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Freedom and fashion in materials science and engineering

ROBERT W CAHN

Department of Materials Science and Metallurgy, Cambridge University,
Pembroke Street, Cambridge CB2 3QZ, England
e-mail: rwc12@cam.ac.uk

Abstract. This paper briefly analyses the rate at which a new scientific discovery is taken up around the world and further pursued, and the factors that govern such take-up, with special focus on freedom and fashion.

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1. Introduction

The first papers by the Braggs, father and son, on simple crystal structures determined by X-ray diffraction, appeared in 1913. In 1917, the older Bragg contributed a chapter on current physics research to a multiauthor book, but modestly included not a word on X-ray diffraction. As late as 1928, an editorial by P Groth in the venerable *Zeitschrift für Kristallographie* again did not include any recognition that it was now feasible to determine crystal structures experimentally. Only around 1930 could it be claimed that X-ray diffraction had become a fashionable line of research.

By way of contrast, the invention of the scanning tunneling microscope (STM) was made public by Binnig and Rohrer of IBM in 1983, and the technique was promptly pursued by both scientists and engineers throughout the world. The STM became fashionable very quickly indeed and spawned numerous subsidiary techniques.

It is reasonable to enquire: Why the difference? Clearly, e-mail, the worldwide web and the growing habit (encouraged by these tools) of joint research between investigators on opposite sides of the world, must all have contributed. But I believe the matter goes further than this: scientists and engineers towards the end of the 20th century were much quicker than their predecessors to recognize the potential scope of a new experimental technique and were thus ready to examine it when its umbilical cord had hardly yet been cut. In other words, they were ready and able to *pounce on new fashions in research*.

For large numbers of investigators to pounce in this way on a new technique, observation or hypothesis was only possible if that subset of the technical population enjoyed substantial freedom to select their own themes of research. Here we have a paradox: older investigators often bewail the alleged regimentation characteristic of present-day research, particularly in industrial laboratories, compared with the greater freedom they claim was present half a century ago. Yet the “pouncing phenomenon” belies this orthodoxy. What is going on?

2. Freedom and choice

One of the under-researched aspects of the history of science is how research scientists choose the field(s) of their life's work. If, while they are still students, they are known to be highly capable, they will probably have some choice as to who their doctoral supervisors will be, though once a student has signed up with a supervisor, the latter will usually determine the initial theme of the doctoral research. (This is very different from what usually happens in the humanities, where the normal thing is for the student to choose a research topic and then to look for an academic supervisor who is willing to look after him.) If the science student is really able, he will usually find new and distinct aspects of the theme imposed on him and by the time the thesis is written, it may cover a range of topics different from what the supervisor initially had in mind... that is, if the agency financing the research is tolerant about the interpretation of its contract with the supervisor. (Here and later, "he" includes "she".)

There is then a bifurcation among new PhDs. Some cling for dear life to their newfound expertise and try to carry on researching the same narrow subject for as long as they can–sometimes for a lifetime–others recognize that to be rounded researchers, they need to broaden out their fields of competence. Usually, the broadeners become the famous scientists. (An example: Albert Einstein is often regarded as wholly focused on relativity, special and general, but in fact his first famous papers were devoted to Brownian motion and to the photoelectric effect, which have nothing to do with relativity.) Also, when the convinced broadeners become heads of research groups themselves, they will in the nature of things keep their eyes open for promising new problems, while trying to relate such problems to those matters in which they are already experts.

Whether the head (or even a relatively junior member) of a research group can pounce on a new problem or technique will depend on his situation, and on the ethos of his institution and indeed of his country. Half a century ago, a number of industrial research laboratories, especially in America, were guided by enlightened directors in such a way that their best researchers were free to follow their judgement and curiosity, within quite wide limits. Bell Telephone Laboratories in their heyday were famous for this, and yet the transistor evolved there through the sustained strength of will of a remarkable research director. Freedom and guidance found their optimum balance in the best laboratories. In the 1970s, very firm direction of industrial research groups became the orthodoxy, and this inhibited the "pounce factor".

An excellent portrait of a "free laboratory" in the 1950s and 1960s is in an essay about the GE Corporate Laboratory in the USA, by two of its former research directors (Suits & Bueche 1967). A subtle and critical history of research policies in another US company, Corning Glass, over many decades, showing the price as well as the benefits of close direction during part of that period, is to be found in a recently published book by two management experts (Graham & Shuldiner 2001). Corning, in particular, is famous for cherishing its best researchers and according to them an unusual freedom to follow their hunches, backed by management; that policy has repeatedly saved the company at critical junctures.

In spite of the gradual drift towards increasingly firm direction of industrial research laboratories, there have been numerous episodes, in recent years, of pouncing on new discoveries, followed by a massive explosion of publications. Nobody has assembled statistics of these occasions, but my impression is that the majority of the groups which dropped what they were doing to pursue new leads were in universities. It is often possible for such groups to rely on long-term funding for the 'pouncing research', or to undertake some initial research cheaply without seeking new funding to begin with. Yet in some instances, especially those in which research quickly became ultra-expensive, pouncing remained an industrial prerogative:

the development of integrated circuits is the prime instance. A few examples of novel topics which stimulated copious research around the world as soon as they became known are electron microprobe analysis, metallic glasses, rare-earth permanent magnets, high-temperature (ceramic) superconductors, quasicrystals, superhard carbon nitrides, thin-film diamond synthesis, lithium-based high-capacity batteries, giant and colossal magnetoresistive materials, nanostructured materials. The last of these proved such a popular theme that it deserves a separate discussion.

3. Nanostructured materials

In 1981, the German materials scientist Herbert Gleiter gave a lecture at a symposium in Denmark about the predicted mechanical properties of polycrystalline materials in which the crystal grains are only a few nanometres across, as contrasted with normal grain sizes around a thousand times larger. The lecture was published in the proceedings and caused considerable stir, especially because it was soon found that such materials can often be superplastically deformed by the Nabarro–Herring mechanism (migration of vacancies between grain boundaries differently oriented with respect to the applied stress)–even when the material is normally, in the coarse-grained form, wholly brittle, as for instance a ceramic like TiO_2. Soon afterwards (Gleiter & Marquardt 1984), nanostructured materials were made by evaporation into a noble gas at high pressure, followed by compaction of the ultrafine powder without intermediate exposure to air. This process is fine for research but not for commerce; for large-scale production, a range of chemical methods has since been developed, some starting from colloidal organometallic sols. One of the paradoxes in using nanostructured polycrystals is that at the moderately high temperatures used for superplastic forming, rapid grain growth is to be expected, but in fact some forms of treatment have been discovered which impede such grain growth. One of the most intriguing is the use of dopants which segregate to the grain boundary and can under certain circumstances destroy the driving force for grain growth.

Since the 1980s, a huge amount of research has been done on nanostructured materials. One example is the use of nanostructured tungsten carbide, made by a chemical process, to produce ultratough Co/WC cermets. Extensive studies of self-assembly of nanosized constituents have developed, especially in the laboratory of George Whitesides at Harvard. However, recent work has often focused on functional materials, such as porous silicon and quantum wells of various kinds, and also on the quite separate study of ultrafine powders (not compacted), which are of interest to physicists (Andres *et al* 1989), for instance because their crystal structures and melting temperatures can be quite different from those of bulk materials (Edelstein & Cammarata 1996). Another recent development of great importance (starting in Japan in 1988) is the discovery and improvement of materials with superb soft ferromagnetic properties, nanostructured crystallites dispersed in an amorphous matrix; this large subfield has recently been well reviewed in India (Gupta 2001). The originator of the whole adventure, Gleiter, has recently surveyed the field as it is now, hardly recognizable as what he modestly proposed 19 years earlier (Gleiter 2000). What was initially a circumscribed, clearly defined field of research has become a huge, hazy combination of barely related activities, and it would be hard to find a better example of scientific fashion in overdrive.

4. Fashion

The word ‘fashion’, when used in a literary context, conveys an impression of arbitrariness: the changing length of women’s skirts (perhaps not in India), the adoration of conceptual 'art', pop music in vogue one month and forgotten the next, Bollywood films all the rage for a short time. There can be an element of such arbitrariness in scientific research also, though the half-life of a scientific fashion is apt to be very long compared with that of a piece of pop music. The topics listed at the end of § 2 can all be described as scientific fashions, but all of them (except perhaps carbon nitrides) have lasted many years and have not yet fallen out of favour. The longevity of such scientific fashions suggests that there is much less arbitrariness about them than there is in women's fashions in clothes; nevertheless some scientific fashions, such as polywater and cold fusion, have come and gone rather quickly, killed by ridicule and disappointment.

Just how seriously the inflated fashion of nanostructured materials should now be taken is still unclear. Gleiter, in his 2000 review, has this to say: "The remarkable potential the field of Nanostructured Materials offers in the form of bulk materials, composites, or coating materials to optoelectronic engineering, magnetic recording technologies, micro-manufacturing, bioengineering, etc. is recognized by industry. Large-scale programs, institutes and research networks have been initiated recently on these and other topics in the United States, Japan, the European Union, China and other countries." Gleiter himself has become Director of a successful Institute of Nanotechnology in Karlsruhe, Germany. Just as this paper is being written, the National Science Foundation in America has announced awards "estimated to total $ 65 million over 5 years to fund major centers of nanoscale science and engineering" at six universities in the US. Also just published is a remarkable paper from the Netherlands (Bachtold *et al* 2001) which describes the use of the semiconducting variant of carbon nanotubes to design exceedingly compact logic circuits, using individual nanotubes as components of transistors.

The current fashion of nanostructuralism has been dissected in a long, rather critical book review by an Oxford engineer, P J Dobson, in his review of a recent book, *Nanotechnology*, edited by G Timp (review by Dobson 2000). The publishers of the journal in which the review appeared have counted the number of 'hits' on the journal's website over a year and found that this modest review was one of the most popular of all the papers and reviews published in that year. This suggests a measure of uncertainty among many observers about the prospects of the field. Dobson remarked: "What are the issues in 'nanotechnology' and how are they best addressed? The problem can be approached from two very different angles. One could take the view of expediency as in the case of electronic circuits, in which size reduction enables more transistors to be put onto a wafer of silicon that operate faster and consume lower power by virtue of their reduced size. Or, one could examine some of the new physical effects that are size-dependent and ask how these might be exploited for useful purposes. An example of the latter is the use of the 'quantum well' lasers that are already used in most CD players . . . ". He goes on to criticise many of the chapters and praise a few (like the chapter by Dresselhaus on the nanotechnology of carbon materials), and he asks: "What are the objectives of all this 'nano' activity? Some, such as the quest to make computer processors smaller, faster and less power-hungry are fairly obvious. Are there any other such obvious benefits? Most books presently available on nanotechnology do not really address this question well." He goes on to criticise physicists active in the field: "Far too little attention has been given by physicists to the possibilities offered by chemistry in making novel nanostructures".

I have no doubt that the very large sum allocated by the National Science Foundation will mostly lead to valuable results – and may just possibly generate something really spectacular – but it is always a good idea to apply the brakes of moderate scepticism when a fashion diverges very rapidly. That way, valid enthusiasm is tempered by proper caution.

Fashion has also occasionally led to bursts of experiment promoted simply by the availability of a newly perfected experimental technique, without consideration of what purpose the experiments can serve. An example, in which Indian groups have joined together with groups in several other countries, is the measurement of crystallographic stacking-fault densities in intensely cold-worked face-centered cubic metallic solid solutions, as a function of solute concentration and therefore of valency electron/atom ratios. The cold-working was imparted simply by filing with a fine file. The technique applied was developed by diffraction specialists in America in the 1950s: the presence of stacking faults had been shown to generate small shifts of certain X-ray powder diffraction lines. Paper after paper was published based on the purely mechanical application of this technique, without anyone ever asking: "So what?" The findings did not relate to any other property of the solid solutions, and thus did not 'radiate' beyond their immediate neighbourhood, as good researches always do. The moderate scepticism I mentioned in the preceding paragraph can help to counter bare fashion by asking the crucial 'so what?' question.

Finally, fashion in science is sometimes complemented by antifashion. Thus, in the early years of the 20th century, up to the end of the 1930s, semiconductors were held in vociferous contempt by many (indeed, most) physicists, on the grounds that they were apt to be dirty (i.e., full of impurities) and their properties irreproducible (Cahn 2001). Alan Wilson in Cambridge, who around 1930 undertook the first serious theoretical study of semiconductors, was warned that he was putting his reputation as a serious physicist at risk. Still later, Isidor Rabi at Columbia University in New York vowed that his physics department "would never occupy itself with the physics of dirt". He said this at just about the time that the transistor was being invented in the Bell Laboratories. The turnaround for semiconductors, from antifashion to fashion by the end of the 1940s, was spectacular. Dirt became dopants, the disagreeable became indispensable.

5. Conclusion

This short paper is about fashion and freedom, and their consequences. The reader will have detected that I have slight reservations about the pursuit of fashion (and where pop music is concerned, my reservations are not slight), but none about the primacy of freedom. To get the best from new scientific developments it is essential for the best researchers everywhere to have a substantial measure of freedom–of course that can never be absolute. That is my conclusion.

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