The case for national research in mineral processing

PRESIDENTIAL ADDRESS

given by R. E. Robinson*, Ph.D., B.Sc. (Eng.) (Wits.)

Some of you have only raised your eyebrows, some have made tactful comments, and a few of you have been fairly outspoken, but all of you in industry are concerned at the high level of State expenditure by bodies such as the National Institute for Metallurgy.

The case for a national research organization in mineral processing is not clear-cut and in fairness I must present both cases — for and against.

It is difficult to do this simultaneously. As W. S. Gilbert has put it, it's

a rather serious crime
To marry two wives at a time,

and, to ensure that

From bias free of every kind
This trial must be tried,

I am going to adopt a somewhat unusual form of presentation — unashamedly cribbed from *Trial by Jury*, the first Gilbert and Sullivan operetta, which was performed for the first time one hundred years ago.

I ask you, therefore, to imagine that you are not listening to a presidential address, but that you are in a court of law listening to a case for the prosecution and a case for the defence. To begin with, I ask you to picture me as a rather elderly, cynical, and perhaps somewhat sarcastic, public prosecutor, who is summing up the case for the prosecution. I shall therefore eschew the traditional introduction 'Mr Chairman, Ladies and Gentlemen' and begin by addressing you as 'M'Lud, Gentlemen of the Jury'.

THE CASE FOR THE PROSECUTION

With a sense of deep emotion,
I approach this painful case

It is my sad duty today to sum up the case for the prosecution against a group of people who are perhaps rated among the elite of our society. It is a serious accusation — nothing less than fraud — and the people concerned are the scientists of our national research laboratories. I believe that their fraud has been neither malicious nor premeditated, and possibly unintentional; in fact, they may not have recognized that their actions could be described as criminal. I am certain that the presentation of the evidence will have brought forth cries of 'righteous' indignation from the accused, who, I am sure, are totally and sincerely convinced that their actions have been correct and beneficial in the extreme.

But the evidence is irrefutable, and to reveal their fraud has required much detailed study and investigation, since their crime is a subtle one, and the subtlety lies in the facade of technical jargon and mathematical equations that has been erected by the accused to make it almost impossible for the public to penetrate the magnitude of their fraud.

The accused are, in fact, that group of scientists who, for many years, have been utilizing large amounts of public funds for the conduct of research, which, they claim, has been highly beneficial to the country and the population in general, but which has, in fact, produced very little of material benefit at all.

Let us go back some twenty or thirty years to the time when I believe the fraud first commenced. I might say that the defendants in this particular case in South Africa are not alone in their crime, and that they have many counterparts in most other industrialized, or so-called advanced, countries in the world. In fact, the defendants have merely copied the skilled techniques of others, and the case that I am presenting here is by no means unique. Similar cases are being argued in many other countries now that the fraud has been exposed.

The establishment of these fraudulent empires started immediately after the Second World war. The opportunity presented itself as a result of the work of the scientific community during the war, when it was demonstrated that there seemed to be few, if any, problems that could not be solved by scientific research. The development of radar, nuclear weapons, and guided missiles had made research scientists indispensable to any country concerned with preserving its safety, and we entered a phase that Ronald Clark has aptly named 'The Rise of the Boffins'. A climate existed in which any form of research was good, vital, and essential, and it was universally accepted that no country could hope to progress economically or socially unless it developed its scientific and technical resources to the utmost. In this period, we saw the rapid expansion of research facilities at universities and in industry, and the creation of many new and very large institutes by various States. Many famous and well-known organizations had their origin in this particular period: Harwell, the Oakridge National Laboratory, Saclay in France, and, in South Africa, the CSIR, and, at a later stage, the Atomic Energy Board and the National Institute for Metallurgy.

As shown in Exhibit 1, the growth of these State-financed national research organizations has been phenomenal, and, in an examination of this growth, we can begin to understand how the fraud has been so skilfully and cunningly perpetrated. When first established, these nat-

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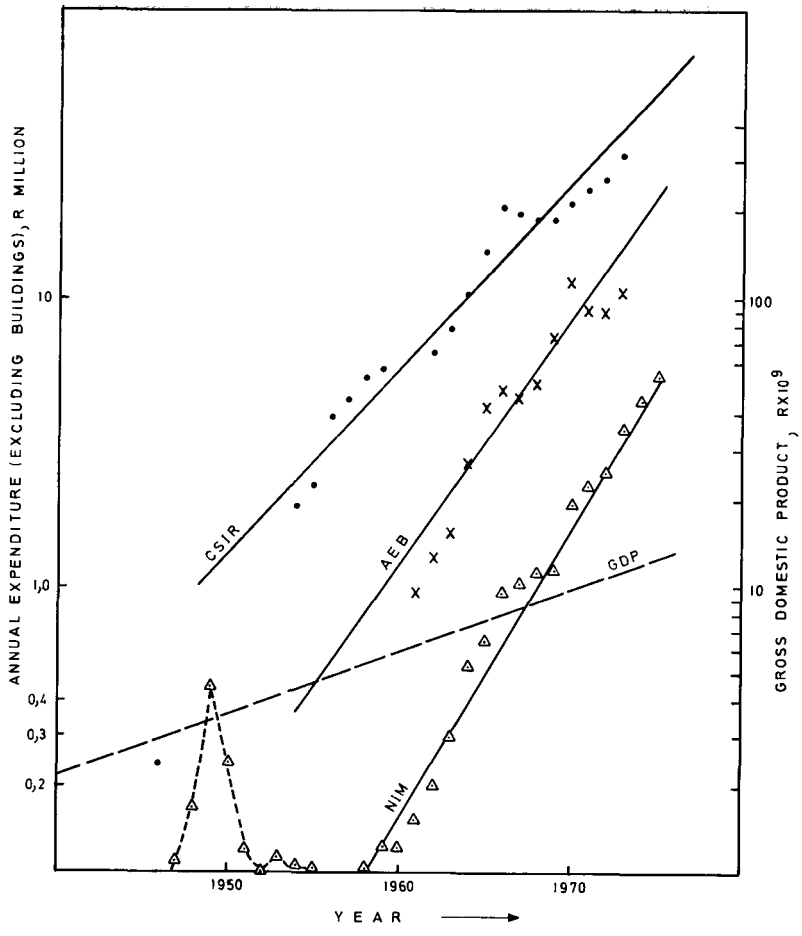

Exhibit I—Expansion of the statutory bodies in South Africa

ional research bodies had reasonably clearly defined and useful objectives, but, in the course of time, these objectives became heavily diluted. It became easy to argue that useful applied work could not be divorced from fundamental work, and, for well-trained scientists, it was easy to find a connection between the original nature of the work and almost any field of scientific endeavour that took their fancy. Thus, to an ever-increasing extent, these national research organizations started delving into a wide variety of so-called fundamental investigations covering virtually every field of chemistry, physics, biology, mathematics, and other scientific disciplines. In the initial stages, the motivations for this fundamental work may have been based on practical and useful objectives, but in the course of time they became more tenuous, and, after a period of years, they were dressed up in such complex scientific terminology that it became impossible for any layman to challenge them. The mere fact that a new field of scientific research became fashionable in overseas countries was in many cases sufficient justification for our scientists in South Africa to jump on the same bandwagon. In this way, the scientists had very little difficulty in making out irrefutable cases for the purchase of the most expensive and exotic equipment, and were allowed to develop the facilities they needed for their work almost regardless of expense. This type of confidence trick was very easy to perpetrate, since there were few persons among the politicians and those officials responsible for Treasury funds who had any hope of understanding the function or useful purpose of fancy pieces of apparatus such as linear accelerators, electron microprobes, and neutron generators. Thus, people who might have controlled this expenditure dared not, in the early stages, refuse these demands for fear of being accused of ultra-conservatism or of retarding the development of the national economy. Once these enormous, exotically and expensively equipped laboratories had been established, it took a brave man, even when the original motivation had disappeared, to refute the argument that one could not possibly allow them to stand idle and therefore it would be well worth while to undertake fundamental research programmes. 'Something of value was bound to come out of them'. Almost invariably this new work required the purchase of the most recent, most sophisticated, and almost equally expensive accessories.

Whether the creation of the enormous scientific empires represents a wilful or unconscious act, we shall probably never be able to determine. I have no doubt that many of the guilty parties began their scientific careers with the sincere and honest desire to contribute to the welfare of the community, but, like the youth of today in their discotheques, they soon became victims of the fascinating effect of the flickering lights of sophisticated scientific equipment, or were drugged by the incredible calculating capability of the computer into believing that a dream world of technological achievements was theirs for the taking.

It was, of course, not only in those scientific organizations that were controlled by the State that this fraud was attempted. No doubt the same tricks were tried in industry, but very soon the danger of wasteful and fraudulent use of money or the employment of idle research workers reached the point at which shareholders' profits and the productivity of the company were affected, and you may be sure that, at this stage, the companies themselves took action to put an end to such wasteful activities. In fact, this is what has been happening during the last five years, as evidenced by the number of Ph.D.'s driving taxis in San Francisco, the dismissal of a third of the research staff of one of the largest industrial research organizations in the United Kingdom, and here in South Africa the closing down of one of our

largest industrial research laboratories and the severe curtailment of the programmes of several others. Industry was well able to look after itself. If a research department did not show a reasonable financial return, the company concerned sooner or later closed it down or reduced it to a reasonable size.

Even at the universities, which conduct a great deal of research (not so much in South Africa, but certainly to a great extent in America and in Europe), there were a number of factors that could help to restore the balance if research activities got out of hand. Universities are still predominantly teaching and training institutions (thank goodness), and their research activities are largely related to the number of research students available and the limited amount of funds that they can glean from industry. Thus, when industry itself believed that research expenditure was to be reduced, the funds made available to universities were also reduced, and in many countries this has had a severely limiting effect on the research activities of the universities. Also, in the disillusioned climate that exists at present among young people regarding science and the contribution that it can make to the welfare of human beings, the supply of graduate students for university research has decreased. In South Africa, this is so marked that, in many university scientific departments, research is almost non-existent.

Unfortunately, these same constraints do not exist in the large State-financed research organizations, whether they are the so-called statutory bodies (CSIR, NIM, and AEB), or the research departments of the Civil Service itself, such as those of the Departments of Agriculture and Water Affairs.

The most important factor that allowed the exponential expansion of these research organizations to go unchecked was the lack of accounting of any kind to measure their effective output. In almost all of our economic activities in industry, a company has to produce something, be it gold, diamonds, fertilizers, or TV sets. Each company has inputs (labour, capital, raw material) and outputs, (products, services, etc.), and the balance between input and output determines whether the company is profitable or whether it goes bankrupt. Even at universities, one can develop a similar input and output equation in the research field. A department takes in a given number of undergraduate or post-graduate students and produces trained engineers and scientists with Bachelors', Masters', or Doctors' degrees. The job opportunities, and the quality and performance of these graduates, can be assessed and can be used to determine whether or not the university is performing its function successfully.

In the case of the national research organizations, the situation is almost unbelievably open. The organization of these laboratories has been so cunningly designed that few people have any hope of determining what the useful output really is. Very few people can understand in depth the content of the many reports and research publications, let alone evaluate the contribution that these collections of paper make towards the community. It is therefore almost impossible to judge whether the State or the country has obtained good value, or even any value at all, for the money that it has invested in research activities.

Once a scientist has had a research budget approved, little further accounting is expected of him. In very few instances are restrictions imposed on the amount of time he takes to conduct his research, and it is almost impossible to measure his progress. He could work at full pace or half pace, and no one would really know the difference. He could cover up any mistakes he might make in a complex jargon of technical explanation that no-one but the specialist would dare to challenge. He is virtually a law unto himself.

Thus we are faced with a situation in South Africa in which by far the greatest proportion of the money being spent on research is spent by our national research bodies. (I should like to refresh your memories with the remarkable figures that apply, as given in Exhibit 2.) It will be seen that, in those countries for which figures are available, the proportion of total research expenditure that is spent at State research organizations is much lower than it is in South Africa. The discrepancy is in fact enormous, and it would not be unfair to say that State research in South Africa is at least double that in most other comparable countries. As far as the efficiency of this work and the value of the output from these State research organizations are concerned, we have little or no control, and certainly no quantitative measure, and this for well over half the country's research activity.

You might well pose the question as to whether there is any output at all, and in the majority of cases one can give the answer only in terms of scientific reports and publications. It is, in fact, these publications that the scientists themselves

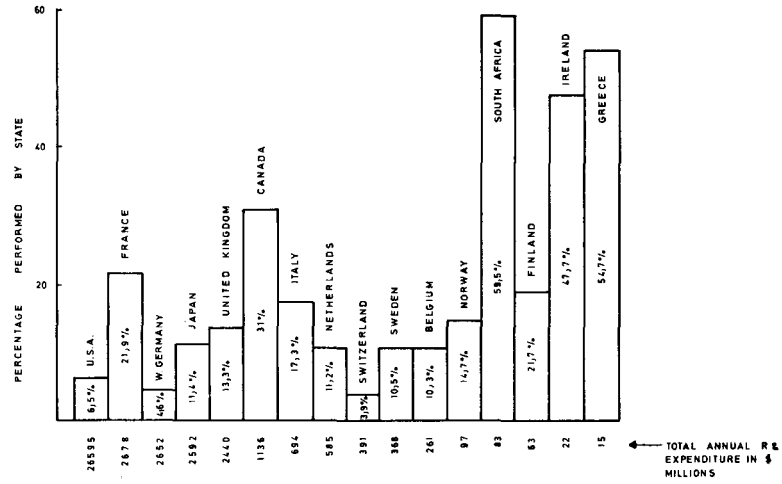

Exhibit 2—Proportion of national research effort performed by the State (based on OECD[1] and CSIR[2] surveys)

use as a measure of their abilities. In many research organizations, promotion and salary advancements are based on the magnitude of a scientist's publication record, and the first question asked of a scientist when he applies for a position in a research organization is very often 'How many papers have you had published?' 'Publish or perish' is a common slogan in this community, and, in a cynical vein, one wonders how many costly research projects have been selected on the basis of the project's publication potential, with only a thinly disguised attempt to relate the work to a worth-while contribution to the national economy.

One has the utmost difficulty in assessing the real value of a publication today. In the good old days, before the research explosion took place, the number of scientific journals was limited, and, in general, they were of very high quality. Editorial standards were severe, and the majority of scientific papers published were subjected to rigorous criticism, usually at an open meeting of a highly esteemed scientific institution. Woe betide the scientist who proposed a new theory that could not be wholly substantiated by first-class experimental results. Does the same atmosphere of severe criticism exist today? Unfortunately not. The publication of technical journals has proliferated to such a degree that it is now impossible to subject even a minute fraction of the published work to the rigorous criticism of the scientific community as a whole. One is faced with such enormous numbers of low-quality, and often blatantly incorrect, experimental data and pseudo-scientific conclusions, that it is becoming impossible to separate the good from the bad, and in many instances it is quicker and cheaper to repeat experiments than to review and select the acceptable information from a mass of rubbish.

A question you may ask is why the scientists themselves do not take severe action to restrict the publication of trivialities. Surely they can arrange among themselves to edit publications severely and to reject debatable or unfounded conclusions? I am afraid that the answer to this question is a rather cynical one. I believe that most present-day scientists have abandoned their ruthlessly critical approach to one another's work under the pressures of internal and international politics. At the many conferences that are held, one rarely hears or encounters ruthlessly critical comments. In any event, at most of these conferences, only a very limited time is allowed for discussion and criticism, and the best one can do during the bun-fight of multi-parallel sessions that are commonplace at most conferences is, perhaps, to ask a question or two.

In all sincerity, I sometimes wonder whether the vast number of international scientific conferences held today are organized with the object of undertaking a serious scientific debate and scrutiny of recent scientific research work, or whether they represent, as I suspect, a mammoth confidence trick on the part of the scientific community to ensure that they enjoy their fair share of the delights of international travel that are so much a part of life today.

Thus, Gentlemen of the Jury, I have summarized the elements of the gigantic fraud that is being perpetrated today by these so-called scientific research workers. Having had almost unlimited funds made available to them for the pursuit of those projects that they have always maintained are so vital and essential to the country's future development, they are now being called upon to demonstrate the benefits resulting from their endeavours. We find that they are not able to do so.

This gigantic fraud is being exposed in many countries. The public is no longer as starry-eyed as it used to be about the benefits of science, and people are now asking for a more positive demonstration than a mass of publications. Most of our national research organizations have been in operation for a long time, and, although I concede that they have made some contribution, the basis of the case for the prosecution is that their contribution is far from adequate in relation to the expenditure that they have incurred. We are often bombarded with dramatic announcements of scientific breakthroughs, but where, in fact, are the contributions to our economy that we have been promised for so long? Can we honestly say that they have made a contribution to the standard of our living, or to the solution of the many social and other problems that beset this country? In spite of all this research work, we are still critically dependent on technology that has to be imported at fantastic cost in the form of royalties, licence fees, and restrictions on the selling of our products overseas.

I submit that, on considering all the evidence, you can come to only one verdict, a verdict of *guilty*. In a similar case overseas, in a court presided over by Lord Rothschild, such a verdict was pronounced. Here, in South Africa, we cannot afford to have professional engineers and scientists indulging themselves in unproductive activities in the name of research. The country needs them far more urgently in productive operations in our mines, plants, and factories.

I suggest therefore, M'Lud, that, if the verdict is one of guilty, a most severe sentence be imposed; that the accused be taken from their ivory-towered research laboratories, and be committed to a life sentence of hard labour in the mines and plants of industry.

INTERMISSION

In radio programmes, and also, I am sure, in our television service in due course, it is usually at a dramatic point such as this that a break is called to enable the sponsors to do a bit of advertising, and for the audience to make themselves comfortable and prepare for the next exciting instalment. I am afraid I can do little more than to ask you to shuffle a bit, and, when you have resettled, to remove the image of the cynical prosecutor from your mind and attempt to readjust yourself to a rather fantastic flight of fancy. You now have to picture me as a young, handsome barrister, something like the Perry Mason that I am sure you have all read about, who steps forward with the confidence and assurance appropriate to all heroes of court dramas to summarize the case for the defence.

I grant you that this is going to stretch your imagination to the full.

THE CASE FOR THE DEFENCE

For, permit me to remark,
On the merits of my pleadings
You're entirely in the dark.

M'Lud, Gentlemen of the Jury, I am here to represent only one of the clients that have been subjected to these severe accusations, but I am sure that the defence I present will be valid for most of the other accused. I represent the National Institute for Metallurgy (or NIM as it is commonly known) and all the research scientists in that organization, and I shall attempt to show you that the case so dramatically presented against them is totally and completely unfounded. I have no brief to plead on behalf of the others, but I am sure that, in due course, a very strong case will be put forward by their own defence counsels.

If we remove all the flights of oratory that were evident in the prosecutor's address, we can boil down the case against them to two main accusations.

Firstly, they have been accused of empire building, by making use of the favourable climate for scientific research that existed after the Second World War. *Secondly*, they stand accused of fraudulently and wastefully expending public money, and we have been told that the benefits of their research are in no way commensurate with this expenditure.

Before I deal with these accusations in detail, I should like to point out that part of the evidence presented by my learned colleague must be disregarded. He has made much reference to statistics, facts, and information that are applicable to overseas countries. Whether or not these facts and statistics are correct is beside the point; the situation in countries outside South Africa has very little relevance to the situation that exists in South Africa, and I therefore submit that this evidence is inadmissible. We are very prone in this country to look to overseas countries for statistical patterns that we can use to provide a model for the control and organization of our own scientific activities. This is an illogical approach, because nowhere else in the world are the problems and conditions similar to those that exist in South Africa. On the political and social front, our problems are unique, and there is very little inclination on our part to look to overseas countries for advice and guidance. Why, therefore, do we have to do this on the scientific front?

Is there any other country in the world in which such a high proportion of the national income is derived from only two areas of activity — agriculture and mining?

Is there any other country in the world in which the balance of payments is so dependent on one single product — gold?

Is there any other country with a population that has the same proportion of various racial groups, and in which the responsibility for providing professional scientific expertise has rested with only one of these groups?

Is there any other country in the world that has the same potential in material resources, or has the same critical need to develop its economy as rapidly as possible in an attempt to solve its social and political problems?

Comparisons between the research expenditure of countries such as the United States and the United Kingdom with that of South Africa are hardly valid, because the differences are so profound. This is particularly so when one considers the balance of research expenditure between industry and the State, where the evidence submitted suggests, at first sight, that we are completely out of step with other countries. I hope to be able to show you that this particular situation is, in fact, an extremely logical one for South Africa, with its own peculiar scientific and technical problems, and I can see no sensible reason to assume that we should conform to the patterns of other countries.

Empire Building

The first accusation of empire building during the favourable climate that existed after the Second World War can hardly apply to my particular client, as far as timing is concerned. NIM was in fact formed long before the Second World War. The birth of the organization was as long ago as 1934, and, like Onderstepoort, it can claim to be one of the first of the national research organizations. It is interesting to record that it was created at a university, the University of the Witwatersrand, where Professor Stanley, Head of the Department of Metallurgy and Assaying, promoted the formation of the Minerals Research Laboratory, which, after several name changes, developed into the National Institute for Metallurgy. The small Minerals Research Laboratory grew so rapidly, and the projects it was working on were of such importance, that it soon became clear that the university could no longer, on its own account, manage to contain the infant, and it invited the Department of Mines to act as the co-parent in the form of a financial sponsor for the erection of special buildings adjacent to the university. So it was that the Government Metallurgical Laboratory was formed in 1944 as a joint undertaking between State and University. Please note, the State was *invited* to take over the financing of the particular organization, because no industrial firm nor the University had the resources to do justice to the importance of its work at that time.

The new Minerals Research Laboratory was formed with the intention that it would work on all aspects of mineral processing. It is fascinating to read of some of the early projects undertaken by this laboratory.

In 1934, for example, work was started on the recovery of andalusite from the western Transvaal, and samples were submitted to overseas users. A large refractory raw-material industry has developed from this early work. Also in 1934, work was started on the 'pyroxenite-apatite rocks of Palabora', and the flotation process for the recovery of apatite was investigated.

In 1935, work started on the production of magnesite from the ores of the north-eastern Transvaal to enable refractories for the steel industry to be produced from local sources. An investigation was also initiated into the direct reduction of titaniferous iron ore of the Bushveld

Igneous Complex, using coal from a Transvaal colliery. Another investigation related to the reduction of Transvaal chromite in an electric-arc furnace to produce chromium steel, and later (1939) to produce high-grade ferrochromium alloy.

In 1936, tests were conducted on the concentration of fluorspar from deposits in the western Transvaal, and many other investigations on platinum, gold, tin, diamonds, vermiculite, and antimony were undertaken in the period 1934 to 1944.

However, during the war years the broad scope of the work of the Laboratory had to be restricted to strategic raw materials, and, immediately after the war, its work became even more specialized, since it was then appreciated that the ores in the South African gold mines contained uranium. A headquarters was required for a large centralized research programme for the development of a process for the recovery of uranium. The Government Metallurgical Laboratory was chosen, and in 1946 a research programme was begun that must almost certainly rate as the most important ever undertaken in South Africa. Engineers and scientists from the mining industry were seconded to the Government Metallurgical Laboratory, together with some key personnel from the U.S.A. and Canada, and a highly confidential, strategically urgent research programme was initiated, which absorbed virtually all the facilities and expertise available at the Laboratory. Completely new analytical methods for uranium had to be developed — both rapid radiometric methods using the exciting new geiger counters, and accurate chemical methods. Detailed mineralogical investigations had to be undertaken to identify and characterize the new mineral of uranium, ore-dressing tests involving flotation and gravity separation had to be conducted, and work on pyrometallurgy and chlorination roasting could not be excluded. Exciting new techniques in hydrometallurgy were investigated. For example, it was at the Government Metallurgical Laboratory, at its pilot plants, and in industry, that ion exchange, although originating overseas, was first used in a large-scale metallurgical application for uranium extraction.

These were exciting and productive years. That the research programme was successful is well-known, but I wonder if many people realize the full impact it had on South Africa. Quite apart from the value of the uranium produced to date (one thousand million rands) and the probable future value (ten thousand million rands), the fact that South Africa became a large uranium producer gave it world status in the nuclear field. It became a foundation member of the IAEA, and gained an entry to the scientific expertise of the U.S.A., the United Kingdom, and France. Without the uranium programme, we would probably have had no Atomic Energy Board, nor the uranium-enrichment programme, nor would we have reached our present international status as one of the key countries in the energy crisis.

Above all else, the uranium programme gave South African metallurgists and industry confidence in their own abilities. It was realized that, not only was research in South Africa very profitable, but that there were occasions when it was simply impossible to solve our problems by buying technology from overseas, and that South African scientists could hold their own with anyone in the world. Many of the mining groups established their own metallurgical research organizations; the giant Anglo American Research Laboratory, for example, can trace its origin to the uranium days.

However, from the point of view of the Government Metallurgical Laboratory, this preoccupation with uranium was not without disadvantages. Much of its previous work on other minerals had to be abandoned or left in abeyance, and, when the first uranium programme came to an end in about 1956, the laboratory suffered three or four lean years while attempting to revive some of its earlier interests. In 1959, when the Atomic Energy Board took over all the work on uranium, the expertise available at the Laboratory was called upon once again, but at that stage it was decided by all concerned that the organization should never again become totally preoccupied with only one research topic, but should immediately expand its range of interests to all the other minerals produced in South Africa. In line with this concept, the National Institute for Metallurgy (NIM) was created in 1966 as a fully fledged statutory body.

I quote this history at some length to illustrate three main points.

1. There was no question of empire building. NIM's expansion rate from the Minerals Research Laboratory to the present was at all times well below the optimum requirements, in that extremely important work was left undone for many years.
2. The State was the only organization at the time that could assume responsibility for the financing and provision of facilities for the work of the Government Metallurgical Laboratory.
3. The complexity of the uranium programme fully demonstrated the desirability of having integrated research facilities encompassing many different disciplines.

Let us look more deeply into this latter aspect of the complexity of research today as compared with what it was, say, three to four decades ago. I should like to illustrate this by referring to one of the most commonly used techniques in mineral processing, froth flotation.

In the pre-war days, a flotation process could easily be developed on a laboratory scale by one or two workers with simple equipment. All they needed was a small experimental batch flotation cell, a supply of ore, a selection of perhaps half a dozen flotation reagents, and a good 'assayer' (as he was called in those days) to analyse the products. These were also the good old days when a marginal copper proposition contained 2 per cent copper (today the treatment of 0,6 per cent by flotation is commonplace), and when the separation of complex mixtures of the sulphides of different metals was regarded as a very doubtful possibility. Recoveries of 80 per cent were considered excellent, and emphasis was placed on empirical methods in the selection of the correct combination of reagents to produce a concentrate of salable grade. Today, the situation

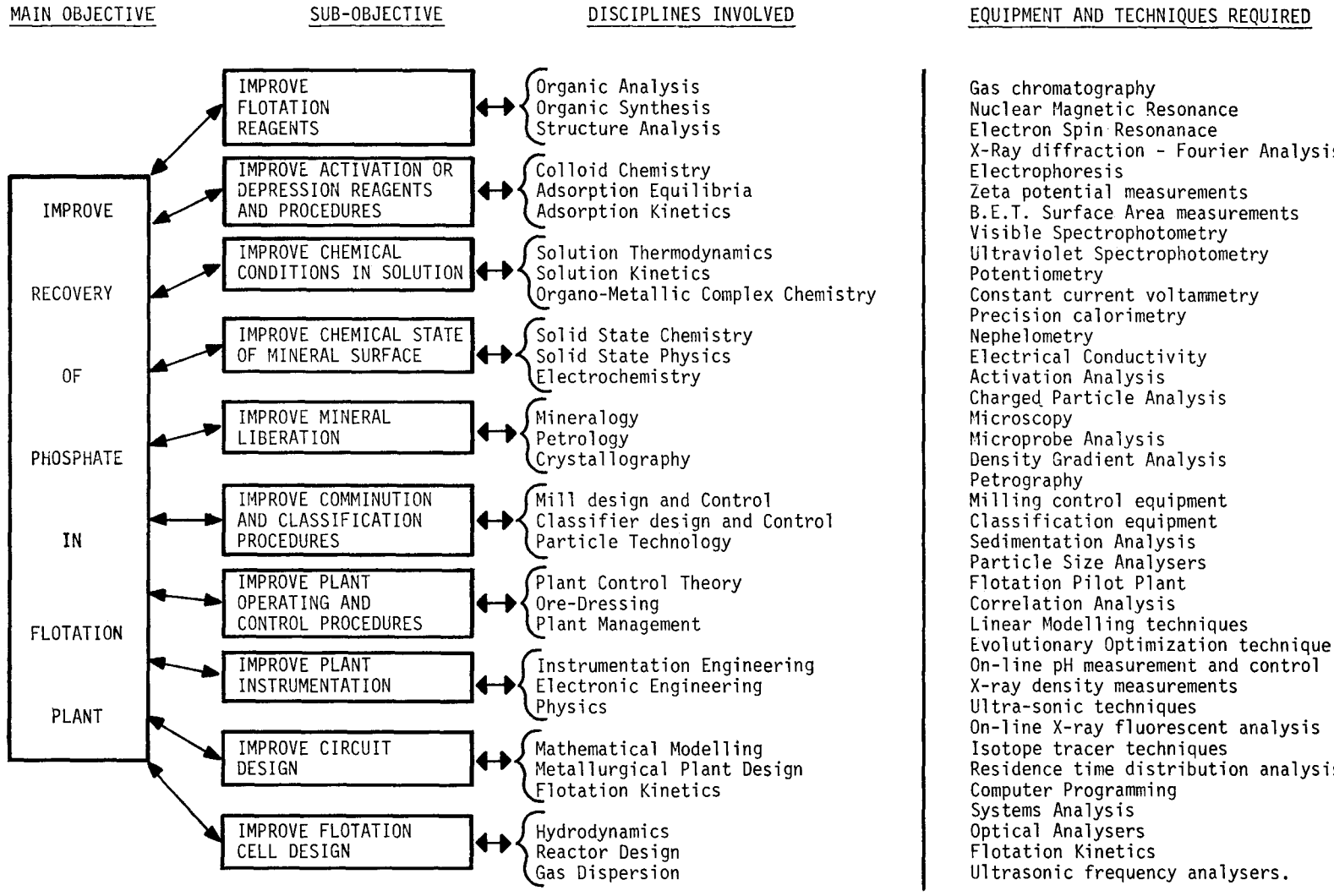

Exhibit 3—Complexity of flotation research

is far more complex. The simple flotation test is probably just as easy to conduct as it was forty years ago, but the problems that respond to this simple approach are very few and far between. One is now striving for the solution of much more sophisticated and complex problems. One has to treat lower grades and more-complex deposits on a much larger scale. An extra 5 per cent recovery probably means many millions of rands per annum, and it is no longer acceptable to allow a small amount of zinc sulphide contamination in a copper sulphide concentrate or *vice versa*.

Exhibit 3 will serve to demonstrate the complexity of modern types of flotation research. It shows the various disciplines, techniques, and equipment that might be involved in a comprehensive research investigation on a typical flotation problem — one, let us say, that attempts to provide a 5 or 10 per cent improvement in a flotation plant. The second column of the table lists the various ways in which the main objective might be achieved, and the third column gives the type of expertise needed to investigate the various possibilities listed in column 2. Column 4 lists the type of equipment and specialized techniques needed to allow the expertise in column 3 to develop the possibilities given in column 2 to achieve the objective in column 1.

I could go further, but this is degenerating into the old game that starts off 'This is the house that Jack built'. You probably cannot even understand all the entries in column 4. Don't worry too much; there are very few people, if any, who could claim to have a comprehensive understanding of all of them. Yet, we cannot afford to ignore the possibility that any one of these techniques or specialized areas of study might provide the vital clue that will enable us to achieve the main object. Or perhaps the solution lies in a combination of several different approaches. A modern research organization must have access to all these facilities if it is to look at flotation problems comprehensively, and, moreover, must provide for the interaction and coordination between the physical chemist, the electrochemist, the mineralogist, the electronics engineer, the computer programmer, the ore-dresser, the chemical engineer, and all the others involved.

What I have hoped to show is that research today is a very much more complex undertaking than it was in the glorious pre-war days of back-room university research. Behind us are the days of string and sealing wax, and absent-minded professors working simple laboratory-bench experiments. No matter how nostalgic we feel about it, those days have gone, and research laboratories, including those of the universities, must have sophisticated apparatus and instruments, and the infrastructure to maintain them. In present-day research organizations, the concept of minimum critical size has been developed, which indicates that, for a research organization to generate new concepts (i.e., apart from the routine investigations), there has to be a minimum number of scientists of different disciplines to cross-fertilize each other, and a certain minimum number of essential items of equipment. This situation is closely analogous to a nuclear reactor, which requires a certain minimum weight of uranium to maintain a chain reaction, and, of course, the appropriate control and engineering equipment to transform the nuclear reactions into useful forms of energy.

This is particularly the case in an organization such as NIM. Let us assume, for example, that a scientist in the chemistry division generates a new thought that leads to the development of a chemical process for solving a mineral-extraction problem. It is vital, if this new process is to be developed into a profit-making plant, that a host of other scientists and engineers should be available to interact with the inventor. One needs, for example, the services of mineralogists, who are highly specialized in locating and identifying types of minerals that can be used in the process. One needs experts in mineral dressing to prepare concentrates of these minerals, which can then be treated by the new process. It is necessary to have chemical engineers and metallurgists to design and develop the equipment on which the new reaction is to be conducted. One needs instrument engineers to provide the necessary control instrumentation, and one must not forget the essential role played by persons specially trained in the economic evaluation of plant processes, who can pinpoint those crucial areas of the processing techniques that might make the overall plant uneconomic and who can thus indicate the vital areas for further technical development. It is precisely this concept of the minimum critical size for interaction and cross-fertilization of many different disciplines that has led inevitably and logically to the formation of the research complexes to which my learned friend has referred. This has not been an act of empire building, but an inevitable requirement brought about by the increasing complexity of technology, which has led to a situation in which no one individual research worker can possibly hope to have all the necessary expertise at his command to convert a new concept into a practical reality.

This requirement is particularly evident in research on mineral processing, and is even more applicable in South Africa. There are no easy problems waiting to be solved in the mineral-processing industry in South Africa. Our image overseas is one of an Alladin's cave containing gold, platinum, diamonds, and uranium in prolific abundance ready for the taking; but those involved in South African minerals know that this image is far from true. We have few, if any, rich deposits: our gold, platinum, diamonds, and uranium are of the lowest grades mined in the world, and require some of the most sophisticated technology in mining and metallurgy anywhere. We are now striving to improve the efficiency of the gold-extraction process from over 98 per cent to more than 99 per cent, and even a 0,1 per cent increase is highly significant — an amount of gold about the size of a pinhead in a ton of ore! Our chromite, although abundant, is of low quality for metallurgical applications. When the copper mine at Phalaborwa was first started, it was the lowest-grade copper proposition

in the world. The new discoveries of base metals in the north-western Cape may be vast and exciting — they are also complex ore-bodies that will challenge our expertise to the full if they are to be fully exploited.

Thus, South Africa has its own special situation — a situation that specifically demands research facilities above the minimum critical size, and I am afraid it is only with State finance that this can be done. To a small country like South Africa, with its limited professional population, research facilities can be regarded as much a part of the infrastructure as our hospitals, railways, and telephone services. The uranium era suddenly brought this realization home, and, rather than consider the rapid expansion an act of empire building, one should regard it as making up for lost time!

Another important aspect of the concept of minimum critical size for a research organization is the desirability of utilizing research results in many different applications.

For example, a process that was originally conceived and developed for zinc processing finds application in improving the recovery of uranium. A chance mineralogical observation made in the examination of a copper ore-body leads to the establishment of a new product for the phosphate industry. Work on new methods of routine chemical analysis leads to the development of new geochemical-prospecting techniques. Fundamental studies on the electrochemical behaviour of minerals and metals lead to a new instrument for controlling the addition of cyanide to the vessels used in the leaching of gold. There are innumerable examples of instances where research on one specific problem has led to valuable contributions in a completely different field. Thus, the effective yield from research activity enlarges as the range of interests increases, and, the broader the programme of a research organization, the greater is the opportunity of deriving the maximum benefits from any given research activity. Of course, it is essential to maintain adequate communication between the different sections and divisions, and the various programmes and projects must not be undertaken in isolation. But all these aspects can be arranged by good research-management procedures if an organization is large enough to have a sufficiently wide range of interests to absorb the fall-out from the various projects. Certainly, a small research laboratory working for a company with a limited interest cannot hope to achieve the same research productivity as that resulting from an association with a bigger organization, where cross-fertilization and cross-utilization can occur.

In overseas countries it is not inconceivable to think of industrial companies and private organizations that are large enough to support a research organization above the minimum critical size. One has only to remember, for example, that a company such as General Motors has a sales turnover that is larger than the whole South African gross domestic product, and that the total South African budget expenditure for 1974 was less than the sales value of the Volkswagen Company in Germany. Our size is well illustrated in Exhibit 4, which, I hope, convinces you that South Africa is a small and unique country, and allows you to realize the problems that faced South African industry at the time the major State research organizations were created in South Africa. In the course of time, the situation has changed, and today some of the larger companies in South Africa are well able to justify the support of a fully integrated and viable research organization. Without any doubt, certain of the research laboratories of the mining groups have taken a well-deserved place among the leading research organizations in the world. However, this does not mean that the State research organizations must now disappear or be reduced in size. Not only can they continue to provide technical support and backing for the increasing number of smaller industrial companies in the country, but they have an additional important role to play in undertaking work that, although it has to be done in the national interest, does not fall into the category of a profit-making activity for any particular company.

I should like to elaborate further on the work of national importance undertaken by the national research organizations, since it is in this field that their existence is best justified, particularly in mineral research.

	Sales value $ billion (10^9)
General Motors	35,8
Exxon	25,7
Ford Motor	23,0
Total S.A. gross domestic product	21,0
Royal Dutch Shell	18,7
Chrysler	11,8
General Electric	11,6
Texaco	11,4
Mobil Oil	11,4
Unilever	11,0
International Business Machines	11,0
International Tel and Tel	10,2
Gulf Oil	8,4
Philips Gloeilampenfabrieken	8,1
Standard Oil of California	7,8
British Petroleum	7,8
Nippon Steel	7,7
Western Electric	7,0
U.S. Steel	7,0
Volkswagenwerk	6,4
S.A. Treasury expenditure	6,1
Hitachi	6,0

Exhibit 4—Comparison of the sales turnover of some major world companies[3] with the South African budget

I believe that it is no exaggeration to say that the industrial research laboratories all have very full programmes, and must inevitably give priority to those projects that are most profitable to the company; in many cases such work must be of a confidential nature. However, there is a vast amount of research work of a long-term character, which can benefit industry as a whole, but which cannot be justified as high-priority work for individual industrial research organizations since it is difficult to forecast that the work will produce additional profits to the company on a short-term basis. A few examples will best illustrate this type of work.

1. The extraction of alumina from non-bauxitic material available in South Africa.
2. The development of methods for the design and scale-up of autogenous mills.
3. The development of design, scale-up, and control procedures for electric-arc furnaces.
4. The development of techniques to determine the properties of coke and carbonaceous reducing agents of importance in pyrometallurgical processes.

5. The development of hydrometallurgical techniques for the treatment of base-metal sulphides so as to avoid pollution by sulphur dioxide.
6. The development of instrumentation and control techniques for flotation processes.
7. The preparation and analysis of international standards for the metals and minerals produced in South Africa.

These are but a few examples from a long list of programmes being undertaken by NIM. The economic advantages of this research work to the metallurgical community as a whole are obvious, and in some instances the programmes can be rated as being of urgent national importance, and possibly even strategic in character. Very few research laboratories belonging to private companies could justify, in terms of shareholders' profits, the long-term effort required to undertake such investigations.

In many respects, such work can be regarded as a contribution to the scientific infrastructure that the metallurgical industry must have if it is to develop in a healthy way, and it is very appropriate that such work should be undertaken by an organization financed from State funds.

There are many advantages in NIM's being a State body, including the following.

(a) Stability of research funding. In industry, funds tend to swing with the pendulum beat of copper prices, platinum demand, gold prices, etc.

(b) Avoidance of problems that could arise from the conflicting commercial interests of the industrial companies involved in controlling such a body.

(c) Ability to undertake confidential and strategic work for State departments, e.g., Defence.

(d) Commercial neutrality as a non-profitmaking State organization.

There are disadvantages as well — and major ones — and there may well be a case for the management policy of certain of the national research laboratories to be more in line with the principles of industry than with Civil Service principles, but at all costs they should retain their national character.

Expenditure of Public Money

A nice dilemma
We have here
That calls for all our wit,
For all our wit.
And at first sight
It don't appear
That we can settle it.

I must now turn to the second main accusation against my clients, which claims in effect that, because they are not subject to the same vigorous supervision as they would be in industry, their output and productivity are not commensurate with the expenditure incurred.

This is one of the most difficult accusations to refute. In theory, all I have to do to prove my client's innocence is to quote all the benefits that have arisen from their research work over the years, and to show that the economic value of these benefits exceeds the cost of the research. At first sight, this should be very easy — a simple case of retrospective cost-benefit analysis — but in practice it is not so. There have been many attempts throughout the world to do just this but with very little success, and no-one has yet succeeded in producing a really convincing balance sheet.

Let us analyse in greater detail some of the difficulties encountered.

(a) Most national research organizations in South Africa cannot participate directly in the commercial profits that arise from a successful research programme. NIM, for example, cannot participate as a shareholder in a commercial plant that is established as the result of the successful development of a new process. It can charge licence fees and royalties for new inventions, but those of you who have been associated with patents and licensing agreements will realize that significant profits very seldom arise from such procedures. Charges can be made for sponsored confidential work, but at best these are generally sufficient only to cover the actual costs, and, in any event, this type of work should not be the main function of a national research organization. If it were, then these bodies should be reconstituted so as to operate on a commercial basis. Thus, the actual direct income of a research organization from sponsored work could never be used as a proper reflection of the 'benefits' arising from research.

(b) Although a successful research project may result in a new or improved process, which in turn leads to a major new industrial venture and thus a contribution to the national economy and profits to a company, it is not logical to assign the total benefits nor all the profits to the act of research. If this argument were allowed, virtually all research organizations would be able to show enormous benefits-to-cost ratios. For example, the research work done from 1946 to 1956 at the Government Metallurgical Laboratory provided the key to the establishment of the uranium industry in South Africa. The value of uranium that has been, and will be, produced in South Africa is of the order of 1×10^{10} rands, the cost of research is probably of the order of 1×10^{7} rands, and the benefit-to-cost ratio is therefore of the order of 1 to 10^{3}. On these premises, one successful project in every thousand would represent a break-even proposition. However, is it realistic to calculate benefit in this way? The uranium industry was not established only by the act of research that developed an economic metallurgical process, but by the combined efforts of geologists, plant-design engineers, financiers, marketing agents, and many others, all of whom deserve proper credit, and it is this problem of assigning the appropriate proportion of total benefits that can be related to the research costs that presents the greatest difficulty.

(c) Only a small proportion of research activity produces major new developments such as those that resulted from the uranium research. The bulk of successful research activity is concerned

with marginal improvements to existing processes, which reduce the capital costs of new plants and reduce the operating costs of existing plants. In such instances it is very much easier to make a realistic assessment of the benefit of the research work. Even so, the same problem arises — the work is rarely done in isolation, and the benefits usually arise from the combined efforts of the research personnel, plant production personnel, equipment contractors, etc.

(d) Although a significant proportion of research provides a negative answer, such work cannot be rated as totally valueless. In many instances, the research work shows that the objective is impossible to achieve, and a great deal of money can be saved by avoiding further wasteful expenditure.

(e) A significant proportion of research work is of a fundamental character, which, by definition, has no applied economic objectives. The result is a contribution to knowledge — but how do you assign an economic value to such knowledge?

These are the sort of problems that have been tantalizing research managers and scientists for many years, and there are many people who say that it will never be possible to draw up an acceptable balance sheet representing a quantitative estimate of the productivity or benefits of research activities with which the cost of the research can be compared.

It certainly is extremely difficult to draw up such a balance sheet, but one cannot condone a defeatist attitude. The stakes are far too high for us to accept defeat completely. If we spend too little money on research, we are likely to jeopardize the future economic development of the country. If we spend too much, not only shall we be wasting money, but, even more important, we shall be wasting manpower. No matter how inaccurate or how tentative our attempts to assess the benefits of research may be, it is better to make an attempt than to do nothing at all.

Apart from trying to justify the past activities of my client, there is another and more important reason for attempting some type of cost-benefit analysis for research; that is to establish proper priorities for research on an institutional and national basis.

Most research organizations that are above the critical size soon find that they can generate many more new projects than they can handle. There is hardly a project undertaken that does not, during its execution, breed at least two or more new projects. Unless one is highly selective in deciding which of the many new suggestions should be undertaken, one soon reaches an explosive situation as far as expansion is concerned. Decision-making of this type is probably one of the most difficult tasks of research managements. The judgement and instinct of the research director (or whoever has to make the decision) obviously play an important role, but, clearly, a quantitative index representing the probable economic benefits in relation to costs would be invaluable in helping him to make the right decisions. (In passing, it is worth while referring to the analogy between a research organization and a nuclear reactor. The selection mechanism for new projects is closely analogous to the control rods in a reactor.)

These decision-making problems of individual research organizations are very similar to the problems that are encountered on a national basis, and, of course, it is the selection of future national programmes that must be the deciding factor in the allocation of research funds and the determination of scientific policy. I should like to indicate some of the thoughts and activities of my clients, who are working on this problem, specifically as regards the selection of future projects.

Let us begin by attempting to reduce the complexity of the problem by removing one of the biggest stumbling blocks — one that is considered to be the main factor in making people believe the task is impossible. This stumbling block is basic research and, because of the vast amount of confusion about the meaning of this term, we should define it. The best definition, which is now internationally accepted, is given in the *Frascati Manual*[4], and reads as follows:

> *Basic research is original investigation undertaken in order to gain new scientific knowledge and understanding. It is not primarily directed towards any specific practical aim or application.*

In terms of this definition, the prediction of the benefits of basic research in economic terms is impossible. (This does not, of course, mean that basic work does not lead to economic benefits; it means only that we cannot predict these benefits in advance.) True basic research is an act of curiosity, an act of culture, an exercise in creativity, and a demonstration of intellect. It is closely comparable to many art forms such as ballet, music, sculpture, and painting. We may rest assured that, as long as mankind retains the instinct of curiosity, basic research will never disappear, and there is no doubt that it should continue in universities and certain research centres. However, like art and entertainment, it must always be regarded as an exercise that must form a part of our lives in proper proportion, but attempts should not be made to justify it on the grounds of the economic benefits that it will bring, since it quite correctly defies analysis in hard financial terms.

If we can put aside the problem of basic research, at least for the time being, then we have considerably simplified the overall problem, bypassing all the arguments one so frequently hears that it would never have been possible to predict the practical benefits that have arisen from Einstein's theory of relativity, Rutherford's work on the atom, or Mendelejeev's work on the periodic table. We can now limit ourselves to applied research work, which, by definition, is work aimed at solving problems of practical value, be they long-term or short-term problems, and which should therefore be capable of being expressed in terms of benefits. Thus, if one could estimate the costs of research, the benefits that might arise, and the probability of success,

one could easily combine these factors into an index that can be used in establishing priorities.

It is not easy to estimate the cost of research in advance, and scientists who are asked to do this often claim that research is the exploration of the unknown, and it is therefore difficult to estimate how long it will take to reach the object. 'If I knew how much it would cost and how long it would take, I wouldn't need to do any research' is a frequent reply to requests for cost estimates. However, this problem and many others in cost-benefit estimating can be resolved if we are prepared to accept some measure of uncertainty and inaccuracy.

The situation reminds me very much of one of the past dilemmas of physics. For many years nuclear physicists were bogged down in their attempts to describe, in an exact mechanistic way, the precise nature of light and the precise motion of electrons in atoms. They were attempting simultaneously, for example, to calculate the exact position and the velocity of an electron. This problem was not resolved until one of the great fundamental physicists, Heisenberg, in enunciating his 'uncertainty principle' showed that what they were trying to do was (a) impossible, and (b) unnecessary. Progress could be made in understanding the behaviour of matter if one forgot about individual electrons as such, but looked at their motion on a broad statistical basis and considered only the overall effects of their interaction with other electrons, giving due recognition to the evaluation of probability factors. In the same way, the precise definition of the duration and cost of a future research programme is incompatible with the fact that research, by its very nature, is uncertain. Therefore, if we are prepared to ignore ultimate accuracy and fine detail, and attempt to predict only the major steps that are likely to be necessary in a research programme, then it is possible to estimate the cost of reaching a given objective. Even though this estimate is subject to inaccuracies and to statistical variations, it is likely to be adequate for most cost-benefit calculations.

It was this philosophy that led NIM to develop a system of programme charts that has proved to be immensely useful in the estimation of future research costs and objectives. An example of these programmes has been presented to you as evidence in the Court records (Exhibit 5). Although the various scientists have their own individual approach to programming, the three essential features that must always be incorporated in the charts are as follows:

(1) a clearly defined objective, which must include two essential requirements: a built-in definition of the point at which the project will be considered to have been completed, and an indication of the expected value of the project, i.e., the benefits that one hopes will accrue from it;
(2) an indication of the various parallel or sequential activities that will probably be required to achieve the objectives, and the various stages at which certain decisions will have to be made regarding the future conduct of the investigation;
(3) an estimate of the time and manpower required for completion of the various steps, from which the cost of the investigation can readily be calculated.

This system is not intended to be highly accurate, but it is remarkable how many of the projects do, in fact, finish close to the pre-planned schedule. At NIM there is very little sympathy and virtually no money for those scientists who believe that it is impossible to predict the costs of research investigations. These observations should also be taken as evidence to refute the accusation made by the prosecution that research scientists drift along in an undefined way. Most research organizations in the world are adopting one form or another of research-programme planning. The scientists themselves find it beneficial, and certainly regard it as a matter of pride to adhere to the proposed schedules.

Let us now consider the next problem in cost-benefit analysis — that of estimating the benefits of any applied-research project. The whole secret to the solution of this problem lies in the necessity for a clear definition of the objectives of a research project. If we can define the objectives so as to indicate the expected benefits, we are halfway towards estimating their economic value. It is not always possible to do this precisely, but, if we once again apply Heisenberg's uncertainty principle and group similar projects to form what we have chosen to call research programmes, then it is possible to define the objectives in such a way as to enable one to predict the economic benefits. The best way to illustrate this concept is to quote examples, and to do this let us consider the work that NIM is doing in helping to develop the mineral resources of the north-western Cape.

The Economics and Costing Division of NIM can, without too much difficulty, provide approximate figures to indicate the mineral potential of this particular area. It is, of course, easy to justify the work being done in this area when it is realized that it has a potential of approximately 1000 million rands per annum, but it is not logical to assume that all the benefit can be attributed to NIM's research activities. Whether or not NIM existed, there is little doubt that the mineral potential of the area would be exploited, if only by the standard South African technique of purchasing technology from overseas. If, however, one describes the objectives of the new research programmes in a specific way, it can be seen immediately that the benefits arising from the research, if successful, can be calculated. For example, the objective of one particular project could be described as 'to develop a differential flotation procedure for Splitkoppies ore to improve recoveries from a zinc sulphide concentrate free of significant copper content from the present 75 per cent to at least 80 per cent'. However, in many instances, this type of definition is not possible. For example, as a prerequisite for any work of the nature already described, it is essential to conduct a detailed mineralogical examination of the ore samples from a specific area, and such an examination is a

PROJECT TARGET DATES MONTH/YEAR	WEEK 1	2	3	4	PROJECT SEQUENCE	PROJECT DETAILS AND DECISIONS REQUIRED AT MEETINGS	Interaction	STAFF REQUIRED	TIME %
1975									
MAY		X			P	Discussion of data obtained to date for the recovery of both zinc and elemental sulphur from a sulphide conc.			
MAY			X		T				
MAY				X	Further laboratory investigations for optimum zinc and elemental sulphur recovery. a) Low level ferric sulphate leach in oxidizing atmosphere, introducing MnO_2 and utilising spent electrolyte	a)1. Influence of Manganese on zinc recovery (Fe^{+3} content of leachant 10 g/l)	M&PC	M.J. Lombaard	60
					Coordination and flowsheeting	2. Oxygen and air as oxidising media		D. Breedt	80
						3. Temp on zinc dissolution (low as possible)		V. Veronese	20
						4. Follow S^0 SO_4 conversion throughout leach cycle	Anal.	R. Lombaard	50
						5. Iron removal from pregnant solution - Hydrolysis as alternative to Goethite or Jarosite			
JULY			X		b) Exothermic sulphation reaction	b)1. Possibility of utilizing spent electrolyte as leachant - different electrolyte/conc. H_2SO_4 ratios	M&PC	V. Veronese	80
JULY	X				c) Alternate means of purification) Treatment of byproducts	2. Efficient dissolution of plastic sulphur			
JULY			X		T				
						c)1. Co-ordination and planning of project.		Dr.Martin	70
						2. Material balance and engineering calulations for flowsheet proposals			
						3. Costing excercises on flowsheets	Econ.		
						4. Purification methods including conventional and new IX or SX methods	Anal.		
						5. Analysis of flowsheets for byproduct production			
MAY				X	d) Residue treatment for elemental sulphur recovery	d)1. Recovery of S in $(NH_4)_2S$ solution	Anal.	R. Lombaard	50
JULY	X					2. $(NH_4)_2S$ regeneration losses			
JULY			X		T				
JULY			X		H				
JULY				X	P	Discussion of possible process pilot plant.			

Continues on page

P - Programme meeting (including D - meetings), D - Special meeting with Director General, S - Meeting with Sponsor, H - Meeting with Head of Division
R - Report Writing, T - Technical Memorandum, C - Confidential Communication

Exhibit 5—NIM project chart

fairly major research project in its own right.

The information from such a research project is essential before any ore-dressing or chemical tests can be undertaken. How, then, can one assign a value to a mineralogical report, which is likely to be the only outcome of this mineralogical research project? The answer is that you do not attempt to estimate the benefits of such research work by itself. You are, in fact, once again, attempting to achieve too fine a resolution in the cost analysis, and the problem can be solved by combining such projects with ore-dressing and chemical-processing projects to form a major *programme*, the benefits of which can be assessed in total. This approach leads to a definition of what can be referred to as a *research programme*, namely, that it is a group of applied-research projects, all of which will jointly contribute to an economic objective that can be quantitatively evaluated.

Of course, the benefits do not accrue direct to NIM and most of the other national research organizations, since they are non-profit-making organizations. The benefits will accrue to industry itself, or as a contribution to the gross national product. If one is doing a cost-benefit analysis for the purposes of priority rating for a group of national projects, it is completely logical, provided one is consistent, to include the benefits that might accrue to the country as a whole. If, on the other hand, one is attempting to convince a company that a project is worth sponsoring, one would obviously express the benefit in terms of extra profits to the company.

Perhaps, if one wishes to be even more exact, and if one is attempting to persuade Treasury, through the Scientific Advisory Council, to provide the funds, then the best method would be to calculate the benefits expected to accrue to the Receiver of Revenue. However, once again this degree of accuracy is probably unwarranted, and in most instances an estimate of the contribution to the gross national product is adequate.

Finally, one is left with the problem of estimating the probability of success, and here again we face a difficulty. It is important to do this, otherwise cost-benefit analysis becomes meaningless. One could propose many projects having incredibly high benefit-to-cost ratios but with negligibly small chances of success. The classic example, which has often been used to illustrate this point, is a programme to recover gold or uranium from sea-water. Without too much difficulty, one could postulate a number of processes that might well be feasible — ion flotation, solvent extraction, ion exchange, etc. — and to study these processes might require a research effort of a few man-years — say, $R10^5$ worth. If successful, the probable benefits could, in the long term, be assessed as hundreds of billions of rands — $R10^{11}$. However, as far as I know, no-one is working seriously on this problem, mainly because the probability of success is extremely small and completely offsets the high benefit-to-cost ratio.

The probability of success is a nice scientific mathematical concept and, at one time, research-management publications were full of proposals on how to deal with this problem in a very rigorous mathematical way. For example, one could subdivide a research project into a number of sequential and parallel steps, to each of which one could assign a probability parameter. These individual probability parameters could then be combined to provide a reasonably accurate probability estimate for the project as a whole. However, it is very doubtful whether this rigorous mathematical approach is really justified in terms of the uncertainty principle I enumerated earlier.

To decide on the priorities of future research programmes, an order-of-magnitude approximation is probably quite adequate, so that one can consider projects in three or four main categories only; for example,

(i) success probability, $P \approx 1$
i.e., the chances of success are better than 50 per cent

(ii) success probability, $P \approx 0{,}1$
i.e., the chances of success are about 10 per cent

(iii) success probability $P \approx 0{,}01$
i.e., the chances of success are about 1 per cent

(iv) success probability, $P < 0{,}001$
i.e., the chances of success are less than one in a thousand or negligible.

If one has gone to the trouble of preparing a research-programme chart such as that mentioned earlier, there is very little difficulty, on the basis of experience and judgement, in assigning the research proposal to one of these four categories. In practice, one would probably reject projects in category (iii) out of hand, unless the benefit-to-cost ratio was exceptionally high, and projects in category (iv) could be withdrawn from serious consideration.

At NIM, where more than a hundred new projects are undertaken every year, one does not reach for the latest statistical yearbook and then run to the computer terminal every time a decision has to be made. With experience and judgement the majority of the decisions can be made on this simple four-category basis.

If one requires more accurate figures (and there may be some merit in attempting to acquire them), one could probably best proceed by quantifying past experience on a statistical basis. If one were to analyse the results of completed projects in different areas of research, one could probably obtain useful and meaningful figures. For example, one may be able to show that an average ore-dressing project had a success level of 30 per cent, but that chemical-processing projects had a success level of only 20 per cent. Perhaps NIM as a whole might have a success level of 25 per cent, and, on average, a benefit-to-cost ratio of say 10:1, which might suggest that NIM was indeed 'paying its way'.

At this stage, you will be completely justified in observing that a great deal of what I have described relates to what might be done rather than to what has been done. However, I must emphasize again that one is not dealing with a simple problem, and it is only very recently that much attention has been given to cost-benefit analysis and to ways in which expenditure on research can be rationalized and justified.

One must be careful not to impose on research workers a top-heavy

administrative load of form filling, because this will only reduce productivity. Nevertheless, the programming system at NIM is now well established and accepted, and, after many false starts, a simple costing and evaluation system has been introduced, which one hopes will provide the necessary data to determine what might be termed 'research productivity'. If this proves to be possible, then one has the basis for answering many other difficult questions such as 'How big should NIM be?', 'How much State expenditure should be devoted to research on mineral processing?' and 'What is the optimum proportion of sponsored research that should be undertaken by a national research organization?'

However, such questions can be answered only on a subjective basis at the moment, and there are many shades of opinion on this topic.

I shall now take the whole question of research financing a stage further than the mere consideration of NIM's problems, and analyse the whole national research policy. The transition from a single organization to a national situation is so logical — even obvious — that, although once again I am exceeding my brief (which is limited to my client NIM), I nevertheless feel that the extension of these basic principles will be of interest and value.

In discussing NIM's problems, I hope I have convinced you that there is some merit in assessing benefits, costs, and the probability of success, if only to establish priorities for research programmes. In the whole national research effort, it is, of course, even more necessary to establish priorities — not of individual programmes, but rather of a whole area of scientific activity. Whereas NIM might have to consider the relative national priorities of programmes dealing with, say, copper in the north-western Cape or magnesite in the north-eastern Transvaal, the relative priorities of research activities, say, in the medical, agricultural, and mineral fields have to be decided. Let us define these different fields as scientific activities. If we confine ourselves to applied research, and if we can estimate a benefit for each of the major scientific activities in South Africa, plus the cost of research, plus some parameters indicating the probability of success, then surely we can establish priorities, decide on budget allocations and, in fact, do 90 per cent of what should be done in formulating scientific policy.

Coincidentally, but very fortunately, in South Africa the definition of scientific activities falls into a pattern that is almost identical to that of economic activities; so much so that, if one considers the internationally accepted standard classification of economic activities as shown in Exhibit 6, one can adapt this to accommodate scientific activities with only minor modifications.

To assess the future costs of national research is no problem whatever, since the various organizations listed all submit budgets for three years in advance to Treasury every year. To estimate the potential benefits that may arise in each of the scientific activities is a complex task, but certainly not an impossible one if we accept that a measure of approximation is inevitable, and that national benefits from research can be expressed in economic terms. On the latter point, there is much difference of opinion about whether benefits such as would arise in medical research, for example, can be expressed in economic terms. For example, could a cure for cancer, if discovered, be given a quantitative economic value, or could research for strategic and defence purposes be associated with an economic figure? I believe the benefits can be assessed in economic terms without too much difficulty — in fact, this is being done almost automatically every year. Life-insurance companies make a very good living indeed by quantifying life and death in economic terms, and I don't believe that today we would have much difficulty in assigning costs to military equipment.

Finally, as regards national priorities, one also requires some estimate of the probability of success for the various scientific activities. If all the organizations undertaking research work that involves the use of State funds were to adopt some system of cost-benefit analysis, there would be little problem, since average figures based on past records could be used. In the absence of such information, it may be necessary to adopt some form of opinion-survey method. Clearly, in certain areas of scientific activity, this country is in a particularly favourable position to make a success of research. In others it is probably at a disadvantage. In view of the limited number of categories of scientific activity that have to be considered, there should be no great problem in establishing a suitable index to represent this factor.

Therefore, it is believed that cost-benefit analysis could be used, and would be very valuable in establishing a pattern of scientific expenditure most appropriate to the country's requirements. The accuracy of the various estimates would have to be somewhat better than the order of magnitude required for individual projects or programmes, but, on the other hand, on a national basis, the large population of individual estimates would mean that the overall averages used in the calculation would have a much higher level of accuracy.

One can only hope that our Scientific Advisory Council will provide a lead to the research scientists in South Africa in this regard, because this quantitative and scientific approach towards the distribution of national research expenditure would be a considerable advance on the existing *ad hoc* methods.

Before leaving a most interesting subject, but one that is much too complex to discuss in further detail here, I must come back to the problem of basic research, for which it seems impossible to derive an appropriate expenditure from cost-benefit calculations. As indicated before, basic research is the acquisition of knowledge for knowledge' sake. It could well be termed 'curiosity research', and, as such, one cannot believe that it should deserve a special priority rating above applied research, which pulls in the money that allows one to indulge in such luxuries as basic research. Certainly, one would have to allocate to this work a limited proportion of the

R and D classification			SIC classification of economic activities	
Activity	Main agency for research	Subsidiary agencies	SIC index	Economic activities
1a Agriculture	Dept Agricult. Tech. Services	AEB CSIR	1.1	Agriculture, Forestry and Fishing
1b Forestry	Dept Forestry	AEB	1.2	
1c Fishing	CSIR (Oceanography)	Dept Industries	1.3	
2 Mining	Dept Mines	G.S.O. (Exploration)	2	Mining and Quarrying
2a Mineral Processing		AEB and NIM	3.6 3.7	Overlap with 3 in Base Minerals, Iron, and Steel
3 Manufacturing and Secondary Industry	CSIR Dept Planning	Dept of Industries	3	Manufacturing
4 Energy	AEB F.R.I.	Escom	4	Electricity, Gas, and
5 Water and Dam Construction	Dept Water Affairs	CSIR G.S.O.	4.2	Water
	W.R.C.	Dept Agric. T.S.	5.2	Overlap with 5
5b Construction Buildings	CSIR	P.W.D.	5 Excl. 5.2	Construction
6 Business and Commerce	H.S.R.C.	Dept Commerce	6 8	Wholesale and Retail Trade, etc. Financing, Insurance, etc.
7 Transport and Communication	Dept Transport CSIR		7	Transport and Communication
8 Medical, Dental, and Health	M.R.C.	Dept Health	9.331	Medical, Dental, and other Health Services and Community Services (including Education)
9 Education and Community Services	H.S.R.C.	Dept National Education	9	

Abbreviations
SIC — International Standard Industrial Classification for Economic Activities
NIM — National Institute for Metallurgy
AEB — S.A. Atomic Energy Board
CSIR — Council for Scientific and Industrial Research
G.S.O. — Geological Survey Organisation
F.R.I. — Fuel Research Institute
W.R.C. — Water Research Commission
H.S.R.C. — Human Sciences Research Council
M.R.C. — Medical Research Council
P.W.D. — Public Works Department

Exhibit 6—Proposed relationship between R & D in State sector in South Africa and the SIC

funds available, but, at the same time, one feels that we in South Africa cannot go to the extremes of the major western countries in putting up enormous accelerators for work on basic nuclear physics or giant radio telescopes for modern-day astronomy.

I believe that we must adopt an approach similar to that of a family attempting to decide what amount should be spent on entertainment. They would almost certainly look first at what they could afford for luxuries after meeting all the essential expenses for food, clothing, and accommodation. Thus, I believe the country should never give priority to basic research while there is urgent applied research waiting to be done. At the same time, the family would also no doubt decide that 'all work and no play makes Jack a dull boy' and that they would be out of the social life of the neighbourhood if they could not exchange notes on the rugby test or the latest rock-and-roll record. Similarly, a country as a whole cannot afford to do no fundamental work, and it also has a need to maintain contact with other countries in the basic sciences.

However, unless our hypothetical family were of the millionaire playboy type, they certainly wouldn't attempt to play the whole field of cultural and entertainment activities, and, correspondingly, South Africa should not attempt to climb onto every basic research bandwagon that is riding round the

world, but should carefully select those topics in which its local situation can give a special advantage. Obviously, basic geochemistry and certain areas of human sciences research are a natural for this country. Ultimate particle physics and astro-physics are topics that could well be left alone. Using these general guidelines, we would probably arrive at the same conclusion as most other countries, that a level of expenditure for basic research of about 10 per cent of total research expenditure is about right, and it would seem that, until one can specify reasons for in-increasing or decreasing this figure, this approach is the best solution to a difficult problem.

Summing Up

I hope, Gentlemen of the Jury, that I have convinced you of the innocence of my clients, even though I have not yet been able to prove it in hard financial terms. I believe you can return only a verdict of not guilty, but I should like to refer to the sentence that the prosecution has suggested should be imposed, namely, that the accused be removed from their ivory towers of research and sent into industry to undertake a life sentence of hard labour in productive activity. My clients have asked me to advise you, M'Lud, that they would be delighted to accept such a sentence even if they were found *not guilty*. Their only request is that they should be used productively. We all agree that no-one in this country can afford to encourage parasitic activities, nor to use anyone in a job that demands less than his full capabilities.

Instead of bringing innocent persons to court, could we not rather focus our attention on some of the other parasitic manpower-consuming activities in this country? Do we honestly need the multiplicity of new shopping centres that are springing up like mushrooms all over the country? What do they do but produce more furniture, hardware, and pharmaceutical stores per head of population, with the inevitable result of smaller turnovers in relation to overheads, higher rentals, and even greater mark-ups to maintain the fabulous profit levels that pertain to the marketing of consumer goods in South Africa? Do we need all the petrol filling stations that we have in this country or, for that matter, all the motor-car and property salesmen? My learned friend has referred to the paper explosion. Has he forgotten the incredible quantities of paper put out in the form of advertising blurb to sell a bewildering variety of different makes of virtually the same product? It is this proliferation of such parasitic activities that is causing inflation. Every tinker, tailor, and salesman today wants a house in Houghton and a Jaguar, and is getting them. The man with seven or eight years training to attain a Ph.D in engineering and science is falling way behind in the salary rat-race, so that one could hardly say that my clients have benefited very much from the fraud of which they are accused.

Can industry today honestly claim that it has used graduates to the maximum of their capabilities? In the years immediately ahead, we are going to have to do some careful thinking about who is to do what. Clearly, in this country, we have enough hands available to meet our requirements for labour in the skilled and semi-skilled activities, and, if we wake up and establish the necessary training organizations, we shall be able to provide the technical labour required for the expansion that will have to take place if we are to avoid revolution.

I am quite sure that, with proper training courses, most of the foremen, superintendents, and even perhaps certain of the managerial jobs, can be filled adequately, but not even in my wildest dreams can I foresee that we are going to meet the demand for professional engineers and scientists. During the next decade, a capital expenditure of about 10 billion rands is contemplated in the mining, metallurgical, and other technological fields (Exhibit 7), and the very minimum manpower requirement that I can equate with this expenditure is an average of one professional graduate engineer per million rands of expenditure. This means that we shall need 10 000 such engineers in the next decade.

A colleague who estimated these figures for me thought the following limerick would be most appropriate:

	Expenditure R Million	
(1) *Mining, Mineral Processing, and Metallurgical*		
(a) Gold-uranium mining	1 250	
C.O.M. research programme	150	
(b) Iron and steel — Iscor	2 500	
Highveld Steel and Vanadium	66	
(c) Ferro-alloys and electrometallurgy	252	
(d) Coal and collieries	650	
(e) Phosphate and fertilizers	160	
(f) Base metals, N.W. Cape, S.W.A., etc.	500	
(g) Richards Bay titanium plant	300	
(h) Base minerals, fluorspar, etc.	50	
(i) Cement	450	
(j) Uranium Enrichment Corp.	1 000	
SUB-TOTAL	7 328	
(2) *Chemical Industry*		
(a) Sasol II	1 021	
(b) AECI	736	
(c) Shell	500	
(d) Sentrachem	200	
(e) Solvay plant	50	
SUB-TOTAL	2 507	
TOTAL (1) *and* (2)		9 835
(3) *Related Infrastructure*		
(a) ESCOM	3 254	
(b) SAR	1 600	
(c) Saldanha harbour	500	
SUB-TOTAL	5 354	
GRAND TOTAL		15 189

Exhibit 7—Some planned capital and research expenditure in South Africa, 1974-1984

There was a maths student from Trinity
Who worked out the cube of infinity.
The sight of those digits
Gave him such fidgets
That he gave up his Maths for Divinity.

It does seem that we shall need Divine intervention if we are to avoid a situation in which, because of the lack of professional engineering and scientific manpower, we plunge into major capital projects without proper research and planning. Therefore, there must be the closest collaboration between State and industry in sharing and making the most productive use of our research facilities. My clients are well aware that research is done not only in laboratories — a very large proportion of their activities is on plants and mines — and they will be delighted if such operations can be extended even further.

It would be fatal in this crazy world for us in South Africa to develop any measure of polarization between State and industry, least of all in the scientific and technical field. In fact, because we are small and our resources are limited, we have perhaps the opportunity to show the world what can be achieved by collaboration between State and industry in this area. In a more general way, the capitalistic societies of the western world are now entering a crisis period in which the very survival of the western democratic capitalistic system is at stake. To a very large measure this is being made evident by the increasing polarization between industry and governments — governments that steadily but inevitably are becoming more and more socialistic, and more and more determined to nationalize industrial effort. The gulf between the two poles of capitalistic industries and socialistic governments seem to be widening into a chasm. Yet in South Africa, and certainly in the mining and metallurgical industries, I have the impression that State and industry are closer together than ever before in our history. That there is everything to be gained by collaboration is well demonstrated by the incredible economic advance of Japan, where industry-State collaboration is at a very high, albeit subtle, level. Collaboration does not mean that there are no grounds for criticism on either side, but rather that the points of difference are discussed openly and frankly, and are resolved in the face of urgent national requirements and programmes.

Let us avoid accusations of this severity in future — but concede that, if these cases for the prosecution and for the defence have brought points of suspicion and criticism out into the open for debate and resolution, then the time of the court has been well spent.

M'Lud, Gentlemen of the Jury, I close my case, and ask you to retire to return a verdict of *NOT GUILTY*.

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In addition, data were taken from the Annual Reports of the CSIR, AEB, and NIM.

Competition for student members

Each year the South African Institute of Mining and Metallurgy offers a prize (or prizes should the entries warrant it) of up to R100 for the best paper or dissertation on a topic appropriate to the interests of the Institute. The competition is open to all Student Members of the Institute.

A Student Member who is in full-time study at a university may submit the dissertation or thesis he has to write in part fulfilment of his university degree, provided that it is presented in a manner and on a topic suitable for publication in the journal.

Entries for 1975 should reach the Institute by 31st December, 1975.