Preferred citation: D.A.I. Goring. Seventh fundamental research symposium summing up. In **The role of fundamental research in paper-making**, *Trans. of the VIIth Fund. Res. Symp. Cambridge, 1981*, (Fundamental Research Committee, ed.), pp 879–891, FRC, Manchester, 2018.
DOI: 10.15376/frc.1981.2.879.

SEVENTH FUNDAMENTAL RESEARCH SYMPOSIUM SUMMING UP

D.A.I. Goring
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Thank you, Mr. Chairman and Fellow Researchers.

Firstly, I must thank the Conference Committee and the British Paper and Board Industry Federation for inviting me to do this important but difficult job.

There is one advantage in being the summary speaker. I can say anything I like. I have no preprints, no abstracts, not even very much in the way of guidelines. I have complete freedom. All I have to avoid is saying anything bad about my boss, because he's right there in the audience. So be clear about one thing: the very best talk at this symposium will be that given by Mr. Burgess later on this morning, and I urge you all to listen well.

So, having said that, I can go on to say something about what has happened here during the last four days.

Like Gaul, this symposium can be divided into three parts.

The first deals with the philosophy behind fundamental research and the relationship between it and its practical application in industry.

The second is really a collection of technical papers on topics, both fundamental and applied, of current interest to the industry.

The third, which hasn't happened yet, but which comes later today, is the description of the way various research enterprises connected with our industry are managed.

My task is to review the first two parts, and to lead you courteously into the third.

I shall review first the technical part of the programme, even though this followed the talks on the raison d'etre of fundamental research. I have grouped the papers under what I saw as logical headings although my scheme may not fit with how the authors concerned saw the subject matter of their papers.

Technical Programme

Visitors' talks:

Evolution of life molecules	Eigen
Composite materials	Kelly
Examples	Tabor

To begin with we heard two lectures on topics not directly connected with the paper industry, though in the third Tabor gave some examples of how his own research had affected it. I would like to say that, like everyone here, I thought this was an excellent idea. Seated in that seat there, I could feel myself being cross-fertilised. (There are several Freudian references in this review, but between friends that doesn't matter.)

Let me give you some examples.

When Professor Tabor was speaking about the diffusion of molecules in polymer systems, I couldn't help thinking about some experiments that we have just done to investigate the way in which lignin molecules can be washed out of unbleached fibres, seemingly unendingly. All we had to do was to leave the fibres in water and the lignin came out. If this could be speeded up, just think of the difference it would make to our pulp processing. Can Professor Tabor's ideas be applied to that diffusion process to speed up the removal of lignin molecules?

What Dr. Eigen was telling us is rapidly becoming the basis of an entirely new technology, which will affect our industry in many ways.

Then again, I had a very bizarre idea while listening to Professor Kelly. Suppose you asked a composite professor, like him, how he would go about reversing the process he spends all his time doing, how he would take apart a composite material using very little energy and recovering the fibres intact? He doesn't know anything about chemical and mechanical pulping, probably doesn't even know the difference, but his answer might be very useful.

Pulps and Pulping:

Anthraquinone pulping	Clayton and Fielding
Recycled Fibres	de Ruvo and Htun
Straw oxygen pulping	Ceragioli
Non-wood pulping	Grant
Non-wood pulping	Judt

Then we moved on to papers on pulp and pulping, and I would say that all of these had, as a common theme, the concern of the industry for its fibre supplies. This is so even in the case of anthraquinone pulping, whose biggest advantage is, of course, its high yield. I've included the paper by Grant, which is in the preprints, though it unfortunately wasn't given because, as we've heard, Dr. Grant is sick. I have put the paper in by Judt because although he was talking about UNIDO his main message was "Can you make pulp from materials other than Northern Softwood?" I would like to say that this is an important message and is going to become increasingly more so. We are in the process of becoming an industry that eats garbage and produces useful products. We have to be.

The very best use of wood still in North America, even in these days of depression in the lumber industry, is to use it whole, for building houses or furniture, and even now the proportion of the pulp production in wood-rich Canada that is dependent on residues is between 40 and 50% of the total. So we are becoming an industry of recyclers and residue users, and we will just have to accustom ourselves to using anything that has cellulose in it. I think this is a world-wide message, and I think it was exemplified by this group of papers here.

In passing, I want to compliment Clayton and de Ruvo for making the effort to connect their message with the fundamental research that was behind it. They both took a lot of pains to do that.

Fibre Suspensions:

Ultrasonic characterisation	Habeger and Baum
Flow resistance	Eissa, Naito, Usuda, and Kadoya

Then there came a couple of papers on fibre suspensions, which were of particular importance because so few methods are available to us to measure the fundamental mechanical parameters of fibres that are in suspension. Why? because it is difficult. The only thing I can say about these, is that while the paper on ultrasonic characterisations is interesting, I agree with Baum that the technique is a long way from becoming a convincing and useful laboratory tool. But I am sure that it will eventually.

In Kadoya's paper the flow properties of the suspension were related to the mechanical properties of the wet fibres. Certainly, there are discoveries to be made in this difficult field of research, and there is no doubt that from such work will come the inspiration required to solve the problems of formation and retention on the high speed formers of tomorrow.

Water removal:

Pressing	Carlsson and Lindström
Press drying	Back and Swenson

Then we heard about water removal. We still don't know much about the basic mechanism of pressing. We still don't know what happens when we squeeze water out of the fibre substance. It is all very well to take the water out of the interstitial web, one can visualise that, but what happens when you try to squeeze the water out of the fibre itself? I think that Carlsson and Lindström, with their new apparatus, are in a position to get some answers in this area, provided they can reduce the duration of their pressure pulse enough that it become more representative of a paper machine press.

Of course, press drying, although not a new concept, is an exciting new possible application and the excellent papers by Back and Swenson created a lot of interest, particularly when

Setterholm came up and contributed to the discussion as well. On the laboratory and pilot scale at slow speed the results are impressive. A liner-board which meets all specifications, made of an 80% semi-chemical hardwood pulp is a real pot of gold at the end of the rainbow. All we need is the hardware, and during the session Chairman Wahren flung down the gauntlet to the machinery builders, one of whom is in the audience.

But I wonder why he didn't pick it up himself, because, after all, our Research Institutes are supposed to take on development projects which are too big for individual companies. I suspect that this topic can be taken a bit further by one or other of the interested pulp and paper research institutes.

Mechanical Properties:

Stress-strain behaviour	Seth and Page
Fibre geometry and mechanical properties	Perkins and Mark
Ultrasonic characterisation of paper	Baum, Habeger, and Fleischman
Biaxial failure	Fellers, Westerlind, and de Ruvo

From what we heard then we can say that basic research in mechanical properties is alive and well. I would like, at the risk of being considered a chauvinistic Papripig, to mention and compliment Seth and Page on the elegance and convincing simplicity of their theory, and compliment Derek Page on the compelling lucidity of his presentation. Well done! It shows that the glass is clearing. It could be argued that this work on the understanding of the way paper behaves mechanically won't add many centimetres to the breaking length of a tough paper perfectly made from a Northern softwood-kraft. But, even now, paper is seldom perfectly made, and as ours becomes an industry of recyclers and residue users, these theories are going to be absolutely essential in helping us get the best out of the various materials that we will have to depend on.

Optical properties:

Formation	Komppa and Ebeling
Gloss	Oittinen
Coating structure	Ranger
Print quality	Lyne, Parush, Richter, Jordan, Donderi, and Ramsay

These papers all dealt with, in one way or another, the other great concern we have for our products, and that is how they print. This is an area which, in my opinion, is in the process of transformation from an art to a science.

The first three papers (as listed above) dealt with the analysis of the properties of the paper as a substrate for printing on, while the last one dealt with the quantitative measurement of perception and preference. I think these papers mark the start, and a very important start, of our concern for the fundamentals of the surface physics and the surface chemistry of paper, and of their influence on human visual perception. Research work in these areas lays the foundations for major technological changes in the next couple of decades.

Image analysis:

Fibre characterisation	Jordan and Page
Fibre characterisation	Taylor and Dixon
Paper structure	Kropholler, Clark, and Gorres

Then we had what was really three papers on image analysis, followed by a panel discussion on the subject. I think that what was said showed that image analysis is not yet of age but is growing up fast. It also warned that though this is potentially a very powerful technique which could lead to new instruments, and even eventually to on-line measurements, now the message is "Caution: be sure that you know what you are getting into, because there is more to it than there seems on the surface". It was a very interesting discussion period that stated clearly the potential of this important method.

Paper machines:

Spectrum analysis	Parker and Epton
Pilot paper machine (FEX)	Norman, de Ruvo, Fälldin, Hallgren, Lilja, Lindström, and Röding

Finally, there were a couple of papers on what I call paper machines, a very apt theme for a paper-making conference. Listening to the talk from STFI on the new FEX installation took me back to my youth, and the physical chemistry I used to do with mercury and handblown glass mainly, and I wondered "basic research with a $12 m machine?" Why not? I bet a series of experiments on this equipment would cost a lot less than a series of experiments on any of the big particle accelerators that we have around the world. Maybe we need more FEX's, so I would like us to wish STFI the very best of luck in their new venture.

This is going faster than I thought. I hope I don't finish too soon, but I must just say two things. One is that there was a number of prepared discussions contributed which dealt with the finer text of the book, but with apologies to my friends who made them, and they were invariably of a high standard, I just haven't had time to mention them.

The other general comment I would like to make about the overall technical programme is "For goodness sake, do it again!" What Nissan said last night is absolutely right: these are the flagships of our pulp and paper symposia. Time and again young people have come to me saying that it was at one of these meetings they got the idea for some research topic or other. So my message is "do it again".

And since it was quite clear that the meeting really came to life when well-presented papers on original research were being discussed, I think that next time you will easily find five days' worth of good technical papers to put on. So I give every encouragement to BPBIF to continue the series.

Raison d'etre of fundamental research

General	Tabor
General	Place
Quality control	Hendry
Mechanical properties	Steenberg
Paper-making	Wahren
Pulping	Hartler

The other part of the symposium dealt with what I call the raison d'etre. Why fundamental research, why, in the words of Place, intrinsic components to our research. Here we had six people, Tabor and Place during the introductory session, and the others later on the first day.

What struck me and, indeed, others about Tabor's talk, was his stressing the important concept of fashion in research. Research is a thing of fashion and all kinds of influences set these fashions. Sometimes they are set so that they are not particularly good either for science in general, or for any particular industry. How I agree with that. Sometimes the people who really take giant steps are the ones who don't follow fashions. They're either too stupid or too lazy to read or understand things and then they go and do something, and boy! they've made a discovery.

Place gave an excellent, vivid description about what the USA, in fact, everybody, must do to make sure that technological progress goes on. He considered that the separation of effort into basic or applied research may impair the efficiency of the overall process. Instead, he saw a project as having both intrinsic (knowledge generating) and extrinsic (knowledge using) factors. Our judgement on the worth of a project should be based on an assessment of both factors.

He pointed out how governments, by massive funding of certain areas, can distort the distribution of effort, and how government support is a major factor in creating Tabor's research fashions.

He also made the point that there isn't a great source of technical people, not already in R & D, who can be brought in. The net has captured virtually all of them, so that in future we will be people-limited, and money-limited, and priorities will have to be set. Pardon me for going on about it, I could keep going all day, but hearing Place' and Tabor's talks reminded me of the immensity of this problem.

I am sure that everyone in the audience agrees that our industry has to support research and keep supporting it, both on the fundamental and on the development side. Everybody agrees with that, but I felt, and this is my own opinion, that it is not enough. As an industry we have to interact with government and, as Tabor said, make friends with university professors, so that we start to take a hand in setting the fashions. Tnen young scientists working in universities will think how great it would be to do research associated with industry, and particularly with the pulp and paper industry. That we in the industry must take a hand in managing the fashions was the message I got, personally, out of those two talks.

Then there were four talks that were, in a sense, looking back. These were talks in which the speakers, respected and proven leaders in our industry, looked back to try to connect basic research with practical application.

Hendry came right out in his preprint and in his talk, saying that he couldn't see any connection between fundamental research and quality control, and I admire that. I don't know enough about the subject to know whether he is right or not, but I could sympathise with his view.

But what I do know is that in his excellent talk he discussed beta meters. Beta meters are now essential to the control of the paper-making process and therefore to the quality of our products. So let us consider the research tree that led to the beta meter: there were between 500 and 1000 papers in the primary literature, without any one of which we wouldn't have beta meters. There is a mountain of fundamental research on which the beta meter rests. And this led me to the thought that certainly in one area of quality control, the area of sensors,

fundamental research is of paramount importance.

Steenberg came out and said, and again one has to admire his courage, that he didn't think that the work on the mechanical properties of paper was useful. In the written version of his paper he called such knowledge sterile, though he conceded that good experimental data is always useful in optimising a process. He prefers the more modern holistic theories of materials science, that is theories that look at paper, not as an assembly of fibres, but as a material on its own.

Now Steenberg may be right, because after all, scientists have concentrated on describing things which are easy, like gases, solutions, and crystals. Only recently have we looked at the tough things like plastics and non-homogeneous solids. Maybe, we have to turn over a new page (excuse me Derek). But there is one area where I don't agree with Steenberg at all, and that is the utility of the present theories. For example (and I can quote this at first hand), I have seen the theoretical understanding of the mechanical properties of paper applied, time and again, by Derek Page and his group at our institute, to the solution of practical mill problems in Canada. Radvan supported this from the floor earlier in the meeting. I'm sure that the same could be said of Alf de Ruvo and his group in Sweden, and of other such groups around the world. It is far better to look through a glass darkly than to have no window at all. And what we have seen at this meeting is that the glass in this particular window is clearing rapidly.

Wahren deserves a special accolade for his magnificent documentation of the thread from fundamental research to practical application in the paper-making process. He did a splendid job. And the reason he could do this, as he pointed out, was that right at the start, when things were boiling up, there was a number of great people (Wrist, Mason, Mardon, Nissan, Taylor, and others) interacting on the basics of it. Wrist confirmed this, and so did Mardon.

Hartler, you know, talking about pulping, was looking for the same thing, but didn't find it. He found something a little different: the basic knowledge was there and significant, but

the advances were actually made by what he called artists or empiricists. They did not come directly from the fundamental research.

Now, how can we characterise the views of these six speakers? How can we say what is common about them? I think it is that they are all different. They don't disagree, but they do differ. If you asked six artists to come up here and talk about beauty, six jury-men to talk about the law, or six writers to talk about style, their views would all be different, and it is the same with our six speakers; each has given a facet of the truth.

Does that mean that the relationship between fundamental research and technological progress cannot be defined exactly? Perhaps, but I don't think even that is the case. Instead I want to make a suggestion.

There exists today a discipline of The Science of Science. From a calculation that Place and I made we think there are probably about 2 million professional scientists in the world. Some of them, academics and in industry, are studying scientifically the way in which science works. I think that if we want to shed some light on this question we shouldn't ask the Wahrens, the Steenbergs, the Hartlers and Hendrys. At the next meeting we should have a visitor speak who is not a scientist in the paper industry but who makes it his business to study how science works. Lets ask him to tell us the relationship between fundamental research and practical application.

Perhaps because he and I are both chemists I like Hartler's imagery the best. He sees researchers as mountain builders, inventors as mast erectors. (I told you there was some Freudian stuff in this.) Scientists and engineers go about building the mountains of knowledge that we have, and on these mountains entrepeneurs, empiricists, or artists, rush about getting inspiration and erecting flagpoles. Suddenly a new flag is flying, a new invention, a new process, a new product has arrived, and then the world of science and technology goes on building the mountain around it.

Now, I would like to make two points about this. One is, the height of the flag is the height of the mast plus the height of the mountain. Back down at sea level there are dolphins and apes, both with roughly the same body-to-brain ratio as man, and I'm sure an ape could run a paper machine, but it is only people with knowledge who occupy the mountain tops.

The other thing I would like to say is that research managers have to decide what mountains to build, because there isn't time for all of them, and so we need a policy.

Many people have preached sermons at this Symposium, so why shouldn't I do the same? When I mentioned that I was going to do this, David Clayton suggested that I use the well-known text from St. Matthews's Gospel

"Wherefore by their fruits ye shall know them".

This, of course would be very appropriate, as it is, in a way, the theme of this Symposium and excellent advice to all research managers. However, the text I finally chose instead is a little more disturbing

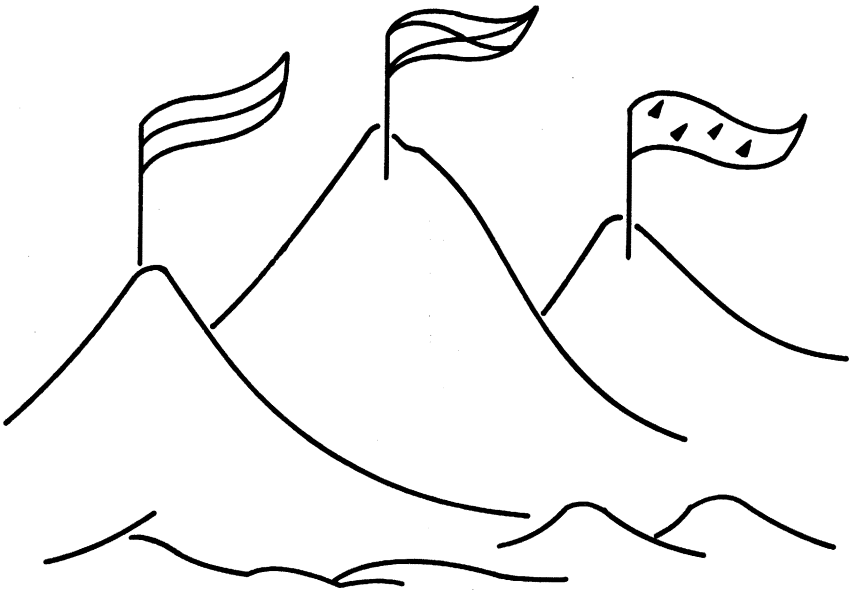
"Where there is no vision, the people perish"
Proverbs, 29,18

A clergyman friend of mine once said that this is an inspired mistranslation. But it's legitimate to use it anyway, as they are strong words. It is not that a little bit of vision is a good thing: nor even that without it scientists are going to be out of work: it means that without vision everything goes down the drain, people, society, everything. If this industry is to survive we must have people with vision.

What do we mean by vision? We don't mean unquestioning supporters of basic research: we don't mean innovating every wild idea that comes along: we don't mean pressing for detailed accountability of research, though this is important.

I think what we mean is that first and foremost we be aware of all the mountains, making commitments to mountain building, and taking a lot of pains, even in fundamental research, in choosing what mountains to build. Then we must commit ourselves to taking risks to erect the masts, and get the flags flying, while taking a lot of pains in choosing what masts to put up. That's what I understand by vision. This industry has always attracted people with vision. The very existence of this symposium taking place here in front of you all is an indication of vision in this industry. In the `80's, as Place said, we are going to need a bit more vision than we've ever needed before and I am sure we will find it.

Thank you very much.



Thanks to Nils Hartler

Discussion

Prof. H. W. Giertz, University of Trondheim, Norway

Having discussed the importance of fundamental research and having heard at this symposium of how many practical achievements and industrial applications have been engendered by fundamental research, I would like to know if anyone has gone over his files to see what proportion of fundamental research projects has actually given rise to useful results? In the research organisation at Trondheim we did analyse the useful returns on fundamental research and concluded that only 15% of projects that began as fundamental research had any ultimate practical use.

Mr. H.A. Posner

We have tried to check back, as you suggest, on several occasions. However, it has always turned out very difficult, for two reasons. Firstly, the records often aren't very good. It is very often the case that to unravel the course of a particular development is impossible without the assistance of the personnel involved. Secondly, we find that much of the research we do has to be considered as building blocks, not of direct relevance to an identifiable end, but nevertheless very important to it. Combinations of apparently unrelated building blocks can, sometimes and in the right hands, be the correct combination for a technical breakthrough. For these reasons we think it very difficult indeed to try to perform the sort of analysis Prof. Giertz mentions.

Dr. J. Mardon, Omni-Continental, USA

Please forgive me if I phrase this question a little tactlessly, but I want to ask about what happened at IPC when it went through its difficult period some ten years ago. At that time, as many people in the industry know, its reputation diminished, so that it made a substantial effort to reverse this change. Could you identify what aspects of IPC's work or organisation you found inadequate, and how you changed your planning in order to remedy the deficiencies?

Mr. H.A. Posner

It is a very long story. Most people are aware that the sticky patch IPC went through in the later sixties-early seventies was partly a reflection of the mood of the times. There were however, some particular factors which contributed more than most to the problems at that time. They were quite easily identified, and all were important.

The institute at that time was trying to support its non-educational faculty on a contract research basis. This is a tough way of supporting yourself under the best circumstances. It seems to me that efficient and successful contract research organisations work very differently from most governmental research institutes. They, like everyone, have good people, and then leave them to make their own contacts and build their own organisations, subject only to the condition that they continue to turn in a profit. As soon as that condition is not met, then they are out.

At the IPC it is difficult to do that, partly because of our educational role. Thus, the use of contract research as a way for university staff to support themselves outside their academic life was one of the factors which led to the institute's difficulties.

A not-unrelated factor was our losing touch with the outside world. As is always the case, researchers would rather talk to one another than to anyone else, being quite capable of concocting enough interesting problems for one another to work on, with absolutely no reference to anyone else. So why go out to find problems? Thus I think the institute had become very much too introspective.

We also had staffing personality problems, of delayed decisions and insufficient flexibility.

Mr. G. Place, Proctor and Gamble, USA

You mentioned that the IPC targets about 50% of its resources in basic research. I believe that the paper industry is going to change its technology radically within the next two decades. What percentage of your institute's research effort is devoted to

major technological changes, discontinuous with existing methods, as opposed to evolutionary upgrades of what we are doing now?

Mr. H.A. Posner

A relatively small percentage, at a guess about 15%, but not more than that.

Mr. G. Place

Is that because you are interacting with an industry that already exists?

Mr. H.A. Posner

Yes, and it is very understandable. The IPC is not looking at things beyond the realms of current paper-making technology, because its emphasis must be on the realistically practicable. We can and do bring up questions of discontinuous change sometimes, but they must be couched in terms of existing practicability. I don't believe it is the role of IPC to undertake that type of research except when an identifiable need for it arises. We must always be aware of what industry sees as the priorities.

Mr. L. Rodes, São Paulo, Brazil

Some years ago you ran a strategic planning exercise in your institute. Would you say it was successful, and, if you were to repeat it, how would you change the way you conducted it?

Mr H.A. Posner

As a matter of fact we are conducting a similar exercise now. There is a wide variety of possible methodologies for such investigations. The one we at IPC selected is that which seems most appropriate to the collection of people involved, not only within but also outside the institute. Even the selection of the methodology has involved not only members of the institute, but also a number of people from industry.

Dr. A.H. Nissan

People have been asking how much of the fundamental research effort in the various institutes has a useful outcome. One study mentioned suggests 15%, which I consider surprisingly high. To understand how this figure comes about, I think perhaps we must appreciate that the term "fundamental research" has two connotations. Thus Sir G.I. Taylor's work on the instability of rotational flow, published in the Royal Society transactions, was pure fundamental research. Studying what happens on a table roll, even when it is the same problem as Sir G.I. Taylor's, should properly be called "Paper Science fundamental research". This is therefore an application of a deeper level of fundamental research, and I presume that this is why such a relatively high proportion of what is understood in the research institute as fundamental research has a successful outcome. Now may I ask Professor Giertz to repeat his earlier question to Mr. Posner, so that others may have a chance of answering it.

Prof. H.W. Giertz

My question to Mr. Posner was, has anyone in your institute looked back through the last fifteen or twenty years' files to try to follow up lines of research, to establish whether or not they led, eventually, to useful results? This is to some degree the matter to be covered by Dr. Scheuring in his paper later today. He will show the technical leader always goes over a project after its completion to try to show what it has led to.

Mr. B.W. Burgess

We find that applications of our work can surprisingly often be traced back to fundamental research. Though we have never conducted an exhuastive examination, it is surprising how often the comment that some piece of work is clearly traceable to such and such past fundamental research is heard. Consider these examples, which all began as fundamental and basic studies: our study of pitch fouling, now being applied in the majority of Canadian mills: similarly our corrosion research, resulting in the Papritection system which greatly extends the life of bleach

plant washers. Dr. Tabor earlier this week mentioned the work of Dr. Atack, whose study of sliding friction gave us a very useful insight into the mechanism of fibre removal in all forms of mechanical pulping. There are other examples too, where fundamental studies at our or other institutes have resulted in significant improvements to industrial processes.

Mr. G. Place

I am concerned that the paper industry must soon face serious changes, and I am trying to discover what role the various institutes are playing in identifying and forcing our attention on these changes. The structure of the funding and managerial control of these institutes suggests, in my experience, that they will be the last places to discover the discontinuities that must occur.

Mr. D. Attwood, PIRA, UK

PIRA is at present involved in pursuing such a discontinuity as those of which you speak, though it is of no help to paper-making. I am speaking of the new electronic information laboratory. This is a discontinuity that will affect us all, though it can only harm the paper industry, which will have to struggle on, trying to compete and think up different uses for paper.

Mr. B.W. Burgess

Mr. Place's question is very important. Part of the brief of our institutes must be to lead the industry, to try to determine what is going to happen in twenty year's time, so as to prepare the industry for it. We spend a lot of time on this. We have a future awareness committee engaged in technological forecasting and we make use of every device we can think of to try to anticipate future technical needs. This committee works alongside our Research Programme Committee, where the summary organisation of our research effort is done. We believe that one mechanism for initiating action on these technical step jumps is to encourage research by our staff on which they do not have to

report. We are concerned that the level of this exploratory research in our institute has declined recently and moves are in hand to reverse this. We believe it to be of the utmost importance that a scientist can retire into a corner to try out his screwy idea before it is exposed to the harsh light of reality.

Dr. R. Martin-Löf

I agree very much with what Mr. Burgess said, with the addition that I believe the government support for our institute adds to the freedom of the scientists to explore less immediately useful directions. Thus a project doesn't have to convince industry of its viability too early.

Dr. A.J. Michell

Perhaps the best example of a discontinuity, though not a very great one, in CSIRO, was the move into composite materials. It required a completely new start by us, with initially no enthusiasm from industry. This has now changed, since we have come up with several interesting inventions and patents.

In CSIRO, being a government body, the advisory committees are purely advisory. All the decision making power lies in the hands of the senior executive, who can see a project started if he feels sufficiently strongly about it.

Mr. E.J. Justus, Beloit Corporation, USA

Discontinuities, doing things differently, always need one or two dedicated people, backed by a courageous organisation, to come to fulfilment. The onus of responsibility for adopting a new idea lies with the paper industry itself. The problem of the transfer of good ideas from laboratory to mill makes demands as great on individual courage as on technology.

Prof. N. Hartler, RIT, Sweden

I think that the industrial committees play a very useful role in directing the institutes' research programmes, but that it is unreasonable to expect them to take much part in

identifying the long range changes, the discontinuities. The reponsibility for this must be with the individual institute staff, who must be sufficiently strong to resist following completely what the industry committee says. They must be forceful enough to be able to see their own ideas through, and good enough that these ideas will be of value. But the responsibility for step changes must be with individuals in the institutes.

Mr. S.O. Dillen, Stora Kopparberg, Sweden

I think there are two aspects of the discontinuity subject, and the answers so far given don't match the question put. The answers tend to have been concerned with the difficulties of adaptation, of the individual effort needed to force changes through, which is indeed one aspect of the matter. But I think that the questions have been asking to what extent research in institutes can hope to recognise the discontinuities: quite another matter. It is by no means self-evident that it is in the institutes' interests to make discontinuous discoveries, because of the drastic effects they would have on the industry.

Dr. A.H. Nissan

That is why they are called discontinuities: some companies discontinue. I have not found a single instance in history of an important or novel idea being born in the mind of the majority. The ideas from which discontinuities stem invariably occur to a minority of one, and they are almost never welcome. We shouldn't be concerned about that, as one of their strengths is their ability to withstand criticism. They will not be valuable if they can't. Anyone with suggestions of how to foster such ideas should please speak up.

Prof. D. Wahren, IPC, USA

On what criteria do the members of the panel believe that a research director should work when trying to judge whether or not to support a new idea, such as might give rise to a discontinuity?

Mr. B.W. Burgess

With difficulty. There are no rules, and such decisions can only be made with support, advice, and, ultimately, courage. Our institute recently moved into bio-technology. We don't know what will result, though we hope it will be useful. We do expect, however, to have to support that work for a good number of years with no returns. The initiative to move into this field came entirely from within the institute, and has had nothing to do with the industry.

Mr. J. Adams, BPBIF, UK

Nothing has been said here about the role of universities in fundamental research. I suggest that they have a much greater likelihood of provoking the development of discontinuities than do the research institutes, because of their greater potential for cross-fertilisation from different disciplines.

And now I would like to ask Professor Göttsching whether he, in view of recent EEC bureaucratic intervention in the matter of the amounts of waste paper to be included in pulp, believes that the European research institutes should work more closely with the industry federations, the better to resist bureaucratic pressures?

Prof. L. Göttsching

You are asking for better co-operation between the research institutes and the industry federations in the various countries of the EEC. This you think would be the way to improve communication between the research institutes and the EEC bureaucracy. But I think that they work very closely together already, at least in West Germany. And then there is the question of who should try to improve this communication, the federations or the institutes. I think it would be a matter for the federations, as they have the necessary power.

Dr. A.H. Nissan

I draw a different conclusion from the recent EEC experience mentioned. I believe there are problems faced by all industries, particularly paper, which are not purely political or commercial, but have also a technological content, such as this one regarding the inclusion of a greater proportion of waste paper in new pulp than hitherto. These problems must be studied within the industry, even at the risk of a disquieting result. It will be impossible to stop that study; so surely it is better that it shouldn't be conducted by outside amateurs, who may well fail to take important technical aspects into consideration. I am certain that it is better in the long term for the industry's research institutes to investigate responsibly and fully the fringe problems such as conservation, pollution and safety.

Dr. R. Martin-Löf

The Swedish experience in the environmental debate was that by taking the lead and the initiative, industry could so improve its relations with the government that its point of view is much more fairly heard. I think the outcome has been greatly more satisfactory to us than it would if the initial study had been left to the National Environmental Board. I think industry must vigorously study its own problems because that is the only way of ensuring that proposed solutions fall within practical technology, and that end products meet the customers' requirements without being hazardous.

Dr. J.E. Luce, International Paper, USA

Returning for a moment to the question of discontinuities, I am sure no-one here believes they are spontaneous. Discontinuities result from the combination of two processes. Firstly, there must be the recognition of a need, that is to say, an overall need, which might be defined by asking "What busines are we in?". Thus in the paper industry we are in the business of substrates, communication, wrapping or cleaning up mess. Secondly, one has to ask oneself what alternative ways are available for satisfying those needs, other than those in current

use. Generally, scientists aren't very good at asking these overall questions, but one thing fundamental researchers are good at is recognising opportunities for satisfying these needs. I suggest that even the largest companies are not able to support truly fundamental research, but they are aware of the needs. Thus the combination necessary for a discontinuity to result can occur if the links between the large company, with its knowledge of the needs, and the fundamental research institute can be strengthened. This I see as the weakest link and one that must be reinforced even if it involves considerable retraining of the people involved.

Mr. G. Place

Dr. Goring said that the greatest ideas can only arise from mountains of solid background work, and I agree with him. But my experience suggests that they usually occur only at interfaces between displines, not within the core of a single discipline. Thus, for them to arise there must be interaction between several sciences besides the one of need. Having created a climate in which there is this required interaction, then the exploratory team investigating it should, I think, be fairly small: one or two of the right people is probably the correct size: with any more it is likely the team would come apart. Thus I think it is worth keeping the number of people involved in the early stages small, at least until they begin to produce some results: then it becomes sensible to increase the effort. One of the roles I believe could be usefully fulfilled by the research institutes is the bringing together of the various disciplines from the universities, with whom they often have closer ties than industry does. The resulting interactions I believe, as I have said, would provide the groundwork for Dr. Goring's flags of achievement.

Mr. P. Waern-Bugge, Stora Kopparberg, Sweden

According to the figures given in the preprints, Europe manufactures some forty million tonnes of paper annually. The larger part of this goes forward to converting in one way or

another, and yet almost all the research funds are spent in the paper industry, and very little on converting. In fact there is an appalling lack of basic research on the downstream side of our industry, which I think reflects a lack of innovative thinking on the part of the end users. If any of the panel would care to elaborate on that I would be the most interested.

Dr. R. Martin-Löf

To a large extent I think rectifying this deficiency should be the responsibility of the paper industry. Converters are the paper industry's customers and as such should be encouraged to develop their uses for paper. Any rapprochement must also include the converting machinery manufacturers, and will take courage and determination. There is every reason for the paper industry to take the initiative in this, and to try to get as much as possible out of the contact. There should be two-way communication, so that, for example, paper can be matched to ink rather than, as is usually the case, the reverse.

Mr. D. Attwood

The paper industry has sometimes been very bad at recognising discontinuities when they occur in the downstream industries. For example, when web offset printing for newsprint was introduced,the paper industry failed to take notice, and all the research had to be done afterwards, at great expense. There are now new developments taking place in packaging, which the paper is not good enough to handle. We seem again to have been caught unawares by these developments, and I suggest that we should spend more time talking to downstream equipment manufacturers in future, to try to be aware of what changes are in the offing.

Mr. E.J. Justus

I want to say a few words in support of Mr. Posner. The strength of the Institute of Paper Chemistry, and the other institutes, lies primarily in their education role. The outstanding young people from these institutes who enter the

industry give it its great strength. Our company does not look to these institutes to do our research work for us. We are interested in the Ph.D. and other research work that is done, but the primary function of these institutes is as centres of educational excellence.

Mr. P. Wrist, Mead Corporation, USA

Mr. Posner described some of the changes that were made in the re-organisation of the IPC. In particular he mentioned that changes were made to the mixture of personalities, which, it was felt, had become too homogeneous. Probably one of the essential ingredients in furthering a discontinuity is a mixture of personalities and disciplines.

The Advisory Committee felt that further re-organisation was still needed, so they tried to advise the institute management of the direction in long-term research where they felt there was need for knowledge. I believe it is in supplying essential understanding that an institute's main purpose lies, rather than the development of this understanding to useful applications.

After considerable discussion, five areas in need of long term investigation were identified. The emphasis on the long term was felt to be important because of the institute's one step removal from the market place; it was felt that the institute should not chase after every short term development of the market, which it couldn't possibly hope to follow because of this position of remove. The five areas have continued important over the past eight or nine years.

The first was the supply of raw materials, the concern being to maintain adequate supplies to ensure the healthy future of the industry. Within this overall title investigations ranged from genetics to pulping yield improvements. The continuing recommendation from this branch of the work has been that the productivity of our eventually finite land resource must be continuously improved.

The second area investigated was energy consumption. It was recognised that the paper industry is highly energy intensive, second only to aluminium smelting. Thus here too, there must be

continual pressure to improve the energy efficiency of the process.

The third area was that the popular view of the environment had seen a discontinuous change, so that many actions acceptable before 1970 were no longer so after 1970. This has created the opportunity for a considerable re-evaluation of the economic factors in decision making in our industry. The initial reaction from the industry was to patch up, and reduce the impact of the waste produced. This approach has been replaced over the years by one in which the total amounts of waste are reduced, which of course has benefitted the first two fields of study.

The fourth area concerned the capital intensiveness of the industry. It is becoming increasingly more costly to introduce a new unit of production in paper-making, and, even without revolutionary change, it is important continuously to improve the process of productivity. This of course involves further investment, and so the process was thoroughly examined to try to reduce some of the capital intensity.

The last area chosen for investigation concerned the fact that very little account of intended end use is ever taken in the design or testing of our products. Q.C. tests tend to be limited to what is easy, without any real evidence that these have much relevance to properties important in the market place. So effort has been spent trying to discover what properties are of importance to end users, to try to optimise the product without excessively increasing raw material demand.

Dr. A.H. Nissan

I appreciate your having made this contribution at this juncture, where I am sure it is appropriate, and thank you for having made it at such short notice.

Dr. A. Mawson, Wiggins Teape, UK

I wish to return to the question of revolutionary change, discontinuities, that we began discussing. It has been proposed that the universities might be the ideal germinating ground for the seeds of such changes, and also that collective

government funding actually acts against the stimulation of revolutionary change. It is indeed true that large government finance (e.g. the EEC) tends to go into the collective interests of the industry, where collaboration presents no threat, and that this tends to promote the status quo. But in the U.K. money for R & D is being put increasingly into specific companies within an industry, which by helping to avoid the problems of confidentiality, opens the possibility of more revolutionary changes. This approach can, of course, give rise to products like Concorde, for which the primary need was never properly identified, and whose spin-off products weren't sufficiently immediately useful to be widely adopted.

The question of whereabouts to find the most fertile ground for revolutionary change has concerned several speakers here, and surely the need for cross-fertilisation must be apparent. But if this cross-fertilisation is to occur within a committee, it must be a committtee of one only, and of course modern specialisation renders such committees very unlikely to have the necessary range of experience. The main problem, therefore, I see as being one of tapping existing sources of knowledge, mainly in universities, which is a slow, laborious job. I myself am trying to undertake it and I seek suggestions as to how I can improve my technique

Dr. J. Colley, APPM Ltd., Australia

Yesterday Professor Judt called upon the Research Institutes of the developed countries to do more work for the benefit of the developing countries. Could you briefly outline the extent to which the CSIRO Division of Chemical Technology answers this call.

Dr. Michell

The CSIRO Division of Chemical Technology has been engaged for some years in the assessment of the pulping qualities of woods from Papua New Guinea and Malaysia and in advising these countries in their negotiations of chip export contracts with pulpwood buyers from the developed countries. The work has also included assessments of the potential of possible re-

afforestation species. The work is being done in collaboration with the forest departments of the countries concerned and has included training programmes for their personnel.

The work was funded initially by the Australian government through the Australian Development Assistance Bureau but more recently funding has been provided by the governments of the participating countries.