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EFFECTS OF FUNDAMENTAL RESEARCH ON PULPING

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Abstract

Considering the very small amount of fundamental research, that is to say scientific investigation conducted with no particular objective, carried out in the past in the subject of pulp and paper, it is pointless to try to discuss its impact. If, however, the term is widened to include orientated research, then it becomes possible to elucidate its impact on breaks-through in pulping.

Numerous examples are presented to illustrate the relative importance of fundamental research in the achievement of technological breaks-through in such areas as outside chip storage, carbohydrate stabilising additives in alkaline cooking, delignification promoting additives in alkaline cooking, two-stage sulphite cooking, oxygen bleaching, displacement bleaching, and steam drying.

The findings indicate that empirical experimentation as well as the artistic combination of existing knowledge, often from different disciplines, has been most effective in bringing about technological breaks-through. Fundamental research has also been quite effective when it has been directed towards the attainment of a specified objective. It seems to have been least effective when it has been primarily either explanatory, to set a recent finding in its proper background, or repetitive, to discover how a finding made elsewhere can be applied to local conditions.

All high quality fundamental research will give rise to new data for relevant system properties, and to new models representing the best available descriptions of the systems under consideration. This helps others to make future technological breaks-through, perhaps through empirical approaches, but often through the proper combination of knowledge from various fields.

The technical strength as well as the competitiveness of the industry will in the future, as in the past, be strongly dependent on the extent and effectiveness of the research which is undertaken. It is therefore essential for the future of the pulp and paper industry that R & D activity be maintained at a very high level.

Introduction

Fundamental research is, strictly speaking, scientific investigation conducted without a particular objective. In my judgement, we have in the past carried out very little fundamental research in the field of pulp and paper. If, however, the term is broadened somewhat in the applied direction, then the amount of research activity that can be counted is considerably increased. This is particularly so if relevant work within related fields is also included.

It is thus pointless to discuss the impact of fundamental research on developments in pulping if only fundamental research proper, that is to say according to the definition above, within the strict pulp and paper field is to be considered. Expansion of the definition to include orientated fundamental research does, however, make the task relevant. In the following discussion, the term fundamental research will therefore be taken to include orientated fundamental research. In the following sections a number of developments within the subject of pulping are described, and the extent to which fundamental research has been causal is discussed.

In a concluding section, the relative importances of various R & D activities on developments in pulping are discussed. The cases presented are taken as illustrations of what has happened in the past and as a personal prognosis for the near future.

Recent Developments in Pulping, and the Extent to which Fundamental Research has been Causal

1. OUTSIDE CHIP STORAGE

The storing of pulp wood chips in large piles outside instead of in small covered silos was introduced in Sweden in the early sixties. The increasing cost of wood storage at the mill and the gradually increasing rates of consumption of chips made it imperative that a new storage system be devised. Outside chip storage (OCS) was the solution, devised by the machine manufacturers in response to pulp mill demands. Certain problems that were encountered at an early stage necessitated R & D work in their solution. Examples of these were: avoiding local over-heating: assessing minimum storage times necessary to allow for sufficient modification of the resins to avoid problems in sulphite mills: minimising both loss of substance and discoloration by working out suitable strategies for building up and reclamation from the pile, etc. In all these areas R & D has been of value in establishing and optimising OCS in different local situations. New strategies for proper building up and reclamation, which resulted from development work carried out by machine manufacturers, have been particularly valuable. Fundamental research has also played an important part in achieving a better understanding of the chemical reactions involved, especially in connection with extractives. Additives to control the problems of loss of dry substance and discoloration have been discovered, often, as in the case of manganese ions, empirically, and brought into use.

In the last few years, the high capital cost of large scale wood storage has resulted in a gradual shortening of the storage time in kraft mills. For the extractives to be sufficiently seasoned, sulphite mills must, however, accept a minimum storage time of three months. A new process has been developed by MoDo for removing the majority of the resins from sulphite pulps. This will eliminate the need for more than a short, 1 to 2 weeks´, buffer storage. The basis of this process is a kneading - compression action on the chips in an alkaline medium, followed by washing. To achieve this development, previously available background knowledge of a fundamental nature was combined with available machine designs. One or more clever individuals accomplished this synthesis by skilled combination of knowledge from various fields: let us call them the artists.

2. ADDITIVES PROMOTING DELIGNIFICATION

During 1976 evidently strictly empirical development work was undertaken by Holton's group at CIL in Canada, which resulted in the discovery that small amounts of anthraquinone added to the cooking liquor not only increased the pulp yield but also drastically improved delignification. The discovery of this ideal additive was reported early in 1977(1), and was in certain respects based upon findings of Bach and Fiehn published in 1972(2). The new results stirred up exceptionally great interest among R & D workers and much effort has since then been put into finding better additives and explaining the underlying mechanisms.

In this case there is no doubt that the break-through was achieved empirically and was followed by R & D work at various levels of sophistication aimed at explaining or improving it in some way.

3. TWO STAGE SULPHITE COOKING

Among the great variety of sulphite processes, consider the two-stage process utilising neutral sulphite in the first stage and acid bisulphite in the second. This process shows an improvement in yield over conventional processes of some 5 - 6 %.

The process was already known and in commercial operation in Sweden in the early fifties. It was developed through empirical investigations aimed at finding ways of utilising pine and tannin damaged spruce in the sulphite process. Many attempts were made through fundamental research over a period of ten years to find explanations for the observed higher yield and for the ability of the process to pulp not only spruce, but also pine and inferior spruce. Not until 1961 when Meir(3) demonstrated the existence of alkali-labile acetyl groups in native glucomannan could Annergren and his coworkers(4) explain the effect first observed empirically ten years earlier.

4. OXYGEN DELIGNIFICATION

The delignifying power of oxygen in an alkaline medium was discovered by Nikitin(5) and his research group in Soviet Russia. But because of its poor selectivity their method gave only a slight delignification. Robert and his associates(6) in France disclosed in 1964 their discovery that the addition of magnesium salts considerably improved the selectivity. This started intensive R & D activity culminating in the first commercial operation some six years later in South Africa. Judging by the character of the original article, the original French break-through seems to have been based not on fundamental research but on the endeavours of an artist or empiricist.

From the inception of the idea it took four years until the break-through, and then a further six years before the commercial operation was established. Today, ten years later still, many installations are in operation and it is expected that during the eighties the process will be used to an ever-increasing extent.

5. DISPLACEMENT BLEACHING

Conventional bleaching processes are comparatively slow, with reaction times of from one to four hours. Even fifty years ago it was known that bleaching times of about ten minutes could be achieved by maintaining the concentrations of the active chemicals at a high level throughout the reaction, and that this could be achieved by continuous displacement of the liquor surrounding the fibres by fresh bleaching liquor. Rapson[7], in his publications from 1965, revitalised these ideas, calling them Dynamic Bleaching. Later Kamyr developed his ideas and the first displacement bleaching plant began operation in Texas in 1975. The new process enables chemical pulps to be bleached to 88% brightness in sixty minutes, with low effluent volumes, low energy consumption, and low space demand.

The basic break-through in this case seems to have been an artist's accomplishment. Its practical realisation involved many phases of straightforward development work in machine design and in pilot plant operation. In addition fundamental research was required to solve specific problems, e.g. of the fatigue strength of machine elements, and of the hydrodynamics of strongly sheared non-Newtonian liquids containing cellulosic fibres.

6. STEAM DRYING

Drying pulp in a conventional system requires about 2.8 GJ per tonne of evaporated water, that is to say 3.3 GJ per tonne of dry pulp (assuming the dry content of the pulp entering the drier to be 43%). In 1974 a new drying method was invented in Sweden, whereby fluff pulp transported in a stream of superheated steam is dried indirectly by steam at a somewhat higher pressure. The water evaporated produces low pressure steam and, provided that this finds a use in the mill, the net steam consumption can be reduced to 1 GJ per tonne of pulp: this is a very substantial energy saving.

This technique has recently found an application in the pre-drying of biomass material for use as fuel.

The basic idea behind the development is simple and elegant, and probably the result of an artist's conception. The transfer of the idea to a commercially acceptable technique called for a considerable amount of research, both fundamental and applied.

Relative Importance of Research Work of Different Types

In the preceeding section it has been demonstrated that the conception of a new idea or the experimental finding which constitutes a technological break-through, is, in the majority of cases, the result either of a skilled combination in the mind of an artist, or of a series of well-planned empirical experiments.

The artist in his ability to combine, like the empiricist, relies to a large extent on the body of knowledge which research, particularly fundamental research, has built up. We may picture this body of knowledge as a number of different mountains from whose surfaces technological breaks-through rise up as masts. The picture may demonstrate the importance, sometimes, of combining knowledge from different disciplines (mountains) to make breaks-through possible. Following the erection of such a mast, research will widen our knowledge and thus eventually enlarge the mountain up to the top of the mast. The new level will form the basis for new breaks-through at a later date.

Fundamental research thus plays an important role in the process as a whole, by creating the necessary background of knowledge for others to have new ideas and achieve technological breaks-through. At times fundamental research has been causal and has had a direct influence on developments in the field of pulp and paper. This has been particularly the case when the research has been directed towards attaining specific objectives. Fundamental research has been least effective when either explanatory, or repetitive, trying to establish how findings made elsewhere are adaptable to local conditions.

Fundamental research will, as mentioned above, play its most important role in building up new data for relevant system properties and new models representing the best available descriptions of our knowledge in various, related fields. This helps others to make technological breaks-through, perhaps through empirical approaches, but often through the proper combination of knowledge from various fields.

For example, new knowledge with regard to the properties of synthetic polymers is of the greatest importance for the continual improvement of our understanding of the mode of dissolution of lignocellulosic-based polymers during pulping. And again, continual refinement of our understanding of the hydrodynamics of fibre-water mixtures is of great importance in the design of mixers and pumps, especially of those operating at high consistencies.

As for the future, it is most likely that the pattern of the recent past will prevail. The technical strength as well as the competitiveness of the industry will in the future, as in the past, be strongly dependent on the extent and effectiveness of the research which is undertaken. It is therefore essential for the future welfare of the pulp and paper industry that R & D activity be maintained at a very high level. Background knowledge must be built up, systematised, and publicised. This is achieved most readily by maintaining a proper balance between fundamental and applied research. In addition to the above objectives, researchers should also collect corresponding information from all over the world. Applied research operates mainly by transferring new concepts, by way of the pilot scale, to eventual commercial operation.

For technological breaks-through and the creation of new ideas empiricists and artists are essential. A substantial part of any research budget should consequently be spent on empirical investigations as well as on the development of new concepts, through artistic and innovative combinations of knowledge from different fields or even disciplines.

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Transcription of Discussion

Discussion

Discussion following papers given by Prof. Hartler and Dr. Wahren

Dr. J.Mardon, Omni-Continental, Canada

Professor Wahren's talk, as printed, will become a much-quoted secondary source and therefore I think should be as accurate as possible. I would like to correct a few of his mistakes and rectify some of his omissions. There are three headings under which my comments fall.

Firstly, factual: the name of the company in which Borje Wahlström worked when we made the contributions Dr. Wahren alluded to in his text has been corrected in his addendum. It was the joint research company of two newsprint companies, under the direction of Dr. K.C. Logan. I was in charge of paper-making and Wahlström worked for me.

Secondly, omissions: the most important paper on the subject of taper flow distribution, given in Tappi, 1963, has been noted in Dr. Wahren's errata. There is a good description of the development in Truffit's monograph, also referenced by Dr. Wahren, but Truffit's part in the original team working on this problem has been overlooked. Nor was it mentioned that the same monograph, as its acknowledgement shows, was one of the products of many years' co-operation between several people.

There is also a serious omission on the subject of table roll drainage. Sir G.I. Taylor's paper of 1958[(1)], in which he propounds his mixing theory and derives his expression for the maximum developed suction ($1/2\ \rho V^2$), has not been referenced. This same important paper also contains the theory of foils. The important paper in Papper och Trä[(2)], to which Sir G.I. Taylor wrote a long foreword and which he carefully reviewed, is likewise not mentioned. This same paper gives an experimental result more in agreement with Sir G.I. Taylor's theory than any other had shown until then.

When discussing the question of perforated headbox rolls, Dr. Wahren has omitted any discussion of the wake effect, which must be the most serious problem in modern headbox performance.

Lastly amongst his omissions, Dr. Wahren has failed to give due attention to the paper of Heikki Pellinin where the theory of rewetting is very clearly expounded (no ref. available for this).

Finally, I would like to say something about the philosophy of research, and how it actually works.

Several of the people mentioned by Dr. Wahren, namely B. Howe, A.D. Truffit, G. Gavelin, A.B. Truman, P.B. Wahlström, Sir G.I. Taylor and myself, were, and to some extent still are, in contact with one another and with others active in developments not considered in the paper. Anglo Paper Products was in fact the start of an invisible college rather like that of the nineteenth century inventors or the sixteenth century sailors, that endures to this day. It succeeded so well because Dr. Logan had the talent of hiring outstanding people and of leaving them alone, however much they disagreed, to get on with the job. Dr. Wahren does allude to the importance of fighting an idea through to its acceptance. The importance of this has already been referred to here, and undoubtedly will be again, but I wonder how many people really know what it means. In the Great Hall of Kings' College hangs a portrait of Sir Francis Walsingham, Drake's letter to whom after the assault on Cadiz might well be used as a lesson on this.

Lastly, Mr. Chairman, I would like to suggest that, following this conference, a study be made of the history of the Inverform development. R.J. Thomas is already dead, and I think the least that should be done is to give him some posthumous award. Fortunately George Curry and Brian Attwood are still alive, so first hand evidence is available for such a work. Thank you.

Dr. D. Wahren, IPC, USA

Thank you Dr. Mardon. I have no criticism of your history, but would like to say in my defence, that I had no intention in my paper of trying to be complete. Some 500 of the references I found I simply discarded, either because they were not in the few fields I decided to look at, or because they were not firsts. I keep 60 or 70 references in my files on pressing, just to keep up to date. These I have not quoted because they were not part of the main development of the subject. My paper is intentionally very incomplete. But I agree with you entirely that the people responsible for developments, such as the Inverform, must record what actually happened. In preparing my paper I was very suspicious of company records. May I reiterate Dr. Mardon's suggestion that somebody write the history of the Inverform development.

Dr. H.F. Rance, Chairman.

Thank you both. I fancy there is a challenge there to Brian Attwood.

Professor R. Kerekes, Paprican, Canada.

In addition to presenting new information and reviewing old, these symposia can serve a useful purpose in charting avenues for future research. Could each of the authors please cite a few areas where they feel a need for knowledge exists.

Dr. D. Wahren.

If I could give you a really hot tip, I wouldn't. However, what happens to gaseous phases present before the nip? Most must be expelled on the way in, but some must remain to be dissolved under the elevated pressure. Similarly, gases in solution beforehand must to some extent be given off when pressure is released. Then again, what happens to the stratification that one knows must exist, of fibres, water, air and felt, before the nip? This is one of the areas I have been studying, and it really doesn't seem that the system is always stratified. I would very much like to see someone do some really clever analysis of this. If we assume, with Alfred Nissan in 1954, that there are really only water and fibres in the press nip, then we can give up doing any more research on wet pressing.

Dr. H.F. Rance.

I don't know whether that's a hot tip or a challenge.

Dr. A. Ibrahim, AccuRay, USA.

Dr. Wahren's paper about some of the fundamental research that has lead to breakthroughs in the industry interested me greatly. In my experience though, it seems that, often, improvements intended for use on high speed machines are used indiscriminately on low speed machines also, to their detriment. I would appreciate Dr. Wahren's views on this.

Dr. D. Wahren.

Looking at this problem in the terms of set theory, with the need for knowledge constituting the universal set, the background, then existing applications and existing knowledge form overlapping, but not identical sets. Our existing knowledge does not always overlap with our application, and so we have a need for knowledge which does not call for further research, but more probably for education.

Dr. N.K. Bridge, PIRA, UK

The objective of this second session was, I believe, to illustrate the value of "fundamental" (Mr. Place's extrinsic) research, and so perhaps draw conclusions about how to select and manage good projects.

I think I have identified three criteria which are important in deciding which fundamental research is likely to be useful, and I would like to hear the views of the speakers.

Firstly, fundamental research must be clearly directed towards a particular need.

Secondly, we need as much as possible, since there will always be only a small proportion that is useful.

Thirdly, and perhaps most importantly, only the very best people should be entrusted to do it.

This afternoon's speakers have demonstrated how useful some fundamental research has been, but we are all aware of how much goes on that is less valuable.

Dr. D. Wahren.

Of course you are right, though I believe there is still hope. My philosophy is that it would be entirely wrong for me, even if I "know" which experiments to perform and how, to give anything but general guidance. We must of course arrange to have the best brains working on the problem, but most importantly, we must arrange that they are really aware of the needs of the industry. Only by such awareness on the part of the research staff shall we reap rewards from our fundamental research effort.

Professor N. Hartler, RIT, Sweden

To guard against excessive, useless fundamental research, I agree that we must make use of the best brains available. However, it can still happen, despite all our efforts, that the final results are of no particular use. When this happens, we must remember that though there may be no immediate application, our work may well be valuable in the future. In this way, I believe a very large proportion of fundamental research eventually turns out to be useful.

Dr. H.F. Rance.

From the chair, I would like to add that it seems to me that a lot of apparently useless fundamental research adds significantly to our pool of knowledge.

Dr. E.Bohmer, The Norwegian Pulp and Paper Research Institute

On the question of semantics, Mr. Chairman, I would like to say that we all heard Mr. Place describe how there is no point in discussing the terms `fundamental´ and `applied´ research. It seems to me that this afternoon´s speakers have amply demonstrated his point.

According to the definition of the American Research Council, I don´t think anything that has been discussed this afternoon properly belongs to the field of `fundamental´ research. Consider, as an example, the question of the filtration process on the paper machine. The goal of this research was the improvement of drainage and it was successful. But it could only have been truly fundamental, as Sir G.I. Taylor's work was, if it had been presented as a general theory covering the distribution of liquid films on rotating surfaces.

This is why I think the terms `extrinsic' and `intrinsic' to be so much more appropriate.

Professor B. Steenberg, RIT, Sweden

I want to take issue with Dr. Bridge. I don't believe that more research is better on the assumption that statistically one day we must get better results. My reason for saying this is that, as we heard in the first paper this morning, research runs in fashions. If research activities were independently selected then Dr. Bridge would be correct, but in practice the research community works in Markov chains, which frequently do not follow useful paths.