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FASHIONS IN SCIENTIFIC RESEARCH: The interactions between scientific curiosity and industrial needs

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Abstract

This paper starts by considering the factors that determine fashions in fundamental scientific research in the field of physics and discusses the interaction of such research with industrial needs. It emphasises the importance of pure research both within industry and outside and suggests that this should be regarded by industrial accountants as an issue of `enlightened self interest´ for industry itself.

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It is a great honour to be invited to give the opening address to this distinguished symposium on Fundamental Research. You must pardon me if I begin with a small correction. I find that on looking through my manuscript I have given it a rather fuller title which is, perhaps, indicative of what I meant to talk about: I called it `Fashions in Scientific Research: Interactions Between Scientific Curiosity and Industrial Needs.´

One of the most fascinating issues concerning the development of science is the problem of what determines fashions in science and in scientific research. In this talk I shall deal primarily with physics. In the early days of science when even the methodology had not yet been established, it was easy to discern certain scientific activities that were clearly influenced by the interests of surrounding society. I do not intend to give you a historical survey from the early days, but one or two points are quite obvious to us. For example, the study of astronomy is amongst the oldest examples, particularly in those superstitious days when astral-influences might determine the destiny of kingdoms. Of course, our daily horoscopes show little advance from this tradition. The study of geometry must have been encouraged by the need to determine shapes and areas of land for the proper construction of houses, palaces and temples. Yet as late as the Middle Ages the record of collapsing cathedrals indicates that basic studies had not yet gone far enough, and in our own day disasters that have overcome certain box-type bridge structures show that research does not always keep in step with practical requirements.

The simple art of weighing goes back as far as recorded history and some of the features have left their mark on our culture to this very day. I may remind you that in jewellery the unit of mass is based on the weight or mass of a carob seed and, in fact, derives its name from carob, which has been corrupted to carat.

I suppose that lots of us take it for granted that science as we understand it today received its first and most powerful impulse from the Greeks. They were marvellous pioneers of rational thought which they applied with great originality to philosophy and to certain aspects of scientific analysis. But their scientific ideas, though wonderful, were largely speculative. Unfortunately, in the field of physics Aristotle and the authority of his word became a hindrance rather than a help to scientific development. It is, indeed, strange to consider the time gap between, say, Aristotle and Galileo and the rather arid period that lay between them. It may of course be that in the broader terms of human history, this is no more than a hiccup in time. Maybe our time scales are wrong and 1,000 or 1,500 years in the development of science may be nothing in terms of the way in which our general ideas and concepts have evolved. Yet the developments since the Renaissance, especially during the last 300 years, have been so enormous that the previous millennia seem by constrast to be a vast scientific wilderness.

Science and technology and the arts were all mixed up with one another in the earlier part of the post-Renaissance period. The great European Academies of Science, established three centuries ago, had distilled physics and chemistry from astrology and alchemy but they were still very much involved in everyday practical affairs. Two-hundred years later, when Clerk Maxwell was involved in the construction of the Cavendish Laboratory, he not only applied his ideas of acoustics to the design of the lecture theatres, he actually produced drawings in his own hand of the gear wheels that would open the lecture room shutters. Such a combination would be unusual today amongst professors of physics, for as scientific knowledge has grown, specialisation has increased. Of modern scientists Fermi must be considered as one of those unusual individuals who could, on the one hand, develop a new form of statistics needed for describing electrons and on the other hand, work on a lathe and fabriate sensitive apparatus for experimental work.

During the 19th century steam engines and thermodynamics, electromagnetic induction and electric motors, diffraction and microscopy were interactive activities, but Maxwell's ideas of electromagnetic waves had little connection with practical affairs although it was only a matter of a few decades before they found their practical application in radio communication. On the scientific front Maxwell's research stimulated optical studies and measurements of the velocity of light which showed that the ether was far more ethereal than Maxwell had believed. With Einstein's publication of the simple theory of relativity the ether virtually disappeared. With this development and Planck's idea of energy quanta, the modern era of theoretical physics begins.

Even within these revolutionary domains we are often able to see the scientific influences that led from one idea to another. But I find it hard to believe that at that stage these ideas emerged from interactions with industry or society, or even from military requirements. Einstein's concept of the equivalance of mass and energy is the by-product of his ideas of space and time and owes nothing to nuclear energy or atomic bombs.

Indeed, the whole of the development of what we might call modern atomic and nuclear physics during the first 35 years of the present century was one in which science appeared to flourish for its own sake. There were certain developments in electrical engineering and radio communications that were stimulated by industrial and military needs and in these areas one could see the first clear interactions with pure research.

Again, in chemical research, there were enormous developments that were stimulated by industry and, to a lesser extent by military requirements; but in physics the basic concepts of wave mechanics, the Heisenberg uncertainty principle, the Schrödinger equation, the structure of the nucleus was pure physics, untouched by the world outside. That period which Kapitza rightly referred to as an age of scientific innocence, ended with the second World War.

Not only was science used in a ruthless and terrifying way to prosecute war, the interaction with industrial and societal needs became evident and powerful. The two most striking examples are known to all of you: nuclear energy and the transistor.

The energy released under favourable conditions by the nucleus led to the development of the atomic bomb and to the prospects of an energy source independent of fossil fuels. The transistor gave enormous impetus to solid-state studies in physics. This, in turn, generated a world-wide industry in electronic gadgets, computers and micro-chips whose influence extends from automation in industry and military devices, to artificial intelligence and managerial techniques in commerce.

Yet sometimes the lag between scientific research and industrial needs is quite bewildering. By 1936 we knew far more about the atomic nucleus (although of course nuclear energy and atomic bombs were still undreamt of) than we knew about the strength of solids. Dislocations and the understanding of plastic deformation was a relatively neglected field and although one might argue that dislocations had to await the technological development of the electron microscope this is not the whole story.

There is little doubt that the best physicists were engaged in unravelling the electronic structure of matter and the basic constitution of the nucleus. To this day, for example, the problem of fatigue, which is amongst the most important long-term problems of an industrialised and mechanised society, attracts only a fraction of the effort and talent that goes into modern solid-state physics.

One of the newer areas of solid-state physics is the study of solid surfaces using all the new techniques that are now available, involving ultra-high vacuum systems, various types of spectroscopy, and several varieties of diffraction. Numerous laboratories at universities and in industry are studying the structure of clean solids in order to understand the arrangement of surface atoms and the electronic states of the surface.

This is now a growth area in physics research and is gradually attracting the best talents hitherto engaged in solid-state and semi-conductor research.

One reason for this may well be that solid-state research has reached a plateau and new fields must be explored, while another is the enormous industrial interest in surfaces and surface reactions.

These fall into two classes.

The first deals with problems of corrosion and although most applied research in the field involves `wet´ chemistry the new surface techniques provide unique methods of identifying surface species and surface products.

The second concerns the action of catalysts.

With the impending exhaustion of oil and petroleum the chemical industries are desperately searching for new and more efficient methods of chemical synthesis. The enormous contribution made in earlier years by classical physical chemistry now needs buttressing with new information and new understanding of a much more detailed nature. This it is hoped will be supplied by the newer studies of surfaces and their interactions with vapours and gases. The interplay here between research and industrial needs is clear and direct.

Another area of clean surface research that should be mentioned, because I myself am involved in it, is concerned not with the interaction of vapours with solids but of solids with solids. I refer here to the deformation and adhesion of clean solids when they are brought into atomic contact. This is of first rate importance to our understanding of friction and wear, of electrical contact, and of the adhesion of thin films to substrates as used in modern electronic devices.

In contrast to solid-vapour research in which hundreds of laboratories are engaged, the number of basic research laboratories studying solid-solid interactions scarcely runs into two figures. Is it more boring? Is it more difficult? Is it less profitable? Is it primarily a question of funding?

Certainly, if all research were funded solely by industry the discrepancy could be understood, because the amount of funding being provided by the large industrial companies and by industry for the development of better catalysts is quite overwhelming.

But in most western countries there is always a substantial background of funding from government sources, so that at least at the university level the scientist has a much freer hand in deciding his area of research.

I think that one of the factors involved here is the following.

The huge expansion in research facilities during the last 30 years has created large and powerful research groups, that drag other less original workers along with them.

I do not mean to imply simply that scientists jump onto a bandwagon, but rather that they are often railroaded into following a certain line of research.

A closely linked additional factor is the time lag in our higher educational institutions. The science that is taught to our students reflects quite markedly the interests of the teaching staff. In this way they influence the type of research projects that students consider to be significant and up to date. And once a particular area of research exceeds a certain size in world science the whole field acquires a personality of its own and exerts its own influence on its practitioners. We not only create our own scientific environment, we are also influened by it. To some extent we thus become the prisoners of our own expertise and specialisation.

Then the most gifted research students become the Professors of the next generation so that a new idea may be continued beyond the stage at which it is innovative or fruitful.

There is no reason why scientists should not indulge in their preference for fashionable fields. It leads to greater progress, stimulation and a feeling of commitment to a world-wide activity. The only major drawback is that it draws away research effort from less fashionable fields.

Ten years ago I visited a small university with an excellent Physics Department. After a tour of the laboratory I noticed that all its research concentrated on electrons and phonons in the solid state.

Speaking later that day to one of the more senior staff members I played the part of Devil's advocate and asked him if he had ever considered working on the liquid state of matter. I expected him to say that it was too difficult or that it was a bore or that he was too old to change. This was not his reaction: instead it was something bordering on horror. "But nobody", he responded, "nobody is working on liquids".

That it is not even true is not important: his response indicates the extent to which most of us are imprisoned by our own scientific environment.

There are various areas of research today that have swallowed up very large fractions of the talented manpower available:-

Nuclear research, which also swallows up most of the funding,

radio-astronomy, which does pretty well in that direction,

laser research,

solid-state research, including electronic devices.

In the next 20 years a major growth area, as I mentioned earlier, will certainly be surface science. I would guess that a new area that will soon emerge will be a sophisticated attack, both theoretical and experimental, on the structure, morphology and properties of polymers, carrying our present knowledge far, far forward. All this can surely only be for the good as far as the paper industry is concerned.

Of course, the value of high quality fundamental research depends on the effectiveness of the link between the basic study and its application to the real problems in industry. This is a problem of communication and the quality of the men involved.

In my own case my experiences with the paper industry have been particularly felicitous.

My first research, which had nothing to do with wood or paper, was concerned with the basic mechanisms of friction. We found that in general interfacial adhesion was a major factor.

Wherever the sliding surfaces came in contact there was atomic interaction; bonds were formed and these had to be broken during sliding (Figure 1a).

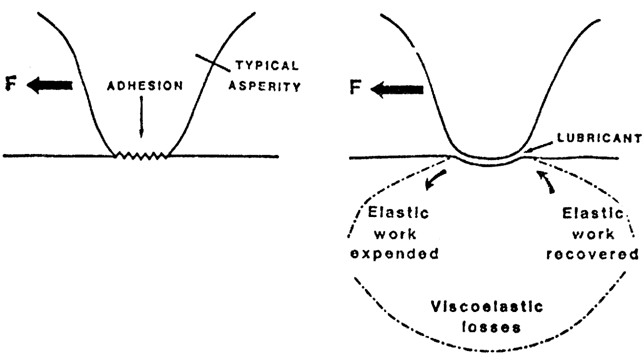

Fig 1— **(a)** Dry sliding: friction is dominated by interfacial adhesion.
(b) Lubricated sliding: friction is dominated by deformation processes. With polymers and rubbers elastic hysteresis or visco-elastic losses are the primary cause of energy dissipation.

However, under conditions where the adhesion is greatly reduced, for example in the presence of effective lubrication, the major part of the friction arises from the deformation of one surface by the other.

Consider for example the sliding of a hard asperity or slider over a deformable solid such as rubber. If the interfacial adhesion is extremely weak, then as the asperity slides forward it does elastic work ahead of it while the deformed rubber at the back half of the contact zone recovers and urges the slider forward (Figure 1b).

If we recover as much energy as we put in then we do not need to expend any work in order to move the slider along the surface. But because the solid is a real material there will be energy dissipation by visco-elastic or hysteretic losses and this will account to a large extent for the observed resistance to sliding. It is this idea, for example, that led to the suggestion that the skid resistance of tyres could be improved by coating the tread of the tyre with high-hysteresis loss rubber.

At the time I was carrying out this work Douglas Atack came to spend a few months in our laboratory to study the friction of wood.

He found, in parallel with our work on rubber, that a large part of the friction is due to deformation losses in the wood as the hard body slides over it, and that this fraction beomes larger and larger the more effectively we reduce the adhesion between the surfaces. Furthermore, it turns out that the deformation energy is dissipated at a small well-defined depth below the region of contact: the region of maximum shear stress.

Dr. Atack at once saw what important implications this has to the grinding of wood to produce wood fibres.

The whole of the process is flooded with water to give effective lubrication and the energy dissipated by each grit as it goes over the wood leads to a temperature rise in the wood a short distance below the surface.

By choosing the grit size, the average loading, the grinding speed and other variables, it is possible to optimise the conditions such that the temperature rise produces softening of the lignin at a depth just about equal to the diameter of a wood fibre (Figure 2). The fibre is thus loosened and easily plucked out. I believe that work along these lines did in fact lead to an improvement in pulping procedures.

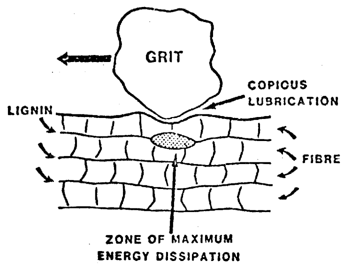

Fig 2—Grinding of wood in presence of copious lubrication. Region of maximum energy dissipation leads to heating and softening of lignin just below surface.

The second bit of work that I got involved in and that has some bearing on the wood industry (although it had no such original intention) concerned an extension of our earlier frictional studies to a more detailed investigation of the deformation losses in polymers.

It seemed to us that at the region of frictional contact the material is under high local pressures so that its loss properties might be substantially different from those measured at atmospheric pressure. We therefore decided to study the effect of pressure on the mechanical losses in polymers.

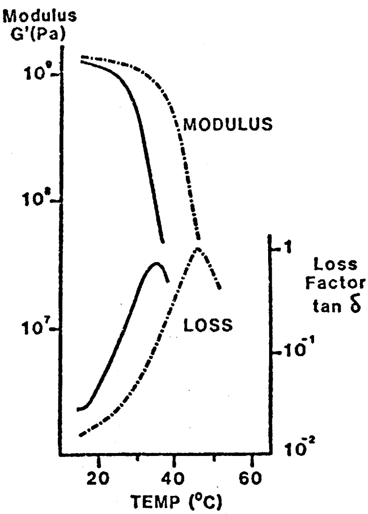

Fig 3—Effect of hydrostatic pressure on the glass transition of polyvinyl acetate.
—— Atmospheric pressure
––– 610 atmospheres
Tg is increased by about 20° per *K* bar.

Amongst various features that we observed was the very striking effect of pressure on the glass transition temperature Tg. This is shown schematically in Figure 3 for polyvinyl acetate. At atmospheric pressure Tg is approximately 35°C and at this temperature there is a marked drop in modulus and the losses are maximal. When a pressure is applied both these features are shifted to a higher temperature. The glass transition temperature is increased by roughly 20°C per 1000 atm. It so happened that while we were involved in this work, we enjoyed a short visit by Douglas Atack who saw in these results important possible implications in relation to the high-temperature, high-pressure fibrillation of wood. By a small change of pressure, he argued, it should be possible to work either just above or just below the glass transition temperature of the lignin in the wood. In this way it should become possible to control the nature and quality of fibres produced.

Later research showed, however, that the effect of pressure was too small for this to be used as a practical parameter. However, it did indicate that by changing the temperature over a small range, a thermomechanical process could be developed which could produce fibres of controlled characteristics.

I understand that this process has proved successful, so that I might almost regard myself as an honorary consultant and adviser to the pulp and paper organisation of Canada. But, of course if any of my work in this field has proved useful at all, the major credit must undoubtedly go to Douglas Atack.

My third bit of work which might be of interest to the paper industry concerns diffusion in polymers.

This arose out of an earlier observation that in the manufacture of polythene bags, sheets can stick together unless a small amount of surface-active material is incorporated into the polymer; this diffuses to the surface and prevents the insides of the bags from sticking together.

We decided to study the way in which these surface films were formed and how they influenced the friction and adhesion, and as a consequence of that research we decided to study the diffusion process itself.

The first thing we observed was that diffusion of longish organic molecules can take place only through the amorphous part of the polymer.

The second was, that as soon as the diffusant exceeded a critical length, above say 20-carbons, the mode of diffusion became one in which the diffusant wriggles or worms its way through the free volume of the polymer.

Figure 4, for example, shows the results of a study by Klein and Briscoe of the effect of the length of the diffusant on the diffusion constant D in linear polyethylene as a function of the length or the number of carbons N in the backbone of the diffusant. Over this whole range for N greater than 20 the behaviour corresponds to a law where the diffusion constant D varies roughly as the inverse square of the length of the diffusant i.e. $D = k\ N^{-2}$. This provided the first experimental confirmation of the theory proposed by de Gennes in France and by Edwards in Britain which suggested that diffusion occurs by a process with the lovely name of reptation. The idea is that in a polymer where in general all the chains are entangled the only way a polymer chain can move is by wriggling its way between the topological constraints imposed by neighbouring molecules (see Figure 5).

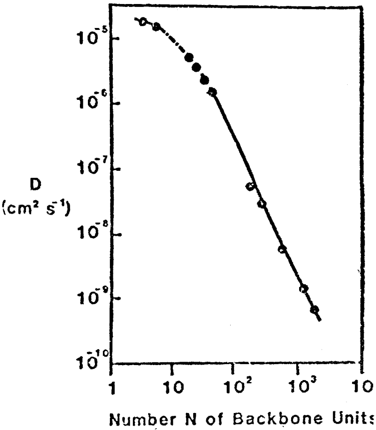

Fig 4—Diffusion of linear diffusants through molten polyethylene at 176°

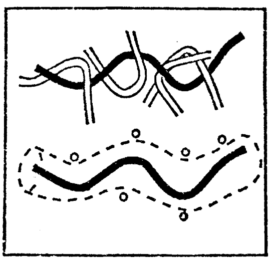

Fig 5—Schematic representation of reptation. (a) The black molecule can only move through the free volume available between neighbouring white molecules. (b) The topological constraints provided by the white molecules in (a) are shown as circles so that the black molecule may be considered to be reptating through a virtual tube (shown by the broken line).

The theory leads to the result that the diffusion constant should be proportional to the reciprocal of the square of the molecular weight, which is what we found.

A more recent study by Rennie and myself has, I think, extended our understanding of diffusion in polymers even further.

In this investigation we studied the effect of hydrostatic pressure on polymer diffusion and the results are shown in Figure 6. It is seen that the pressure reduces the diffusion constant. If we assume that this is due to the reduction of the free volume available we can calculate the free volume involved when one of these diffusant chains wriggles its way through the polymer matrix.

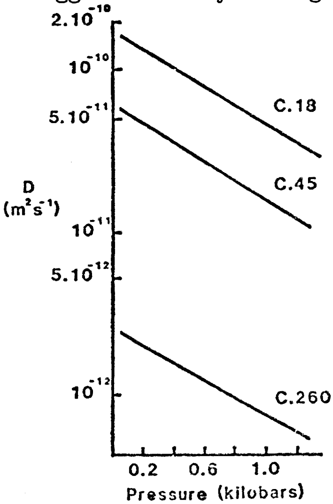

Fig 6—Effect of hydrostatic pressure on the diffusion through molten polyethylene of different hydrocarbons.

The result is the following: the volume of a diffusant molecule of 20-carbons is about 500 cubic Ångström, while the pressure activation volume is only 50 cubic Ångström. A diffusant molecule of 260-carbons has a volume of about 6,000 cubic Ångström; yet the activation volume is still less than 100 cubic Ångström. This at once tells us that although diffusion occurs by the wriggling or reptation of the chain, the chain does not wriggle as a whole; it reptates by a wrinkle, a rotation or crankshaft motion involving a volume of between 50 and 100 cubic Ångström. This new development, provides the micromechanism of the reptation process. (Figure 7). It may prove to be of direct interest to those involved in the study of adhesion of polymers to one another for here molecular diffusion across the interface must be a major factor. If pressure is applied it will affect the rate at which this diffusion occurs. I hope that some research man in the paper industry with the perception of Douglas Atack will find some link between this work and the applied field of his profession.

My fourth piece of basic work was concerned with the direct measurement of van der Waals forces. I do not need to go into detail because this work is now well established and has been taken up by other people. We now know how strong these forces are, and what part they, as distinct from those adhesive forces which

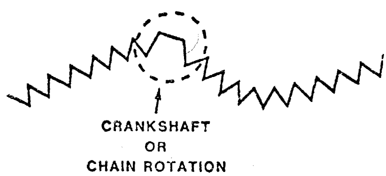

Fig 7—Schematic diagram showing that the micromechanism of reptation involves a rotation or crankshaft motion of a small unit of volume 50 – 100 Å^3.

arise from hydrogen bonding and charge transfer, play in the adhesion between fibres. This knowledge, I presume, can be applied to the adhesion of cellulose fibres.

Now let me turn to some more general concepts.

The value of fundamental research in any industrial enterprise depends on two main factors.

The first is the quality and relevance of the research.

The second, as I mentioned before, is the extent to which a fruitful link can be established between the fundamental ideas and the practical application. Very great perception is required for this to be effective.

In my view, it is desirable for industry to undertake its own basic research, because, firstly, it will enable industry to attract and keep creative research men; secondly, it will provide a corps of professional scientists to interact effectively with scientists outside industry engaged in similar work. If industry cannot maintain such a group (and to be viable it must have a certain minimum size), it would be well advised to second one or two of its best technical men to universities or other research institutions where such research is being conducted.

I speak here without personal motives since I am just about to leave my Chair at Cambridge. (Might I add in a lighter note that I wrote this by hand and gave the manuscript for typing to our new secretary, a very nice and keen young girl, who was new to university life and did not know what a Chair meant in Cambridge; when I got it back in typescript it read "I am just about to leave me chains at Cambridge". she has something there.)

There is of course another nexus between fundamental research and industry which I have only hinted at but not mentioned explicitly, and it is the last point I shall make.

This is cash, the financial support that industry can give to fundamental research, whether within its own establishment or outside. And in this connection I cannot do better than recount the anecdote I first heard from the late Sir Eric Rideal.

This is an anecdote which I feel should be repeated on regular occasions whenever industrialists come together to discuss research and its value to industry. I will tell it in

Rideal´s own words:-

The story is about a certain clergyman and his little son who went for a holiday to Somerset. On Sunday they went to church, and as they were going up the aisle the clergyman put half a crown into the offertory box and then went and sat down while the congregation was coming in and the voluntary was being played.

Apparently it went on for quite a long time and the clergyman looked around to see what was happening. He saw two vergers muttering to one another in the corner near the pulpit, and then one of them came up to him and said

"Sir, I see you are a clergyman. Would you mind taking our service today because our rector, the Reverend Abernathy, has suddenly taken ill".

The clergyman said "Certainly, I will take the service".

He went into the vestry, put on a surplice, came out and took the service, then went back into the vestry and put the surplice away. He started to walk down the aisle with his little boy and as they were getting near the porch the verger came running after him and said

"Sir, I must thank you on behalf of the congregation for taking the service. You got us out of a great difficulty. We are a poor congregation and we can´t afford much, but we can give you the contents of the offertory box."

With this he handed the clergyman the half-crown.

As they were starting to walk home the father saw the son looking very worried and muttering to himself all the time, and so he said

"Sonny-boy what worries you?"

"Well, father", said the boy, "I´ve been thinking. If you had put more money into the offertory box you´d have got more out of it".