. . . *of Shell Models, Model T's, Mesons, and Thunderbirds*

By J. E. Goldman

IT is for me a source of great pleasure to speak at a Chicago meeting. The Chicago meeting is today one of our last remaining links with physics and physicists of the past. Our other big meetings have for the most part shifted their base from the natural habitat of the physicist —the informal, intellectual, and cultural atmosphere of the academic campus—to the cold, forbidding, businesslike bustle of a large hotel. Gone are the pleasant chats in the quadrangle, the equations carved in the soft earth of the Bureau of Standards, the chalk marks on the sidewalks, the blackboard bull-sessions of Pupin Hall, or an evening theoretical session on the Institute grounds in Princeton with a young graduate student named Feynman heckling Einstein. These are being replaced by market tips—both stock and flesh—and the slave auction of the fourth and fifth floors of the New Yorker. This is why it's all the more refreshing to be back in Chicago where the meeting still centers itself about the University of Chicago, where the spirit of Fermi, Compton, and Michaelson still permeate the air of the meetings, and science takes precedence over job security. In a way, Chicago may be held responsible for all these changes because the turning point of our profession came about here under the West Stands not 300 yards from this very spot. But the University has fortunately not allowed itself to become tainted with other pressures and even today in its walls are originated the newer ideas and concepts in what we used to know before the war as physics but today have to refer to almost apologetically as fundamental physics. It is here at the University of Chicago that so much of the important physics of this century was fathered and where one still has the feeling that physics is an integrated whole and not an assembly of separate noninteracting orthogonal subjects. Here also was born the shell model which in effect brings me to the title of my remarks of this evening: Of Shell Models, Model T's, Mesons, and Thunderbirds.

I shall not talk of physics. There are far more competent minds and tongues in this very room and at this very table. I shall instead talk of physicists for it is here that I believe the real problems of administering research lie and it is this subject to which I have given some concentrated thought while in both academic and industrial surroundings. I am concerned about the transition—or perhaps I might even use the term degeneration—of physics from a calling to a profession. The graduating physicist of yesteryear felt emotional and spiritual ties to his physics; an inner compulsion to seek out answers to the challenging problems he encountered and to derive great joy from the very process of seeking; to eat, sleep, think, and do physics twenty-four hours a day. I'm afraid that the younger generation of physicists has lost its dedication to this philosophy. The young PhD today considers his physics as a simple means to an end. It is a way of earning a livelihood so that he can indulge in the vicarious pleasures of life—and I suspect that even the nature of these extracurricular pleasures has seen changes —which is another way of saying that the patterns of cultural interests of today's young physicists differ markedly from those of his predecessors. I must admit that my statistics are not great but I think they are enough—as the election pollsters say—at least to indicate a trend.

May I point out here, parenthetically, that this is not a problem that is unique in the area of scientific pursuit. I think that similar concern is being encountered in the fine arts; that commercialization has supplanted inspiration as the dominating force in the creative arts. Gian Carlo Menotti bemoaned this fact in a wonderful article in the Sunday *Times* magazine a few years ago entitled "A Plea for the Creative Artist". His concern, like ours, was that true creativity in the arts cannot flourish in a millieu in which the public and the students begin to worship the end product rather than the very process of creation. In a way, the problems of inspiration for the artist and the composer are not dissimilar from those of the physicist and mathematician.

But physics and physicists are a closely coupled system. The psychology of discovery and invention requires the complete absence of constraints on the part of the inventor or discoverer. Again I recommend a published work: Hadamard's *Psychology of Invention in the Mathematical Field* which was brought to my attention by Professor F. R. N. Nabarro. I think that we would get complete unanimity among all those responsible for research in physics in concluding that productivity is high both qualitatively and quantitatively when the producer finds satisfaction and inspiration in his goals. Therefore, this deterioration of the inner physicist cannot but leave its lasting impression on physics as a whole, and, therefore, on our life, our security, and our civilization

What are the causes and wherein lie the potential solutions? Obviously, there is a myriad of intangibles of which the entire problem of supply and demand is both a cause and effect. But, by-and-large, the blame should be shared by all—the schools that train, the agencies that support research, and the universities and laboratories that ultimately hire the physicists. I have seen it in the very recent past from all three vantage points.

First, the schools. Again harking back to similarities with the fine arts, it used to be said that one can detect even in the work of the experienced painter the strains and the inspiration of a struggling adolescence, and what painter did not have to struggle through the formative years of his learning? The process of learning in science is similarly colored. Many may disagree, but I firmly believe that the ease of the life on all levels of the present-day graduate student is not conducive to ultimate dedication. The lights no longer burn in university laboratories in the wee hours of the morning. The graduate student, like any other laborer, closes his book, locks his

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laboratory, and picks up his dinner pail to go home to his family at 5 o'clock, and even collects a respectable check at the end of the month for having performed the services for some agency which naturally has first calling on his time and ultimately his interests. The research activities at the universities have themselves become projectized, and more often than not, the research carried out is done not because the professor himself wished to pursue a given area but because money is available for certain favored projects from many philanthropists, and from the greatest philanthropist of all, OXR, where X = N,O, or S. Finally, in order to justify the use of these funds, the professor himself has become an important administrator and, as a result, the graduate student has lost the close personal contact, the source of inspiration, and the continuously guiding hand that helps mold his future attitude as a physicist. The graduate student has to make an appointment to see his professor. I think this is to be particularly abhorred in the areas of experimental physics where most of what a student learns he learns by watching his mentor—the artist as it were, at work—by assimilating a method or an approach to experimental science—not by being called into an office, told what to do, and be led off on his own. Once again may I emphasize that I am simply indicating trends, not attempting to generalize. Needless to say there are an untold number of exceptions to all these observations.

But are the professors really at fault? Here is where the second culprit rears his head: government as well as private. For, the emphasis in nearly all supported research has been not on people but on projects; not on the creation and support of an atomosphere that will prove conducive to the generation of new ideas but in the squeezing out of a final product. In a way, I think this is the crux of the problem and all supporters of research are equally guilty: It is the emphasis that has been placed on all levels on the product rather than the process of creating it. You all recall that beautiful satire which appeared in the *American Scientist* showing how Newton would have proceeded to discover the apple falling under gravity under an appropriately classified grant from some old English alphabetical agency—probably the Agricultural Ministry within whose purview apples fall. I am sure that if we go right down the line in the history of physics, we would all agree that none of the important discoveries in physics are likely to have been made if the discoverers would have had to define a path of operation and justify the use of that coveted half-time for research. Yet, in spite of all the agreement and unanimity, many of our agencies and foundations continue for the most part to turn a deaf ear to people per se and instead encourage the firm delineation of projects and the carving out in minutest detail of areas of research. The only atmosphere in which originality and creativity can truly thrive is one without constraints. We must assume with Lagrange that with each constraint we lose one degree of freedom, and who is to tell which degree of freedom is the one which will lead to an important discovery or a revolutionary breakthrough?

And this leads me to the last of the culprits: The final market place for which our herds of physicists are being grazed, the research laboratory, industrial or otherwise. Needless to say, here I am treading on rough territory as I am myself in one of these glass houses, except that I enjoy the privilege along with my very progressive and like-minded bosses of laying the foundation and of inventing the stone-proof glass of the structure. One of them, by the way, who is in an important policy-making position, is a University of Chicago PhD and a former student of your local chairman, Dr. Allison, who is seated at this table tonight. The same prerequisites obtain in this case too. The problem in industrial laboratories today is that the jobs are created by the administrators who then seek to find physicists to fill them. Again more often than not, these same administrators will clearly define all the terms of reference including conception of the end product. It's strange what goes by the name of fundamental or basic research today. The theory of true fundamental research must be predicated upon the proposition that the emphasis must be placed on people, not on things, facilities, job specifications, or jobs. One should not compete in the New York flesh market for people on the basis of fringe benefits, conditions, areas of activity, scope of pursuit. The problem is far simpler than that. Put the premium on the excitement of pursuit, not on end result, and we will get better physicists and the physicists are more likely to turn out original work and I believe that for such jobs there will be enough to go around. If you turn the question back to me and ask what do you do about this in your own laboratory, I would like to answer the question, but I promised Dr. Darrow no commercialization or advertisement.

But I think I can circumvent the commerical overtones by alluding to an example very close to my heart. Having spent the better part of my professional career working in the field of magnetism, I am very often asked by my friends how come I joined Ford Motor Company? What is Ford's interest in magnetism or magnetic materials? The questioner invariably expects an answer referring to magnetic clutches, magnetic suspensions, and a variety of other present or potential applications of magnetism in automotive engineering. But that is not at all the case. Frankly, I have not the slightest interest in these automotive applications of magnetism. And, in fact, at the present moment nearly all of our effort in the field of research in magnetism is directed at nuclear, para, and antiferromagnetism, areas where one would be hard put to find applications in the automotive industry. But please remember that without Langevin's classical work on the theory of paramagnetism, Bohr would not have developed the quantum theory of atomic structure when he did and without Bohr's atom—need I say more? It is an interesting commentary of history that Bohr's doctoral dissertation was devoted to a proof of the theorem that a truly classical system should give no susceptibility (now known as the Bohr-Van Leeuwen theorem). The fact that Langevin's theory worked none-the-less led Bohr a year or so later to recognize the implications of Langevin's assumptions. He could only conclude that Langevin in fixing the discrete value of the magnetic moment of an atom was in effect quantizing the angular momentum without consciously knowing it or, as Van Vleck put it so beautifully, Langevin who like our hero in "Le Bourgeois gentilhomme" who was astounded to learn that he spoke prose all his life without knowing it. So you see how impossible it is for any true research physicist or administrator to foretell which areas of research will be most promising.

However, this I must say à propos of fundamental research in our industry. If there is any industrial area in the United States where an important new idea is absolutely necessary for survival, it is in the automobile industry. The oil prospects for the world are so very dim that this largest of all American industries must have an important, original, inspired breakthrough sometime within the next 25 years, for by then, we shall have to kiss goodbye to any means of locomotion which requires for its use the combustion of fossil fuels. What we must have is something that is so new, so radical, and so unanticipated that it would be folly to compartmentalize our thinking into how to go about pursuing this.

When Henry Ford built his first Model T, the father—or should I say mother—of the shell model, Chicago's Maria Mayer, wasn't even born yet. But in the brief span of a generation or two we have seen the nucleus become a most important factor in our civilization, so much so that all our forward thinking about power sources of the future invariably invokes consideration of nuclear energy sources. I submit that for all we know the power to move the successor to yesterday's tin lizzy—the Thunderbird of tomorrow—will have been conceived through the catalyst of a bright idea in meson physics. We cannot afford to slough off such areas of physics as too esoteric or impractical and we can ill spare the products of the scientific geniuses whose pleasure of life is the pursuit of knowledge in field theory or general relativity. This is why we feel that we are partners in research with the great universities and not competitors as inferred by physicists and public alike from the spectacle of the New York flesh market and the Sunday *Times* financial section. Between us, we have the joint obligation of keeping physics pure and free.

That pure research remains the life's blood of the universities is an inescapable postulate to all but the occasional overzealous heretic. It is not quite so obvious what industry has to gain and vice versa from a broad program of pure research. To state it more bluntly, what's in it for us? If pure research is to be motivated by philanthropy and altruism why not take the money that government and industrial laboratories would spend on the pure part of research and give it to the universities who could probably keep it purer anyway? The answer to this is many sided. To begin with, we do have the selfish motivation of attracting good people—not only for their sake but for our own. For, even those charged with the more menial, more prosaic, or more industrial, applied or quasi-developmental tasks profit from the interaction with good physicists and good mathematicians. Experience tells us that the only way you can attract and keep the better and more original men is to let them do their own free research. But there is an even more direct consequence of this in relation to the more long-range problems characteristic of our industry and many others. If, as I indicated, we are to look forward to an earth-shaking new idea in the next score and a fourth years, we would like to maximize the cross section that the idea will be nucleated within our organization. And if we are not so fortunate, we must at least maximize the probability that when such an idea is germinated somewhere in the world, our organization will understand it and know what to do with it. I think it is almost superfluous to cite to an audience such as this the ample evidence that bears this out in many great American industrial organizations. The invention of the transistor has now become a classical example of what maximizing the cross section for the generation of ideas by the geniuses attracted to a major research organization can do. Perhaps not so well recognized, but equally valid, is the fact that the leading industrial organizations today in the field of atomic energy are those that had the good physicists, chemists, and metallurgists on their staffs in time to understand and recognize the potentialities. I think no small measure of credit for the conception of this philosophy belongs to our other speaker of this evening, Dr. Condon, who implemented this point of view when he left Princeton for industry in 1937. It is a matter of record that the recipients of the industrial fellowships provided by the organization which he joined now occupy top positions in American industry and have undoubtedly played a formidable role in furthering atomic research and technology not only in their own company but in the country as a whole.

It is, in a way, superfluous to spend so much of the time of an audience such as this making a case for fundamental research in industry. In fact, judging from the reactions of most of the younger crop of PhD and post-PhD physicists it is the converse that is called for. These days one has to sell applied physics to the physicists and there are often no takers. It has not been my intention at all to minimize the new importance that applied physics has taken on in our technology. But I believe that many of the attributes of an atmosphere conducive to creativity in fundamental science are just as valid in the area of what we call applied physics. The distinction between fundamental and applied physics is simply one of motivation. I like Dr. Estermann's definition: fundamental research must be motivated from within; applied research can be motivated from without. One may question the sufficiency of this definition but it certainly has the element of necessity. But while the administration of applied research in physics is fraught with problems, they are mostly localized ones which are a function of the particular organization and personalities involved. But the problems of fundamental research are common to us all. It is our collective responsibility to provide a continuity to the totality of physics in the face of diverse pressures and occasional stifled lines of communication. That *physicists* will continue to flourish in spite of these occasional obstacles, we do not doubt; whether *physics* will, depends on how close to zero the order of these perturbations becomes.