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# Fullerenes as an Example of Basic Research in Industry

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# Fullerenes as an example of basic research in industry

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Some recent trends in industrial basic research are considered. They are driven by a highly competitive global marketplace. The discussion focusses on two recent major scientific advances which have been pursued in industry: high-temperature superconductivity and $C_{60}$ with its chemical family. Fullerenes appear to be appropriate candidates for basic research in industry.

This paper might be subtitled 'Research on a Globe, Chemistry on a Sphere', referring to two very different aspects of the chemical community involved with fullerenes. Research worldwide is undergoing major transformations. Although noticeable in academic and government laboratories, the changes are larger in the industrial sector, especially in companies that have been major centres of basic research for decades. Here we shall consider some of the more prominent developments in industry.

The second aspect is the extraordinary chemistry of $C_{60}$ and the rapidly increasing family of carbon structures. The chemistry will be that developed at Du Pont; the work of Dr Fagan, Dr Krusic and Dr Tebbe with others. Taylor's contribution in this collection provides an extended consideration of some of the topics and will not be repeated here. Rather we focus on a few features of chemistry on a sphere as opposed to the planar or linear compounds with which we are more familiar.

The bridge between these two parts, and a conclusion, is that basic research on fullerenes may well be an appropriate activity for a modern company dealing with chemicals or materials.

Changes in research philosophy and practice have occurred throughout technologically based industry in recent years. As companies position themselves for a competitive, fast-moving marketplace the fundamental importance of research and technology is rarely questioned. But there is continual discussion as to what research should be done, how the research should be carried out, what are the proper roles of industrial, governmental and academic laboratories, and how the results should be transferred and developed for the commercial realm.

These questions are not new. At Du Pont similar ones were being asked at the beginning of the century and have reappeared every ten to twenty years. They are with us now. With changing circumstances, both external and internal to the corporation, the answers are also changing.

Some of the more frequent comments throughout industry are the following.

1. *Build on company strengths.* Now we are less likely to develop a new business from research as man-made polymers and transistors were created in decades past. Pharmaceutical companies continue to produce novel products from their base in

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molecular biology and chemical synthesis. But they focus on areas closely related to those they know well rather than branching into fundamentally new fields.

2. *Faster progress in research.* The increasing cost of research in industry often outpaces consumer-oriented indices of inflation. There is a continuing demand for more rapid developments. This increased rate requires that old habits be changed, organizations more tightly structured, collaborations more effective and movement to market faster than previously.

3. *Shorter programmes.* While related to 2, this requirement highlights the penalty of greater length. We seek 3–5 year programmes rather than 10–15 years. The extended programme can become obsolete before completion. Personnel movement between companies together with the information available in patents and publications puts the competitive success of a longer programme at risk. Sometimes there is an advantage being the second to enter the race, avoiding much of the time-consuming problem-solving required of the pioneer. The increased pace is not unreasonable as new tools, often analytical, allow completion of a programme in less time.

It is not appropriate here to comment on these new rules except that they are consistent with the competitive global marketplace which must be considered in planning industrial activities. My guess is that by the end of the decade we shall be seeing a return to longer term fundamental research in industry but in areas different from those that are now popular. But that is another story.

For now I should like to consider two fields in which recent breakthroughs in fundamental science have led to major efforts in industry and examine some critical differences between them. One, of course, is the fullerenes and we shall return to that below.

The other is high temperature superconductivity which began with the report of Bednorz & Müller (1986). Within a year the upper limit of $T_c$ went from 23 K to 125 K an extraordinary accomplishment given that sixty years had been required for $T_c$ to rise from 4 K to 23 K. Within weeks of the first discoveries popular accounts discussed the wonders that might be possible in energy saving and production together with major changes in society that could be envisioned. While important scientific advances continue to appear, the commercially significant applications are still many years away. The gaps between possibilities and accomplishments has led some to question our ability to judge the likely benefits of basic research for industry. Of course, those of us who have been involved with such research we are well aware of the surprising turns, both positive and negative, which can occur. We emphasize the positive as we must; we are among the best qualified to see possible advantages. But the qualifiers initially included with the conjectures are often lost. The apparent credibility gap can lead higher management to wonder about the level of support appropriate for more fundamental research programmes.

Superconductivity highlighted another complication for basic research with possible industrial applications; the outstanding quality of laboratories around the globe and the consequent high level of competition. With many organizations starting with similar levels of expertise the probability that the efforts of one will lead to a commercially successful project is small. The sheer numbers involved reduce the chance that a given company will win the race. Also, proprietary rights can be confused as different companies and universities contribute to the development of a single product.

In earlier times, in some of the classic cases of fundamental research leading to





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successful new businesses, technical competition was much less. For polymer research at Du Pont in the late 1920s and 1930s industrial competition was centred at I. G. Farben in Germany. The programme under Wallace Carouthers at Du Pont was able to set its separate direction producing polyamides and neoprene for the marketplace while developing the fundamental science which established polymer chemistry as a rigorous intellectual discipline.

Similarly, Bell Laboratories pursued the transistor before and after World War II when solid state physics was not heavily populated. Bell could set its own pace in reasonable confidence that they would have a strong proprietary position. There were also features unique to the Bell System that are unlikely to be duplicated in today's competitive marketplace. Bell was a regulated monopoly with its profits guaranteed by government regulation. In addition, the System's internal needs provided a large market for any new devices.

Another feature is associated with the high level of activity in basic research throughout the industrial world, namely considerable duplication and consequent inefficient use of resources in a suddenly fashionable area. A given experiment may be carried out simultaneously in many laboratories with only a small probability of a worthwhile return from the individual effort.

One example, more extreme than most hopefully, occurred at a meeting of the Material Research Society a year after the initial explosion of interest in superconductivity. During a symposium mention was made of a recently published experiment reporting an increase in $T_c$ by treatment of the solid superconductor with a gas. The chairman asked how many members of the audience had repeated this experiment. Fifty hands were counted out of a total of 450. It is likely that others, not in the audience, had also tried. Almost all of this effort is an unproductive use of the resources alloted to the field. Little appeared in the literature as negative results with such complex solids are rarely definitive and publishable. But remedies are not readily available.

In many countries the freedom of the individual investigators to decide the details of their research programme is well protected. Control of day-by-day activities is inappropriate. Nevertheless, in cases such as the above, a preferable procedure would be for two or three selected laboratories to attempt confirmation of the claim. They would then communicate the results to others. Again this massive duplication may be regarded as a corollary of the large number of well-staffed and well-supported research laboratories around the globe.

Another feature of the superconductivity effort has been that most has been devoted to detailed studies of existing materials rather than wide-ranging searches for new compositions. Substantially enhanced properties are necessary if high temperature superconductors are to have a major impact in the commercial sector. The assumption has been that detailed understanding will lead to improved superconductors. Usually, this has not occurred. The complexities of these underdetermined solid state systems restrict the ability to design improved structures.

The McCall committee on high-temperature superconductivity reported that some two years after the breakthrough in the field roughly US$1000 M worldwide was being spent on research and development while the total market for superconductivity of all types was but US$250 M. Of course the research and development expenditures are in search of new capabilities and markets. While larger markets are possible the area is likely to be unprofitable for years to come.

High-temperature superconductivity initially attracted more than a thousand participants globally, but after one and a half to two years many left the field. Much of that migration occurred because the near-term commercial opportunities appeared limited. While the science remained fascinating and was an extraordinary advance in superconductivity there were fewer major discoveries. The theory of the phenomenon and the related normal state of these unusual materials continues to attract the attention of some of the best solid-state theorists. We can expect major additional scientific developments in coming years.

The shrinking effort in industry has led to smaller, more focused programmes on a scale appropriate to potential markets. The activities have comparatively modest goals. A number of products should appear in the near-term, primarily in electronics with fewer in energy-related fields and transportation.

In contrast, fullerenes after two years of effort are continuing to attract more practitioners to the field. Related areas of research are appearing such as giant and nested fullerenes as well as buckytubes. Such growth and branching is a sign of a developing discipline.

One of the great attractions of fullerenes is the ability to apply to $C_{60}$ much of the olefin and aromatic chemistry already developed with traditional organic compounds. This activity is still in its early stages. One of the earliest accomplishments was the use of organometallic chemistry by Paul Fagan to demonstrate that $C_{60}$ could react as an electron-poor olefin similar to tetracyanoethylene.

Duplication in fullerene chemistry is substantially less than that found with high-temperature superconductors. Several high temperature superconductors are easily made. The science involved in purifying, characterizing and reacting fullerenes can be demanding. At times $C_{60}$ can be as recalcitrant as a piece of coal. It may be the purest form of carbon before reaction but the products are often impure derivatives difficult to separate. The well-defined compounds that have been obtained often precipitated from solution under equilibrium conditions as with Fagan's platinum complexes and Tebbe's $C_{60}Br_{24}$. These can easily be redissolved and transformed.

While the ability to relate the chemistry of $C_{60}$ to that of other organic molecules has been beneficial for many studies, the novel reactivity of the spherical form is of particular interest. The quantitative thermal decomposition of $C_{60}Br_{24}$ to $C_{60}$ and $Br_2$ is one example with little precedent in traditional reactions. Another is the observation by Paul Krusic that up to thirty-four methyl radicals can be added to the fullerene. This holistic behaviour is promising for other novel chemistry.

As a field of basic research in industry fullerenes are attractive as there are a number of possible applications. Some of these could provide new business opportunities. In the short term extensions of existing businesses and variations of existing products are likely to be the focus. It is difficult to discuss specific areas here as present programmes are still primarily concerned with fundamental understanding. Applications will have to wait on that understanding.

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