CONDENSED MATTER PHYSICS IN A MARKET ECONOMY

Knowledge in the field has become a commodity, subject to the same market forces that apply to silicon chips or lasers. The present malaise among researchers is due to an oversupply in a time of decreasing demand.

John M. Rowell

In 1991 I attended my 30th consecutive March meeting of The American Physical Society, held in Cincinnati. This anniversary made me reflect on the changes I have seen in the field since I listened to my first ten-minute talk in Baltimore in 1962. I believe it was obvious to many in Cincinnati that there is a malaise affecting our field that was not present 30 or even 10 years ago. By "our field," I mean what is variously called solid-state physics, condensed matter physics, materials physics and materials science.

The malaise was evident not only from the discussion of funding inadequacies, which has been a routine topic in corridors for years, but also from other signs, such as the number of students asking me for jobs, the mention of professors and students carpooling from Boston and Florida to save money, and the fact that National Science Foundation literature advertising fellowships in Japan disappeared early in the week. Most striking was the sense of despair that seemed to be openly expressed by many colleagues from industry, universities and national labs alike.

The March meeting forced me to think more seriously about the state of our field, and I am led to the conclusion that our field exists in a state of contradiction. On the one hand, the field is stronger than ever, both numerically and scientifically. On the other hand, its practitioners are in a state of depression. I have come to believe that this depression is a result of not facing and adjusting to the realities of today's environment. In this article I will argue that scientific knowledge in our field is a commodity, subject to the same market forces of supply and demand that apply to any other commodity. I believe our present malaise, at least in the United States, is due to oversupply in a time of decreasing demand.

It is valid to ask why PHYSICS TODAY considers me qualified to comment on such matters. I am not convinced that I am, but during my three decades in the field, I have experienced much of what is discussed in this article. After 22 years in the most ivory tower of all industrial laboratories, namely Bell Labs of the 1960s and 1970s, I joined Bellcore, where I faced the task of convincing the regional telephone companies that research in our field was worth their investment (in the end, I failed). Now I am trying to make money from superconductivity at a small company. If anything, I have become a customer of the field rather than a contributor to it, in that I hire one of its products, students, and use another of its products, knowledge.

John Rowell is vice president and chief technical officer of Conductus Inc, located in Sunnyvale, California.

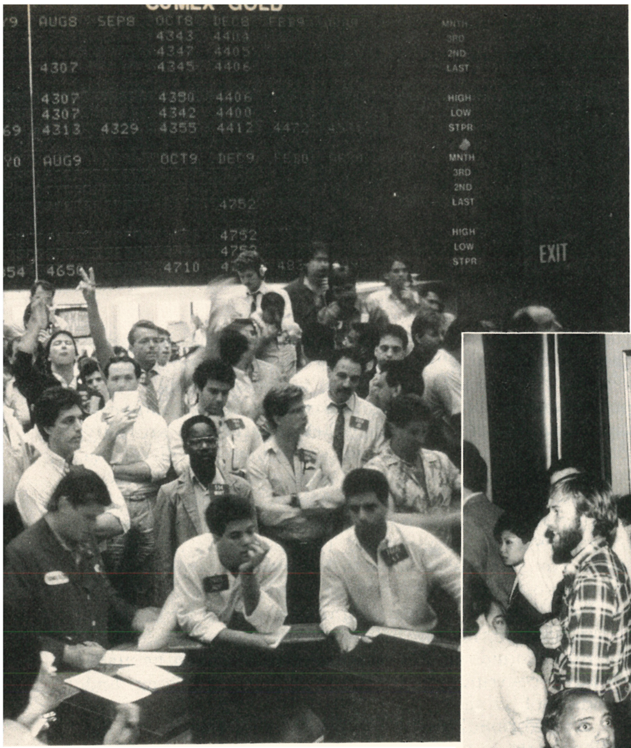
The growth of the field

Judging by the size of the APS March meetings over the years, ours is indeed a healthy field. In 1962 in Baltimore 395 contributed and 42 invited talks were presented. Last year in Cincinnati, the meeting included 3900 contributed talks and 420 invited talks; often 30 sessions proceeded in parallel. This year's March meeting was even bigger. As someone said, the difference between attending and staying at home was that by attending you could listen to 3.5% of the talks. The growth of the field has been monotonic over 30 years.

Obviously, someone is paying for the research presented at these meetings. I did not meet anyone in Indianapolis whose funding came from his or her own pocket, nor do I know of any rich benefactor with an interest in supporting our science. The complaint that funding has decreased is simply a myth. The US is funding a field that in total has grown dramatically.

How has this growth been achieved? Obviously not through the budget of NSF alone. Such dramatic increases have not happened by chance. Numbers are hard to come by, but the condensed matter physics division of APS has tried to analyze those that are available. Multiple sources have supported our growth: NSF, the US Departments of Energy, Defense and Commerce, and significantly in recent years, the states themselves. (I will discuss industrial support later.)

Perhaps a more important question is, Why has this growth taken place? One answer is that the field's



COMMODITIES EXCHANGE INC

AIP NIELS BOHR LIBRARY

Exchange of information in solid-state science gives the APS March meeting (right) much of the feeling of the Commodities Exchange in New York (above).

scientific scope has become broader. The second answer surely is that our field has economic value. Of all the scientific fields, the impact of condensed matter physics and materials science on today's way of life and standard of living is so glaringly obvious that we generally forget to make the case. But even when others use the same argument for fields with much less real dollar value to the nation, events such as the Gulf War make our case for us. Our field has grown because the mission-oriented government agencies wanted it to grow, because university administrations saw it as an area where professors could get funding, because states believed it would enhance the local economy, because we in the field found it an exciting challenge, and because we convinced others of this excitement.

If the stress on the field is not due to total funding, is it due to a lack of exciting research topics? This hypothesis we can reject immediately. This year's March meeting was full of exciting science. Superconductivity rolls on, with high transition temperatures observed in an entirely new form of carbon, as well as the oxides. Integrated circuits comprising 16 layers of epitaxial superconducting and insulating oxides have been reported. Scanning tunneling microscopes yielding fascinating atomic topologies were widely displayed in numerous stands at the trade show and appear likely to become diagnostic tools on semiconductor process lines. The atomic engineering of semiconductors, which allows electron beams to be steered on the surface of III–V semiconductors, has made optical communications a major growth industry and led to the many sessions on the quantized Hall effect. The micromachines etched into silicon fascinate us with their potential, and single-electron devices define the limits of electronics. If I were beginning my research career again, all these topics would seem to me just as exciting as the tunneling studies I chose to work on in 1961. The field is perhaps more exciting than at any time in the last 30 years. The science itself then is not the problem. But our excitement alone is not enough to justify the growth of the field. Only excitement on the part of those who pay for the science creates such a justification.

Diagnosing the malaise

The malaise, of course, is not confined to our field, as evidenced by PHYSICS TODAY's calling a roundtable on "Science under Stress" (February, page 38). Because other fields are outside my experience, let me speculate on the reasons for the low morale in solid-state science. While difficult to quantify, it seems low in all our communities—universities, national labs and industrial labs—but for different reasons.

The universities. From my observations of the universities and from discussions with friends who are professors, the problem seems to be twofold. First, as a result of our growth, the funding level per project, especially from NSF, is simply inadequate. The acceptance rate for a new proposal to NSF in our field is very low, although not quite as low as the 1-in-23 acceptance rate for the Department of Commerce's Advanced Technology Program in 1990–91. (The ATP is for industry, so funding competition is not limited to university scientists.) To obtain block funding, many universities feel they have to create materials research labs, materials research groups or science and technology centers. Resentment toward these centers grows, even though they seem to me to have real value and the work within them is largely carried on by the same people who call themselves individual investigators. The NSF budget grows appreciably, but not in our field; instead, the money goes to rebuilding an education program that was eliminated in the first Reagan Administration. Given the current state of US mathematics and science education, I doubt that any of us question the wisdom of such choices now being made by NSF.

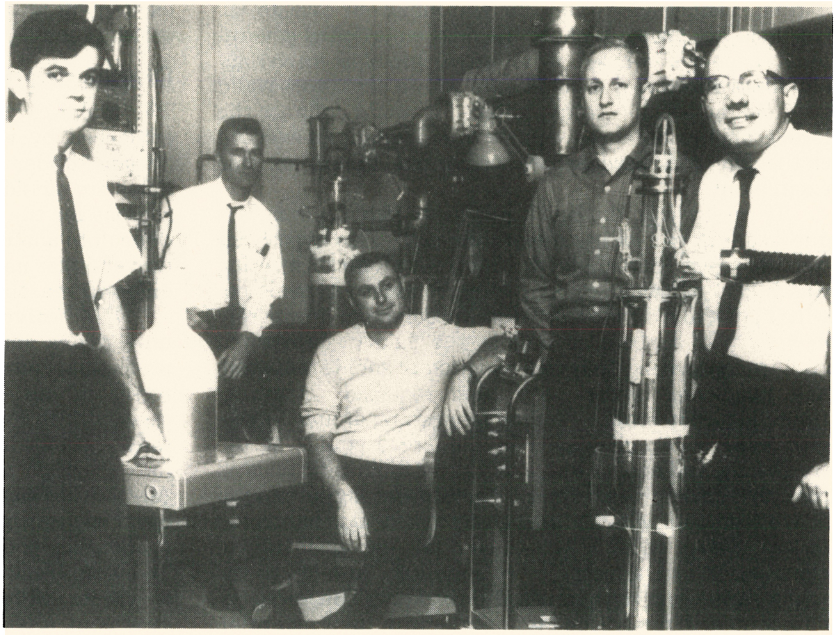
I suspect the second problem causing unhappiness in university circles is simply the process by which funds have to be acquired. Much of the creativity of the academic community is spent in asking for funds, not in using them. The use of word processors proliferates proposals (although it is not clear that the word processor improves their quality, only their number and appearance) to multiple agencies, and so in addition to the time spent in writing, we also spend more in reviews and site visits. Our field is not one that lends itself to massive projects, but obtaining $70 000 from NSF takes as much work as obtaining a million dollars.

One partial solution is for agencies to enforce a strict limit on the length of proposals. Recently I reviewed a proposal to an agency in Japan for a large sum of money (by NSF standards), and it was one page long! While this seems extreme, if the US funding agencies will not put their own houses in order in this regard, those of us involved in peer review should do it for them by returning all proposals that exceed 10 pages of technical content and 20 pages in total.

The national labs. It is said that uncertainty about the future is a major cause of stress on an individual. This appears to be the primary reason for morale problems in the second sector of our field, the national labs. It is clear that the national labs are seeking a new role and that this search will intensify as defense budgets decline. Their only guideline for the future seems to be "Help industry," but their current technology transfer efforts are only Band-Aid solutions to the question of how to redirect the massive Federal investment made each year in these laboratories. Clearly, the labs will not disappear simply because the justification for them has been made obscure by worldwide political events. They are too large a factor in local and state economies to be forgotten by politicians. And because they receive a large fraction of the funding of our field, they will remain a significant reason for the view in Washington that our field is well funded. Therefore, as defense demands decline, the national labs must be made effective contributors to the national well-being and standard of living.

An additional frustration felt by our colleagues at the national labs is that to transfer technology to industry, they must interact with lawyers. It is said that there are two kinds of lawyers, those who help you do what you can do within the law and those who only tell you what you cannot do because of the law. It seems the US law schools train only the latter, at least in the area of technology transfer. The time required to sign a technology-transfer agreement between a national lab and a company is at present unacceptably long.

Industry. I admit that my background gives me a bias, but I believe the major change in our field over the last ten years has been in the third sector of our field, industrial research. It is here that the malaise has become a chronic illness, terminal in many cases. Looking back 20 to 30 years, I remember excellent corporate labs that have



At the Ford Motor Company's scientific laboratory in Dearborn, Michigan, in the 1960s, John Lambe, James Zimmerman, Arnold Silver, Robert Jaklevic and James Mercereau (left to right) made the first observation of quantum phase interference between two Josephson junctions, which is the basis for the superconducting quantum interference device—the SQUID. These sensitive detectors of magnetic fields are now used to detect currents in the brain and for other applications. (Courtesy of Silver.)

Ivar Giaever is the focus of a General Electric advertisement dating from 1973. He shared the Nobel Prize in physics that year for his work on electron tunneling, which was done at the GE Research and Development Center in Schenectady, New York.

since disappeared and some that have become simply contract research houses. The RCA Laboratory is a prime example of the latter, but there are many others. In those days, most industrial labs were supported by their own corporate funds, while today the vast majority scramble to compete with universities and national labs for Federal funds. This represents a very significant shift in industry's attitude towards research. Instead of expressing the belief that investment in research is valuable to the company, the prevailing attitude now is that research is only worthwhile if it is largely supported by someone else. This change has created the odd situation where research directions are determined more by funding agencies than by the industry itself.

Those industrial labs that have survived have changed markedly in their style and goals. Only AT&T, IBM and perhaps Exxon now carry out research in solid-state science that can be called basic, generic or long range, and most companies find the word "basic" unacceptable. To quote someone from a telephone company in 1984: "I hope you never do the kind of work at Bellcore that would win a Nobel Prize." The majority of the industrial labs produce so-called deliverables, either for defense funding agencies or for divisions of their corporations. As these deliverables are largely defined in advance, the time scale for payoff of industrial research has been reduced from 20 or 30 years to fewer than 5 or 10, and in some cases to as little as 1 year.

It is my belief that the state of US industrial research, while directly affecting only those of us in industry, is a cancer spreading throughout our field. Although we have not been effective in selling the economic value of our field, nevertheless its impact on society has been obvious, and the source of great personal pride to many. We have secretly used it to justify our existence and the long hours spent at the bench or—more likely—writing proposals. Now not only is that justification questioned, it seems to be evaporating as we watch. If US industry does not want or need our research, why are we doing it, even in universities and national labs? All that then remains is the challenge of the science itself or the personal need to take home a paycheck.

Interpreting the message

If we accept that this summary of the state of our field represents even an approximation of the truth, rather than just a singular personal view, the next question is, What message is our field being given? For we are being told something, even if we refuse to listen. The present state of our field is not being created by the whims of a few individuals, whether politicians, members of the Administration, heads of funding agencies, CEOs or venture capitalists. The leaders of industry are not inherently vindictive towards research, and to reach their present positions they must have demonstrated in the past the ability to make sensible decisions. CEOs are not stupid; they are simply reacting toward research in a way that is stimulated by the present US business environment, and most significant of all they are reacting uniformly.

I believe that our present situation can be interpreted in terms of four possible messages that we are being sent:

▷ Research in our field is no longer needed. We have created enough knowledge to take care of all the needs of all hardware-related civilian and military technologies into the indefinite future.

▷ The US can no longer afford research in our field, at least not at present levels.

▷ US industry is unwilling to make long-term investments in future knowledge.

▷ Research, as we carried it out in the past, has failed US industry. We did not deliver, and unless we change, we will have to disappear.

It would be easy to stop reading at this point, but I believe there is some truth in these messages and that they deserve examination.

Until recently, I had always believed that research is *essential* to commercial success, whether at the corporate or national level. Surprisingly, I can find plenty of evidence that this is not true. For example, with help from the Federal government, MCI has become a successful communications company in competition with AT&T. Yet it did this without ever investing in research in our field, although its business is based entirely on our inventions. It simply made use of the products that resulted from worldwide research investments—particularly those made by its competitor, the Bell System. Another example is General Electric's 50% share of the market for magnetic resonance imaging machines. GE did not do the basic research in this field; it simply waited until others demonstrated the potential of the technique and then moved into the development phase to build a profitable business. A third example is the smaller computer companies that do not carry out hardware research but are competing very successfully with IBM, which does spend a great deal on such research. As one of my colleagues said, perhaps what these examples indicate is that research is essential, but you'd be smart to let someone else do it.

Large areas of industry exist that have never invested in research. One such is the banking industry, which moves a great deal of data worldwide on a daily basis using advanced technologies that it has purchased, not created. Another example is the airline industry, which relies on the government to provide the air traffic control network. Why does the communications industry invest in its own research to move voice and data over its network, while the airline industry does not need to invest in advancing the network it uses to move people?

Even at the national level, I can find reasons to question the belief that research inevitably enhances our economic well-being. While the US research investment might be comparable to that of other nations on a *per capita* basis, in total it is so massive that the results should be overwhelming. Yet there are many countries where the quality of life seems comparable or superior to that in the US. Housing, air quality, health care, transportation, education and urban infrastructure are not superior in the US, and they cannot therefore be linked to total research investments, in our field or any other.

And then there is Japan. Although today Japan's industries and government agencies are investing heavily in research—creating a long-term optimism for science and technology that I find exhilarating when I visit—the success of industrial Japan in the postwar years was not created by research. It was achieved by taking the existing worldwide knowledge base in various technologies, and particularly that in our field, to create high-quality products that customers wanted.

The above thoughts suggest that knowledge in our field is a commodity. Companies and countries can produce it, buy it, trade it, acquire it from the open literature or simply hire people who have it. To succeed in many businesses, it is now no longer essential to produce this knowledge in house, as long as it is being produced somewhere. Given the global investments in our field, the important step is not to create knowledge but to use it for competitive advantage, whether in building the armaments of the Gulf War or in producing CD players and TV sets.

The second message we can read into the present situation is that the US cannot afford research in our field at levels sufficient to support the present enterprise. I suspect this is partly true. Obviously there are many competing needs within the US, and it is impossible for me to argue that all those needs must take a lower priority than investment in our field. It is also clear that a debtor nation must make choices among all those needs, and that such choices within a constrained budget will leave each individual unhappy over his or her allocation. Still, there are enough examples of government spending in other areas of science to indicate that our problem is not affordability, but rather the value placed on research in our field.

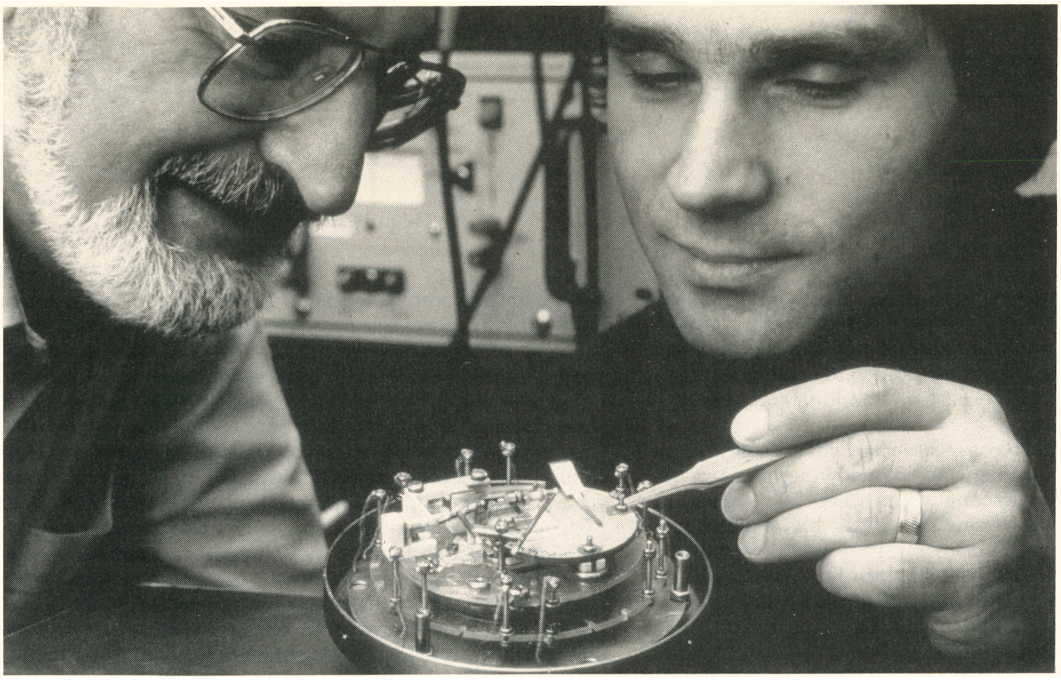
Over the past decade or two, our field has been driven to a very large extent by investments made for military purposes. Nobody expects these investments to continue at present levels given the changed world scene, and I doubt that many of us in the field would argue that they should continue. This is one factor in the decreased value of our field.

Another pressure on our field is that the national economy is changing from one based on manufacturing to one based on service. As mentioned earlier, service industries have not traditionally invested in hardware research. Therefore, if the national economy continues to evolve in this direction, demand for our field must of necessity decline even more.

We can therefore identify two global changes that are already affecting our field and will continue to do so: the demise of the Soviet Union and the much-reduced threat to national security; and the movement of manufacturing offshore and its replacement within the US by service companies that do not invest in research.

For those of you who would argue this point with me, I cannot resist the example of Bellcore, which can be seen as a microcosm of the broader national picture and an illustration of US industry's view of long-term investment. Bellcore was created upon the divestiture of the Bell System in 1983–84 to act as the R&D arm for the seven regional Bell operating companies, which became purely service companies prohibited from manufacturing. They now bring in total annual revenues of around $80 billion—much larger than AT&T ($37 billion) or IBM ($69 billion). Obviously communications is a high-technology business, in fact, one that has been changed out of all recognition by the contributions of our field in the form of the transistor, integrated circuit, solid-state laser and optical fiber. Thus the regional Bell companies' business was created by research investments in our field made by the Bell System and many other entities from the 1940s to the 1980s. Typically, high-technology companies invest in R&D to the extent of 8% (AT&T and IBM), 15% (Hitachi) or even 100% (startup biotechnology and superconductivity companies) of their annual revenues. Since divestiture, the regional Bell companies have invested about 1.5% of their annual revenues in R&D. But the research fraction has been extremely small: Until October 1991, their applied research in our field amounted to roughly one person for each billion dollars of annual revenue. Comparisons with nonmanufacturing communications firms in other countries are revealing: For example, NTT, British Telecom and Australian Telecom support, respectively, 30, 15 and 4 researchers in our field per billion dollars of revenue. Last October the regional Bell companies decided that they could not afford even this level of effort and began eliminating all long-range physics and materials research. Eventually all that will remain is research tied specifically to the reliability problems of today's operating plant and tomorrow's optical network. Thus the net result of divestiture is that almost all of the research in our field is supported by AT&T, which represents about one-third the revenue base of what used to be the Bell System.

The fourth and final message to our field comes from those in US industry, and it is that we have failed them. This of course is hard for us to imagine, because it means the blame for our present state lies with us and not with "them"—that is, the CEOs; the Administration; Congress;



The scanning tunneling microscope took seven years to go from its invention to a variety of commercial products. The STM's inventors, Gerd Binnig (right) and Heinrich Rohrer of the IBM Zurich Research Lab, are shown with one of their early devices; their design of the STM earned them the 1986 Nobel Prize in Physics. The 1991 PHYSICS TODAY *Buyers' Guide* listed 16 companies that now sell STMs.

the funding agencies; lawyers; those responsible for the cost of capital, financial policies and tax structure; and Judge Harold Greene, who supervised the Bell System breakup. However, it seems to me we have to face the possibility that the changes in industrial research labs over the past 20 years constitute an expression of dissatisfaction with our contributions over that time. We have received an appraisal by those who have paid for our work, and our grade is not a passing one.

Understanding our role

The changes at AT&T, GE, RCA, Du Pont, Varian and Bellcore lead me to question our understanding of the role of the industrial research labs, as well as the roles of the universities and national labs.

In our field, more so than in almost any other, we can subscribe to the necessity of having a continuous flow from the knowledge base—created by basic, fundamental or generic research—to applied research, to the building of prototypes and the development of products, to manufacturing and profits, and hence to employment and an enhanced standard of living. The question is, Where should the universities, national labs and industry contribute within this chain, which begins with knowledge and ends with national competitiveness and well-being?

Despite their problems, the universities of the US do a remarkable job of maintaining our knowledge base in condensed matter physics and materials science. They have managed over the years to compete for the best and brightest in our field, to provide research facilities and to educate generations of intelligent and inquisitive students, so that they now represent a research enterprise that is incomparable at the most basic level. This first link of the chain is strong.

However, the universities do not appear to be facing up to the changes that must naturally follow from today's situation in industry and the country as a whole. In our field, the product of most universities after five, six or more years is the PhD scientist who has been taught that research for research's sake is a pure and honorable objective. For many, it is the only objective. But is this what US industry needs today? Is it conceivable that the "working degree" of physics and materials science should be the master's degree, rather than the PhD?

The role of industrial research and development is to apply knowledge to create new products, services and capabilities, and to focus generic knowledge to meet the specific needs of the owner company—that is, those paying for the research. To quote Ralph E. Gomory, formerly the senior vice president for science and technology at IBM: "R&D is not something that can be useful alone, and the focus on R&D alone is wrong. R&D is part of the product-making process."[1] He identified the shortcoming of industrial R&D as "the lack of consistent, organized and continuing close ties to the product and manufacturing sectors of the business." His words essentially tell us that we failed US industry, and his view is probably shared by many others. (As a colleague told me when we were discussing a successful major corporation, "They somehow keep innovation alive in the company, despite the research area.")

For industrial research managers, the distance along the chain from the basic research carried on in universities to the products manufactured in their plants presents an enormous challenge in what might be called the positioning of their R&D organization. At one extreme, focusing solely on solving the technical challenges of the next product means that the research staff have little contact with the university community and are isolated from the knowledge base that is relevant to future products. At the other extreme, the research staff are at APS and Materials Research Society meetings, dedicated to writing papers and isolated from the business of the company—that is, making money from products. Satisfying these two responsibilities simultaneously is difficult in a company of any size, but an inability to do so is perhaps the greatest failure of those of us in industrial research over the past 20 years.

One largely forgotten aspect of industrial research in our field in the US is invention. Worldwide, researchers in our field have in recent years been very good at discovery—discoveries of new materials, phenomena and effects—and the Nobel Prize committee has recognized a number of these discoveries, although few were made in the US. But it is difficult to name a similar number of inventions that we have made or that have received similar attention; the scanning tunneling microscope, which could be regarded as an invention, is a notable exception. It would appear that inventions lead more rapidly to products than do discoveries of phenomena. Inventions are oriented to products, the result of thinking about the needs of a market or customer, whereas phenomena present a scientific challenge. What was the most recent major invention in our field in the US?

Perhaps industrial research should be redefined as the invention of products and services using our interaction with the knowledge base in the universities and our appreciation of market and customer needs. For most of us, the interaction with universities is easy, but the appreciation of market needs is an unfamiliar concept.

The third link in this chain from knowledge to products is the national laboratories. But which link are they? Their role is at present ill defined. In the changing world scene, their prior military missions are becoming increasingly irrelevant. The question is, Are the national labs themselves irrelevant or can they be refocused to make a critical and essential contribution to this chain? If the latter is true, then what is their position in the chain? To do fundamental research that is identical to that carried out in many universities; to do narrowly defined product development under contract to industry; or to have some bridging function in between? Their very name implies they should do research that satisfies a genuine national need, contributes to national well-being, is national in scope (that is, too large in scale to be tackled by others) and is not being done adequately by others (namely, universities and industry). It seems inappropriate, for example, for national labs to compete for research funds with universities and industry. If they do, their role has been incorrectly defined.

I would suggest that there was one example of a successful "national" lab serving the civilian—rather than the military—economy: Bell Laboratories in the 1950s, 1960s and 1970s. It was funded in a unique way, which was one of its advantages, and it reported to a corporation, but it was not allowed to protect its intellectual property, so it was an international laboratory in its scope and impact. Even in the ivory tower of Bell Labs in the 1960s, we never forgot the fact that we existed to serve the telephone business. When Phil Anderson and I observed the Josephson effect, the question was: What is it good for (in communications)? Even though remarkably wide-ranging research was carried out, it was always tied—at some level of management and at least once a year, at budget review time—to telephones.

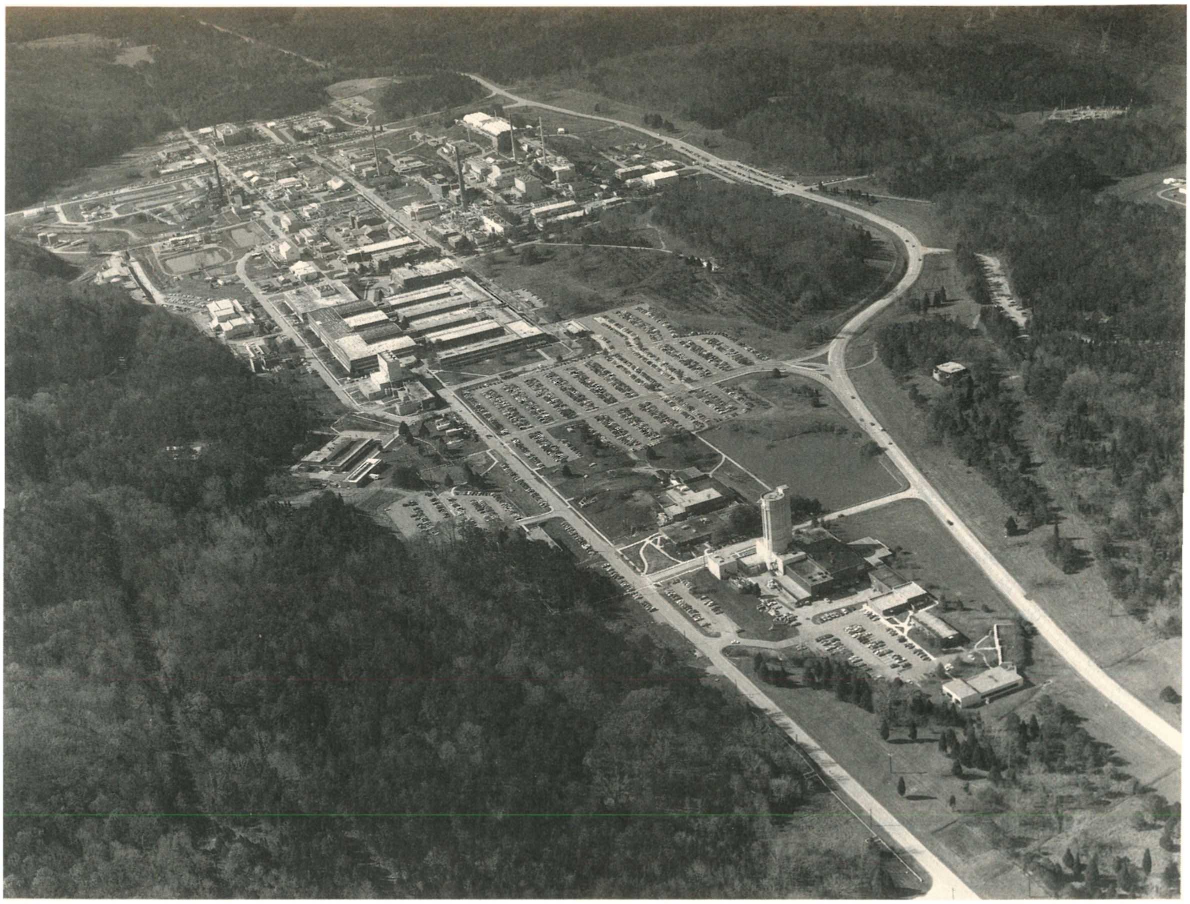
There is a lesson here for today's national labs. They should not try to be all things to all people, to do research that is planned to be useful to all industries, but is more likely to be useful to none, at least on any discernible time scale. I believe that the national labs will only take their appropriate place in the chain when they become industry-specific. Each laboratory will define an industry—not an individual corporation—as a customer, get to know that customer and over time understand its needs. Each lab should focus its research on a selected industry—for example, computing, communications, manufacturing, instrumentation, transportation, education and health. Perhaps Sematech is an example in this regard. In addition, new industries addressing societal needs, such as preserving the environment, should be anticipated and served. One would hope that the growing dependence on foreign oil (yet again) might receive more attention, too.

Thus, I define the eventual role of the national labs as being not the first link in the chain—the creation of the generic knowledge base carried out by universities—but as the next link, which begins to focus that broad base of global knowledge so that it becomes industry-specific and beneficial. The final link—when knowledge becomes proprietary and product-specific—is clearly the responsibility of industry. To some extent, such a role for the national labs will overlap with the university research centers that have proliferated in recent years for microelectronics, optoelectronics, science and technology, engineering and the like. These seem to be attempts to link university research directly to industrial competitiveness, and such work might be better carried out through the intermediate link of a focused national laboratory. However, if this intermediate link has resistive interfaces to both universities and industry, the current of knowledge flow will find a path of lower impedance.

Looking to the future

Several questions, which must be obvious to all of us, arise from my definition of the role of the national laboratories. If US industry has a declining interest in research in its own labs and places decreasing value on our field, will it make use of the national labs in any form? Is US industry going to be a customer for the output of our field, from any source, even it if does not have to pay for it from its profits? If the US continues to evolve as a service economy, will its industry use any laboratories, national or otherwise?

Assuming that the above picture is accurate, what should we do? The basic answer is that we should recognize that we are in a market economy. If the output of research is indeed a commodity in the form of globally available knowledge, then that commodity is subject to the usual forces of supply and demand. All the evidence I have presented and all that I sense as I interact with the field and its customers—that is, its marketplace—convince me that we have created a gross oversupply. Two of



Oak Ridge National Lab, a Department of Energy facility, covers 58 square miles and employs about 5000 people. The national labs, which account for a large fraction of the funding in solid-state science, are now shifting away from defense-related research.

the forces that stimulated supply, the defense agencies and the civilian industrial laboratories tied to manufacturing, are decreasing their demand at roughly the same time. One response is clear: If there is less demand, then the field has to shrink. The excitement of the science alone is not a justification for continued growth. The suggestion from Leon Lederman in PHYSICS TODAY (January 1990, page 9) that "any trained scientist or engineer who is average (B) is assured employment at reasonable wages" makes as much sense as claiming that all the average-quality automobiles that are built will be sold.

Shrinking the field to match supply to present and future demands can be achieved in two ways. The first is painful: As funding difficulties continue, people will give up research in frustration. The second way is for all of us to decide voluntarily to shrink the field. This can be done by every professor's taking fewer students, by the universities' not filling all the faculty positions that become available, by the national labs' not maintaining their size simply for historical reasons, and by industry's being honest about its future need for scientists in our field. Recent events, for example, the merging of the physics and applied physics departments at Yale University, suggest the process has indeed begun.

With our field shrinking, we should also make a serious attempt to increase quality. When demand decreases, quality must be high, as we have witnessed in the automobile industry. At the last MRS meeting, over 300 posters on high-T_c superconductors were presented. In the audience were a few representatives of the companies that are commercializing products based on these materials. But how many of the posters were of sufficient value that they will increase the chance of commercialization? How much of their content can we absorb? The same comments apply to the ten-minute talks of the APS March meeting and are valid across the field.

There is of course another way to redress an imbalance between supply and demand: to increase the demand. The recent brochure entitled "Materials and Technology: The Role of Physics in Materials Research," which was circulated to all members of the APS condensed matter physics division, is an excellent first step. It was accompanied by the suggestion that each member send it to his or her senators and US representative. Perhaps another copy should go to a local representative of an industrial company.

Our field has had a great impact on society, and it will continue to do so. I am most definitely not suggesting that it should disappear, but we must be realistic about its size. I hope we can create an increasing demand for the knowledge output of our field and make an increasingly effective use of that output in the creation of products and national wealth. But if we cannot, then when I see a decline in the number of papers at the APS March meeting, I will know that the field is adapting itself to its present marketplace. I hope it will be done by birth control rather than by famine.

Reference

1. R. E. Gomory, IEEE Spectrum, October 1990, p. 82. ■