

## FUNDAMENTAL RESEARCH FOR INDUSTRY

**The Rt. Hon. The Earl of SHANNON, Director, The Conference of Industrial Research Associations**

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HOW honoured I am to be asked to address this symposium.

Most administrators are agreed that the decision on the ‘right’ amount of money to spend on research and development is an art rather than a science. Here, I am quoting from the recent report by Lord Rothschild. Whether or not one agrees with his conclusions—and, from the fur and feathers that flew at the time of the publication of his report, it was obvious that some did not—this statement is hardly likely to be challenged, as many of us have found to our cost when faced with making or advising on such decisions. In desperation, one can search for the magic formula, but all too often (when found) it turns out to be a paragram compounded in equivocation.

Not only is one faced with the question of how much to spend, but also the interrelated question of what to spend it on and where.

We are all agreed that research must be done, but all too often the simple, straightforward issues are clouded with extraneous considerations that, from our point of view, unfortunately are often allowed to prevail.

As an illustration of this, we have seen throughout British industry during recent years a distinct switch of funds from fundamental to applied research and research for immediate development. In hard times, this is understandable, for it tends to require either farsightedness and a firm, unwavering conviction on the part of the decision maker or a period of stable, affluent economic conditions before fundamental research without an immediate definable return becomes attractive. Such living on one’s fat in hard times is a natural reaction, but logically it can continue only for a limited period without causing damage to the whole system. It is particularly gratifying in your industry to see that, despite the recent difficult times, the Paper Science Department of the University of Manchester, your Research Association at Leatherhead and Messrs Wiggins Teape among others have been continuing with such fundamental research and the organisers of this symposium are to be congratulated on bringing the matter vividly to your attention.

*Under the chairmanship of Dr H. F. Rance*

It might be argued that there is surely little need to do fundamental research in regard to paper: it has been in use some centuries B.C. and the basic raw material does not lead to the charge that the industry is using up one of our planet's irreplaceable natural resources. Yet we have seen the changes that have taken place recently: for many centuries, paper was used almost solely as the vehicle for supporting graphic information, then we saw it lead in the age of packaging, where now much of the market is being usurped by plastics. Here surely is one of the valuable fields to investigate by comparing the fundamental properties of paper and board with those of other materials currently used for packaging. This subject is the main theme of this symposium and such studies relating the product to one of its end use performances could well capture, recapture and even create as yet unknown markets.

Rather than adopt the attitude that 'here is paper, let us find markets for it', such research into the fundamental properties of the product will give one a clearer understanding of how it can satisfy future demand and meet possible future specifications. Because it is in meeting these specifications that the future of the industry must depend, money spent in this way now may not produce immediate quantifiable benefits, but it will produce the essential reserves that will in future be turned to good account by the industry.

Although, as I have mentioned, your basic raw material is not limited in world terms, it is nationally limited because of economics and it could be that, arising from your fundamental investigations, there could well be a lead to the institution of a valuable branch of the industry in the recycling of waste for as yet unprofitable uses.

Having briefly reconsidered some of the all too well known reasons for conducting fundamental research, let us for a moment consider where such money should be spent.

Strongly influenced by the atmosphere engendered by centuries of academic tradition, which we here in Cambridge are currently experiencing, one might be tempted to consider that all such pure fundamental work should be carried out at similar seats of learning. One should remember, however, that, although universities have given—and are giving—much valuable assistance in industrial fields, their main mission in life surely is the pursuit and dissemination of knowledge and only incidental to that end could they and should they provide assistance to industry. The industrial requirement is for the pursuit of a specific piece of knowledge within a defined time scale and rarely for the pursuit of knowledge *per se*. Thus, I suggest that the universities alone cannot provide the answer.

The individual companies within the industry are often left to shoulder the burden alone and such a situation can lead to the work becoming too industrially oriented towards the particular needs of only one section and even one

company within the industry. Whereas without doubt, if the work reaches a successful conclusion, there should be a greater chance of implementation by the company, such implementation is likely to be restricted to that company alone or, at the very worst, suppressed for purely local reasons. I fear that some may disagree with me at this point, for I know of directors of research who wring their hands bitterly and complain that the most difficult people to convince of the value of their work are those in the parent company who commissioned the work, whereas competitors would adopt the results with alacrity.

Having examined both extremes, there remains yet a third possibility and one with which I am connected—namely, a research association—of which in Pira you have in your industry a very fine example. Although not attempting to be all things to all men, a research association can afford to be academic without losing industrial awareness and industrial without becoming subordinated to sectional or local interests.

Recently, all the research associations in this country commissioned an impartial inquiry into themselves to ascertain whether or not they still fulfilled the objects for which they were established, what their defects are and to formulate recommendations for their future conduct. This Committee of Inquiry, under the chairmanship of Lord Bessborough, included representatives from the political, industrial, academic and Civil Service fields. The report, which was published in April 1973, is obviously destined to become a standard handbook on industrial research for some time to come.

Briefly, the report concluded that the concept of co-operative work under which research associations operate was still viable, also that, per pound sterling spent on research, the research associations have given and are giving the best return to their industries of any other organisation. Of course, there was criticism and this is what the research associations really paid for, most of the rest they already knew or suspected; it was what was wrong that they wanted to know so that rectification could be made. The report also disproved many of those old wives' tales that research associations existed purely for the small companies, were unable to undertake confidential work or were predominantly academically oriented.

Even then, the report does not imply that they are perfect nor give them an exclusive right to claim to be the sole research capability for an industry, it merely confirms their place in the spectrum of facilities available to an industry. It confirms them in that very valuable mid-position, which of necessity carries the responsibility for acting as an interface between those in industry and those in research. Without this communication, neither can understand, let alone profit from the activities of the other.

It is in this vital field of communication that this symposium is making

such a notable contribution and I feel sure that at its conclusion you will all disperse enlightened by the following speakers and in the conviction that this week has been very well spent.

# Transcription of Discussion

## *Discussion*

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*The Chairman* The theme of the 1961 symposium at Oxford was *The Formation and Structure of Paper*. Its announcement at the 1957 Cambridge symposium, the first of them all, caused a number of research activities in various parts of the world—which is indeed one of the declared aims of these symposia.

It is difficult to guess how many research projects would have remained dormant or would never have been undertaken without the challenge and call of our symposia. There is no doubt that in many cases they had the function of a catalyst and exerted a beneficial influence on the rate of progress.

In any case, by 1961, some of the research activities had produced their first results. We, the listeners, were presented with an impressive array of new concepts. Some of them were put forward as tentative working hypotheses, some of them were proclaimed with the power of conviction. What is more, some of the concepts have been pursued further up to the present day and have yielded a new crop of results. We will be listening to these today.

If I single out three of the concepts that we discussed twelve years ago, it is only because of their relevance to our first session today and does not reflect in any way on the others. The first is the concept of the uneven structure of paper in the plane of the sheet and, in particular, the concept of randomness and the question whether paper is more uniform or less uniform than random. I think it is now accepted by most that the non-uniform distribution of fibres in the plane of the sheet is the one single structural property that distinguishes paper more than any other from other sheet-like materials. Take three familiar objects—a linen cloth, regenerated cellulose film and a sheet of paper. All three can consist (and often do) of one hundred per cent cellulose and it is only their different structures that make them such entirely different materials. The first part of our session today will deal with the measurement and the consequences of the uneven areal mass distribution of paper.

The second concept is that of the layered structure of paper, a mere working hypothesis twelve years ago, but in the meantime recognised as a fundamental structural feature of paper. This will be dealt with in the first two papers after lunch.

*Under the chairmanship of Dr H. Corte*

The third concept, in some ways related to the second, is the nature and extent of fibre bonding and, in particular, the question of optical contact. The last two papers this afternoon will present some new ideas, also some new experimental discoveries in this area. The last paper will provide too a kind of link to the two sessions tomorrow.

*Prof. D. Wahren* If the work presented in our paper had been done solely for the purpose of seeing whether a real sheet is more or less uneven than a random sheet I agree with you; of course, this is just one of the things that we can do when we have these techniques available. I think that your way of making the assessment is very elegant and I will certainly try it out myself.

The only information obtained, however, is whether or not the number of flocs longer than the scanning length exceeds that of a random sheet. Our method gives a detailed picture of the floc size distribution in the real sheet and the random sheet, respectively.

*Prof. J. Silvy* This contribution is about the extensive use of the Kubelka-Munk theory to deal with the influence of large variations of the scattering coefficient on the intensity of the mass distribution, especially when the conclusion is to use a light with a high absorption—that is, the range where the validity of the Kubelka-Munk theory is doubtful.

I am sure that you know well the limit of this theory as it is applied in the field of paper studies, but may I focus it quickly by two illustrations. The first is from a work published by Dr L. S. Nordman in 1965 of the Finnish Paper Research Institute in his extensive research programme on the measurement of relative bonded area in correlation with variations of the scattering coefficient of more or less bonded sheets. Here we can see the influence of the colour of the light and proof is given of the dependence between coefficients  $S$  and  $K$ . The  $S$  value diminishes when the absorption  $K$  increases—is this explained theoretically?

In the second illustration, we have a comparison made by Klier of the Coating Center in Bethlehem, Pennsylvania, between the rigorous theory of radiative transfer in diffusing media by Chandrasekhar and the results of the Kubelka-Munk approximations. Here, links are established between the Kubelka-Munk apparent absorption and the scattering coefficients of the true absorption coefficients  $\alpha$  (the  $\eta$  curve) and the true scattering coefficient  $\sigma$  (the  $\chi$  curve). We can see that  $K$  is not linearly linked to the true absorption  $\alpha$  nor is  $S$  to  $\sigma$ , consequently  $K/S$  to  $\alpha/\sigma$ . We see that theoretically  $S$  as estimated by Kubelka-Munk diminishes as  $K/S$  increases and the discrepancy is more and more important as absorption of light in the medium becomes higher. (See Klier, K., *J. Opt. Soc. Amer.*, 1972, **62** (7), 882.)

### *Discussion*

*Prof. Wahren* I will try to answer what is really not a question. We have been looking into these matters in depth and I am very well aware of the facts shown in your two illustrations. We are content if in calibration we get proportionality between grammage and the signal. The Kubelka-Munk equations may be inexact, but it is normally a matter of only a few per cent of the absolute value. After calibration (or after automatic standardisation, which is a feature of our method), the errors are much smaller. The scattering coefficient varies with the wavelength of the light. We do not use monochromatic light, but well-filtered light. We have found that this is good enough. White light cannot be used, however, if any reasonable degree of accuracy is to be obtained. Your remarks are relevant in general, but not particularly to our measurements.

*Dr D. Atack* I am gratified that our prepared contribution has elicited such a lengthy clarification in the form of an addendum. Unfortunately, we received the addendum only this morning and have not had time to study it. From a brief perusal of it, we feel that it does not in any way affect the validity of our original remarks. We shall discuss the issue in more detail in a forthcoming publication.