the concepts of chemical dynamics. And the outcome of all this was very fruitful; we changed both the form and the substance of our approach. There was a burst of significant output in all these areas.

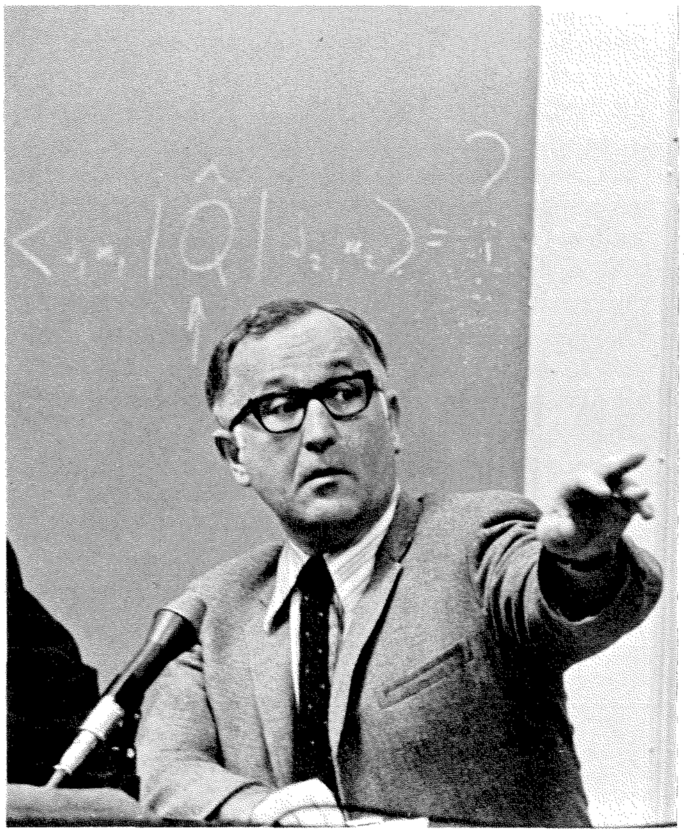
But in all human endeavors there's a basic conservatism in which one tends to preserve the form even when some of the substance is gone, and this applies to solid state physics as well as to legal systems, to social systems, and to everything else. If one has had a successful form and it has produced some real substance, we are reluctant to give up the form. We have tended to feel that it's more important to be elegant and sophisticated than it is to be fruitful; i.e., we have been using these elegant methods to study relatively simple systems: in engineering, relatively simple models for flowing systems; in physical chemistry, relatively simple molecules; in solid state physics, the alkali halides and silicon and germanium and things of that sort. Of course, we learned a great deal about these simple systems. But we tend to refine our measurements and refine our calculations without any real hope that a new generalization can ever arise from these studies.

I think what we need to do is make some kind of break into new areas where we may use sophisticated tools, but our approach may be relatively unsophisticated. People have to be willing to do a little more exploratory work, to open up to new fields. Even though you may use sophisticated techniques, the treatment may have to be relatively unsophisticated because it's a really new idea. I think of Mott & Jones, a book on the structure of solids, printed in 1936. I was talking to Mott two or three years ago, and he said, "Of course, you understand that all that's wrong," meaning it was unsophisticated. Still, this was probably the most seminal book ever written on an area of this kind, because it contained a lot of ideas that could be tested and refined. I think we will have to be interested and excited about partial solutions in large problems rather than complete solutions for very small problems. And I think we'll have to worry about interacting with other fields even though it's not possible to do it in a very sophisticated way.

I think that relevance is a very dangerous term. I come from an engineering education, and the engineering education of 30 years ago was relevant; we studied know-how which was obsolete before we got out of school. It had to be unless the field was dead. But I think there is no harm in studying specific systems of real use, where you can do something interesting and exciting. I can recall a time about 20 years ago when a man in a certain branch of chemistry told me that the thing that made him proud to be in that branch of chemistry was that there was no possible way of applying it. That kind of attitude was nonsense then, and it's nonsense today.

There are problems in breaking into a new field, there are problems in being exploratory. This isn't an easy thing to do, and to ask young people to stick their necks out is asking a lot. There are practical difficulties like getting support and interesting graduate students, and there are more important psychological difficulties. Man is a social animal, and it's a kind of comforting thing to go to a meeting and find a half a dozen other people doing very nearly the same thing you do. You can talk, and it's exciting. There's competition, but there's also companionship. And when you're doing things that aren't quite like what other people are doing, they say, "Well, gee, that's fine stuff. By the way, did you hear about what I'm doing?" So I think if we are going to try to encourage young people to do this innovative work, it behooves us who are somewhat established to move out and be exploratory—not in deference to the young people but for our own self-respect. Perhaps people our age ought to start whatever revolution we're going to start.

**Pings:** I promised the panelists that they could have a crack at each other, so I'll see if they have any pent-up feelings that they wish to vent right now. George, do you have anything to say in reply to these other two presentations?
**Hammond:** Yes. I've been sitting here in the middle and realizing that I really am in the middle. Because when I talk about where chemistry is going, I talk about outlets into other fields, and the two fields I always pick are biological chemistry, which is well established, and engineering science, which is becoming established. And on my left I have a man who clearly has flowed through the breach in the wall and is over there in biological chemistry. And on my right, there's Harry, who a long time ago discovered engineering science. And I'm the poor cat who's stuck back in the middle, which is not the sort of image I like to have of myself. I do not think that the middle is totally dead. The fact of the matter is that we guys in the middle probably do one hell of a good job—as good a job as anybody really cares about or is going to learn a lot from—in calculating the ionization energy of benzene. And that is probably as dead as Norman and Harry make it sound. But I don't think either of these guys can do such a good job with the boiling point of benzene. And that's neither engineering nor biochemistry, it's chemistry.
**Davidson:** I was going to ask what the boiling point of benzene is.
**Hammond:** 84°?
**Davidson:** 80? 78?
**Hammond:** Well, *I* haven't boiled it recently, and maybe it's changed in the meantime.
**Drickamer:** As an organic chemist, I think there was probably something else in your benzene.
**Davidson:** I have another question, but I think I'd rather participate in this discussion. I suspect that there are some problems, like the statistical mechanics of liquids, that are just not going to be solved theoretically. I think we're going to end up taking the attitude that we can measure the boiling point of benzene with a flask and a thermometer, and calculating it is not going to be popular,
**Hammond:** There was a time when anyone would have said that about the ionization potential.
**Davidson:** OK, but I said that right now about the boiling point of benzene. I may be wrong.
**Pings:** Yes, but you certainly hope that if you did do it with a flask and a thermometer, that then when you stuck the ethyl group in the beta position, you wouldn't have to repeat it for the new substance.
**Hammond:** In fact, Wilse Robinson [Caltech professor of physical chemistry] may have the boiling point of benzene while you're still around to admire it. I mean benzene isn't argon, but hell . . .
**Davidson:** Well, unless after Wilse gets done with benzene, he really can do ethyl benzene . . .
**Robinson** [from the audience]: Why are you picking on me, Norman?
**Davidson:** Hammond picked on you; I didn't.
**Hammond:** I didn't pick on him; I pinned my faith on him.
**Davidson:** I'm seriously trying to raise the question of whether it is really valuable to calculate the properties of certain simple systems when we know in a qualitative sense what's involved in the theory, and it's dubious whether the quantitative success of the theory will ever be sufficient to enable us to figure out things that we couldn't just as easily measure.
**Hammond:** In my opinion, yes. I think the point is not to know the boiling point of benzene—because even if I forget it, I can look it up—but because the second or third generation development coming out of this will be enormously fruitful inspiration for understanding the properties and behavior of amorphous systems in general.
**Davidson:** That kind of theory I'm in favor of.
**Pings:** Good. We agree on something.
**Davidson:** Listen, I think we agree on a lot. I think we are really saying very similar things.
**Pings:** Yeah. As a matter of fact I'm beginning to despair right now because we're going to lag. Harry, what do you have to say?
**Drickamer:** Well, I think it's very easy to sit up here and

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C. J. PINGS

lecture people on where we ought to go, but I've been trying to remember why it is that people stick so much to the form. I guess it's simply because they know they can accomplish something. They know they can do a reasonable job with a student in a reasonable time. The real question is how practical is this exploratory work as a means of getting people degrees. That's a very difficult problem.

And that leads me to another point. I think that one of the problems we face in universities is some decoupling between the natural desire of the faculty to do research and the available jobs for graduate sudents. The number of students in graduate school really depends on only two factors: the amount of money you've got to support them and the number of people that the faculty want working with them. It's in no sense even remotely correlated with the possibilities of them getting jobs in the future. We haven't had to worry about that, but I just cannot see a vastly expanding job market taking care of the number of students we're going to turn out. One possible solution is to introduce a much larger number of technicians into the university. A typical faculty man might have, instead of six graduate students, only three graduate students and a technician. He would still get a certain amount of research done for about the same amount of money, and I figure he's not likely to get any more money. In the best of all possible worlds, faculty would have their graduate students to work with, but in this second-best of all possible worlds, perhaps two faculty could share a technician and still accomplish their research and retain their teaching function.

I think there are disadvantages to this system, but there are disadvantages to any system in the real world. This is a smaller disadvantage than the vast expansion of the offering of PhD's in the last 20 years. My impression of the number of schools giving PhD's in chemistry is that it's doubled in the last 20 years; I know it's tripled in chemical engineering; and it's about doubled in physics. And really there's been no demand for this on the basis of people pushing schools to give more PhD's.

What has happened is that we have faculty educated to believe that research is a way of life, and they want bodies to work with. There are far too many places offering the PhD, and a man can shop around at almost any intellectual level and find a place where he can get it. This may be a bookkeeping detail compared with the big picture we've been talking about, but it would be a big detail in the life of students if they were encouraged to go on only if they have a particular vocation for it—not because they would be available to do research for those of us who are already established.