



80 YEARS
OF RESEARCH
AT THE PHILIPS
NATUURKUNDIG
LABORATORIUM

1914 - 1994

MARC J. DE VRIES
WITH CONTRIBUTIONS BY F. KEES BOERSMA

PALLAS PUBLICATIONS

80 Years of Research at the
Philips Natuurkundig Laboratorium
(1914-1994)

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The Role of the Nat.Lab. at Philips

MARC J. DE VRIES

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Preface

It did not take me long to decide whether or not I would accept the invitation to work on the historiography of the Philips Natuurkundig Laboratorium ('Nat.Lab.'). The idea of writing a book about such an intriguing laboratory, of which there are only a few in the world, instantly appealed to me. At the same time, I realised that it would not be a simple task. All that research was eventually to lead to this book, which was written during the period January 1997 to May 2000. I received useful advice from many people, whose contributions I want to acknowledge in this Preface.

For the purposes of this research, a room was made available for me on the Nat.Lab. premises where I could build up a small archive, so that for some time I more or less became part of the laboratory that I was studying. That gave me the opportunity to experience how in some respects the past is still present in the here and now. Many a time I noticed people walking around the pond in front of 'my' WB building, during their lunch hour; later I found in the minutes of a meeting that had been held in 1969 a remark made by one of the managing directors of the lab, who pointed out that walking there could only be allowed as long as it did not extend into working time. The announcement made during my period of stay, that the current premises were to be extended to create a prestigious 'science park', immediately reminded me of the 1963 scale model that had served as a representation of the expectations for a similar type of extension that did not materialise. This time the extensions have actually started to be built. This example serves to illustrate the fact that today the Nat.Lab. in its capacity as part of an international, separate research organisation is (still) seen as a very valuable element of the Philips company.

Of all the people who deserve to be mentioned in this Preface, Dr. Feye Meijer certainly takes the first place. As a former managing director of the Philips Research department, the organisation under which the Nat.Lab. falls, he was able to open many doors for me which otherwise would have remained closed. But more importantly, he was always able to stimulate me with his personal interest and by sharing with me the knowledge that

he has gained after so many years of having worked in the lab at various levels in the hierarchy. At the same time, he had great respect for the intellectual freedom that I needed to obtain conclusions that were not biased by his or the company's preconceptions. From the very beginning, he was my main contact person in this research study, and he has remained that after his retirement in 1997.

In the second place I want to thank Professor Harry Lintsen, who supervised the project on behalf of the Stichting Historie der Techniek (the Foundation for the History of Technology), which had been commissioned to do this study. His knowledge about the 'craft' of historical research and his insight into the history of technology in the Netherlands have been extremely valuable to me.

Dr. Meijer and Prof. Lintsen were members of a larger committee that served as a discussion platform set up to advise me in my research. Other members of this committee were Professor Emile Aarts (Philips Research) and Dr. Marianne Vincken (Philips Research, Public Relations). The Philips Company Archives were also represented in this committee. Each person drawing on her/his background was able to provide useful input at the discussions that we had during the three years that the group met on a regular basis. Many of the practical arrangements for this group's meetings, and my temporary instalment at the Nat.Lab. premises, were made by Loes van Santvoort, who was Dr. Meijer's secretary until his retirement. Even after Dr. Meijer's retirement, she was always prepared to assist in all sorts of practical issues.

All illustrations were used with kind permission of Philips Research.

As a Ph.D. student Kees Boersma, M.Sc., played a major role in the study of the first period of the Nat.Lab.'s history (1914-1946). The text about that period is largely based on the material that he collected. His dissertation on the Holst period is entirely his own original work. I had many interesting discussions with him about this period. After he received his doctor's degree, we continued our co-operation, and Kees contributed to the revision of the text about the Holst period.

I want to thank Peter van den Berg and Margot van Gastel for the work they did on the case studies in the third main period of the Nat.Lab. history. Heleen Neggens and Kenian Domen, thank you for your work on the database of Nat.Lab. scientists. I want to thank Angèle Pieters and Wouter Schilpzand for all the work they did on the illustrations.

Kees Boersma and myself participated in a discussion group chaired by Dr. Ernst Homburg and composed of colleagues whose common interest was the history of industrial research in the Netherlands. This group read drafts of parts of the book and provided me with many useful comments.

When it came to studying the written and printed information sources at the Philips Company Archives, Mr. Ben van Gansewinkel worked hard to provide historical documents. Most enthusiastically, he searched for any

information that could be of importance to me, particularly where the case studies of this book were concerned.

I want to thank Mrs. Diane E. Butterman-Dorey for the excellent correction of the English text of the original report version of this book.

I also want to thank all the people who were prepared to be interviewed and thus served as indispensable sources of information about various aspects of the laboratory's history. They provided me with stories that have added an extra dimension to the written information resources I already had. They also made me come to respect the Nat.Lab. as a place where excellent people have been and still are working.

I want to thank Anniek Meijnders (Amsterdam University Press) for her interest in publishing the book and Chantal Nicolaes for the work she did on it to prepare the publication of the text.

Finally, I want to thank the Stichting Historie der Techniek and Philips Research for their support of the realization of the book.

Of course none of these people can be held responsible for possible errors in this book. As its author, I myself have had to make the choices that have made it what it is. I hope the book will, apart from anything else, give the reader an impression of the exciting time I had while doing this research.

Eindhoven, May 2005
Marc J. de Vries

1. The Role of a Research Organisation in an Industrial Company: Motives and Themes for Writing the History of the Philips Nat.Lab.

On October 23, 1913, a 29-year-old physicist, named Gilles Holst, who worked at the physics laboratory at Leyden University, wrote a letter to the directorate of the N.V. Philips Gloeilampenfabriek in Eindhoven, to apply for the post of 'Doctor of physics, in particular a good experimenter'. The Philips company had advertised for this in the *Nieuwe Rotterdamse Courant* (a Dutch newspaper) of October 23, 1913. Holst was invited for an interview with Gerard Philips the following Thursday and offered the position. On January 2, 1914, he started working at Philips, and his arrival marked the formal beginning of the Philips Natuurkundig Laboratorium (literal translation: Philips Physics Laboratory). This laboratory was to become one of the leading industrial research labs in the world, in size and scope, along with for instance the Bell Labs, the General Electric Labs and the Siemens labs. This book describes the history of this laboratory, the 'Philips Nat.Lab.' as its name is usually abbreviated.

The Relevance of a Historiography of the Philips Nat.Lab.

The development of technology in the twentieth century cannot be properly understood without having some insight into the specific role that the industrial research laboratories played. Since the end of the nineteenth century, a number of technological developments had been continuing that were more dependent on scientific knowledge than on the technological developments that had taken place before that time. Industrial research laboratories were one of the key factors in that process known as the 'scientification' of technology. These laboratories were organisations of professionals (scientists) who all had to look for a proper way of functioning if they were to serve their companies. The history of such labs can provide insight into such processes. Thus, the history of the Du Pont Labs, the Bell Labs and the GE Labs has already contributed to our understanding of this process in the USA. The emergence of these labs as separate entities within an industrial company can be seen in the broader context of the creation and expansion of the organisational capabilities of industrial companies in the USA in the late nineteenth and early twentieth centuries.¹ Although the Philips Nat.Lab. is comparable in size and scope with the labs mentioned above, there is, as yet, no written history of

it that can show how it fits into this overall picture of the emergence of industrial labs. Evidently, such documentation is lacking in the existing literature, and therefore there is a place for a historiography of the Philips Nat.Lab. (as a matter of fact, the same holds true for the historiography of the Siemens laboratories, which does not yet exist either).

Central Theme and Sub-themes

When it comes to establishing a central theme for the history of the Philips Nat.Lab., there are many possible avenues to follow. The impact of this laboratory on the development of science in the Netherlands, and worldwide, could be the focus. It would also be possible to make a study of the role that the Philips Nat.Lab. has fulfilled in social developments in the Netherlands. But the motive that has been mentioned above suggests a different approach. Viewed from this point of view, our primary interest must be the role of the research laboratory as a professional organisation for the company as a whole. This choice of a central theme results in certain limitations for our history of the Philips Nat.Lab. It means that more attention will automatically be paid to the internal position of the laboratory in the context of the Philips company than to the role of the laboratory in the development of science and society. Our main interest will be to investigate the role the Nat.Lab. envisioned fulfilling in the Philips company during the course of its existence. In other words: what sort of professional organisation did the Nat.Lab. want to be in the context of the Philips company? In a number of places, this role will be compared with the views of the Product Divisions on the functioning of the lab within the company. A more extensive investigation into the development of the company as a whole, and the position of the lab within it, is being carried out in the historiography of the whole company that people have been documenting for about two decades now. Four volumes on the history of the Philips company have already been published. For the period up to and including the Second World War, the history of the company has thus been described, but a study for the later years is not available yet.

To summarise this, our main theme will be: what role did the Nat.Lab., as a professional organisation, want to fulfill for the Philips company, and which characteristics of this professional organisation were developed so that the laboratory was able to fulfill this role?

Having identified the Nat.Lab.'s role as a professional organisation for the Philips company as the central theme for the historiography of the Nat.Lab., the next question we have to consider is that of the operationalisation of this theme. In other words, what are the subthemes that can be derived from this central theme if we are to give a comprehensive

description of the Nat.Lab.'s history in the light of the central theme? To answer this question, we have to consider the characteristics of a research organisation as a professional institution. What is meant by 'professional institution' in this case is an institution that is primarily populated by 'professionals': scientists and research assistants. Within such an organisation one finds at least the following five characteristics:²

1. a striving toward goals and objectives; nowadays, these are often enumerated in a 'mission statement'. The Nat.Lab. has such a mission statement. In 1999, when this book was written, the mission statement laid down was the following: "Philips Research generates options for successful industrial innovation; takes care of timely transfer of technical results to the Product Divisions and initiates new businesses within the scope of the company; helps to establish a strong patent position. A Research success becomes a real success if it leads to a business success. Philips Research as a corporate organisation develops a portfolio of key technologies for future products; adds value through synergy; is a source of highly skilled scientists and engineers for Philips; is the objective technical conscience of the company. Philips Research, being the strategic partner of the Product Divisions, strives for effective interfaces with these Divisions, with the scientific world and with other industrial research laboratories." This mission statement reflects a variety of goals and objectives. Although the mission statement is only a recent phenomenon, several of the goals that are mentioned in it played a part in the history of the Nat.Lab. While studying the history of the Nat.Lab. we will see how the emphasis has shifted between such goals and objectives;
2. means to achieve the ends. These means are mainly personnel and material, such as budgets and equipment. In particular, it is the people who work in the organisation who to a large extent determine its functioning. Therefore, in our study of the Nat.Lab.'s history, explicit attention will be paid to the people who participated in the laboratory work and to their backgrounds;
3. structures to enable effective and efficient functioning. Here we can think of the formal procedures relating to the work in the laboratory and to the division of tasks and responsibilities. In an industrial research organisation, these procedures and divisions have to do with a variety of processes that take place in the laboratory. Szakonyi³ described these processes as follows: selecting research topics, planning and managing research projects, generating new product ideas, maintaining the quality of processes and methods, motivating people, establishing cross-disciplinary teams, co-ordinating research and marketing, transferring research output to product divisions, fostering collaboration between research and finance, and linking research to business

- planning. What were the structures that were set up over the course of time to realise such processes in the Nat.Lab.?
4. a culture with certain norms and values. Such a culture becomes visible through e.g. the style of management adopted and the working atmosphere in the laboratory in question. This aspect is probably the most difficult one to describe since for the most part it is not explicitly described in the written material available as sources for study. It requires a certain amount of 'reading between the lines', and this is always risky. We must try, however, to do justice to this aspect of the Nat.Lab's history, too;
 5. the influence that the Nat.Lab. had on the company in practice. What mechanisms and procedures were used to exert influence on the company's product strategy?

Because the Nat.Lab. is a professional institution, this leads to a number of tensions that are specific to such a type of organisation. Managing professionals is different from managing people who are only involved in routine work. According to Weggeman,⁴ in the latter case the application of formal management rules and procedures is much simpler than in the former case. There is a tension between the freedom that scientists claim for their work and the need to control all the research activities that go on in the laboratory. A second tension was expressed by Sarlemijn in the title of his book *Between academy and industry*. This book deals with the management philosophy of Hendrik Casimir, one of the Nat.Lab.'s managing directors. The title highlights the ambiguity of the work of scientists in an industrial research laboratory context. On the one hand, such people feel the need to do scientific research that will result in new knowledge on natural phenomena, but on the other hand they know that they are working for an industrial company that can only survive if a sufficient number of new and successful products are brought onto the market. In tracing the history of the Nat.Lab., we will constantly be aware of these two tensions.⁵ It is intriguing to see how the Nat.Lab. has struggled to resolve, or perhaps rather to live with, these tensions during the course of its existence.

Periodisation and Structure of the Book

In each historiography there is a certain need for periodisation. Even though the transition between periods is never absolute and there is always a certain degree of smoothness and continuity, we can still identify different phases in the history of the Nat.Lab. In defining these phases, the central theme functions as a guideline. We have to search for changes in the role that the Nat.Lab. fulfilled for the Philips company if we are to identify the transitions from one period to the next. The first transition can be identified to have taken place shortly after WWII when a formalisation of

the structure of the Philips company took place. This formalisation for the Nat.Lab. caused a different mode of co-operation with other parts of the company. A second transition can be identified to have taken place at the end of the 1960s when economic growth in the Western world and the growth of the Philips company came to an end. During the same period, two other events took place: there was a change in the growth of Nat.Lab. personnel, and there was a change at the top management level. As the Nat.Lab. role changes were smooth, these concrete events will be taken to define the end of one period and the beginning of another. In 1946 the growth of the number of personnel accelerated, and in that same year Holst, the lab's first director, retired. He was succeeded by three directors, but it was Casimir who was to be the most important one. He became a member of the company's Board of Management in 1956. In 1966 the growth of the Nat.Lab.'s employee population ended, and in 1972 Casimir retired. He was succeeded by Pannenburg who in turn was succeeded by Van Houten, in 1984. Van Houten was succeeded by Carrubba in 1992. The 'post-Casimir' period will be dealt with as one continuous period, as there were no fundamental changes during this period in terms of the main theme of this historical study.

On the basis of these considerations, the following periodisation has been created:

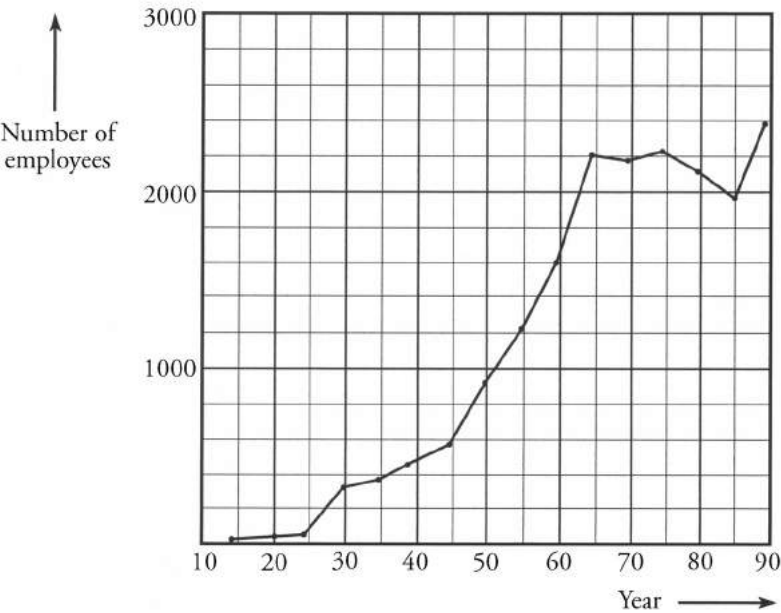


Figure 1. The number of Nat.Lab. employees in the 1914-1990 period.

1. the 1914-1946 period (the 'Holst period'). In this period the laboratory functioned as an integral factor in the diversification of the company's product range. This period will be described in two parts: a Prologue concentrating on the years 1914-1923 when the lab was run by just a few scientists and some assistants ('the early years'), and the years 1923-1946 when the lab went through an enormous growth process related to the diversification and growth of the company. In the first *Intermezzo*, the years 1940-1946 will be described separately so that WWII and the new formalisation of the company's structure can be dealt with;
2. the 1946-1972 period (the 'Casimir period'). In these years the Nat.Lab. functioned as an autonomous entity among the autonomous product divisions, each of which had its own development lab(s). These divisions were the result of the formalisation of company structure. In these years research was based on the assumption that the product divisions should have little direct effect on the research programme. The second *Intermezzo* will describe the years 1966-1972 as a period of transition when the attitude towards scientific research for technological development changed both outside and within the company, and as years of economic change;
3. the 1972-1994 period (the 'Pannenburg-Van Houten-Carrubba period'). This was the era when the role of the Nat.Lab. gradually became more closely tuned to the needs of the product divisions. At first sight, this may seem like a return to the first period, but that is misleading. The Nat.Lab. and, indeed, the company as a whole were much larger and more formalised in the years 1972-1994 than between 1914 and 1946. The short and informal communication channels of the early years were not to return. Instead, a formal route towards mutual commitment had to be taken in which formal agreements were to play a much more important part than in the first period. The year 1994 has been chosen as a closing year because by then the steps towards mutual commitment had been completed for the time being. Also, 1994 brings us very close to the present day.

This periodisation determines the structure of this book. Each of the periods is described in succession: the years 1914-1923 in the Prologue, the years 1923-1946 in Part I, the years 1946-1972 in Part II and the years 1972-1994 in Part III. Each part consists of two chapters. The first chapter contains a description of the period in terms of the goals, means, structure and culture in the laboratory. The second chapter of each part gives a number of case studies to illustrate the general characteristics of that particular period. This approach results in three different pictures of the Nat.Lab., one for each period, rather than the 'year-by-year' historical account that would have resulted from a more narrative approach. Consequently, the focus is on the dynamics from one period to the next, more

than on the dynamics within each period. Two Intermezzi will be used to describe the years of transition (1940-1946 and 1966-1972). In the Epilogue some overall conclusions will be drawn about the role of the Nat.Lab. within Philips as a whole.

2. Prologue:

The Nat.Lab.'s Early Years (1914-1923)¹

Before the early years of the Nat.Lab. are described in this chapter, a short impression of the early years of the Philips company will first be presented in order to provide a context for the start of the Nat.Lab. Some remarks will then be made about the technological and industrial developments taking place in the Netherlands in that time and about the emergence of industrial laboratories in the early twentieth century.

The Early History of the N.V. Philips' Gloeilampenfabriek (1891-1923)²

According to Blanken the success of the Philips company in the 1891-1923 period can be ascribed to three factors. Firstly, the company was able to produce light bulbs of a high quality in an efficient way. Secondly, the company was constantly successful in finding new markets for its product. Thirdly, the company's financial management was very carefully supervised. The combination of these factors was the result of the co-operation of the two Philips brothers who led the company, Anton and Gerard. In Europe the Philips company, which had specialised in making light bulbs as its only product, entered into sharp competition with many small and medium-sized companies, most of which sold light bulbs as one branch in a broader product range or even as by-products. In the USA it was one company, General Electric, that dominated the market. This company had set up a laboratory in 1900. In this lab W. Coolidge had, in 1910, developed a way of drawing thin wires as filaments for tungsten wire lamps. That was quite a step forwards compared to the usual lamps, in terms of solidity and light output. In 1913 it was the theoretical insight of Irving Langmuir in the same laboratory that led to the development of the half-watt lamp with a spiralled tungsten filament. That lamp used only one-fifth of the energy of the carbon wire lamps that until then had been the standard lamps. Langmuir had conducted extensive studies into the heat-conducting properties of gases, and from those he had concluded that the use of thick wires would result in less heat emission. Because thick wires would require large currents to produce sufficient light, Langmuir continued his search and discovered that spiralling a thin wire would give a result similar to a thick wire. These two inventions illustrate that the

General Electric company, and notably its scientific laboratory, was the main source of innovations in those years. This leading role enabled General Electric to also influence the European situation. The companies that obtained a GE licence were in a position to seriously threaten those European companies that did not get such a licence. The German market thus became a threat to Philips when GE drew up a contract with AEG, Siemens & Halske and the Auergesellschaft, three major light bulb-producing companies, to form what was known as the Patentgemeinschaft. The Patentgemeinschaft was in a position to set a limit to the number of light bulbs Philips could sell. This forced Philips to search for opportunities in the USA. In order to avoid what might possibly become dangerous competition on the USA market, General Electric was prepared to offer a licence contract to Philips and to reduce the limitations that had been set by the Patentgemeinschaft. In 1919 the contract was drawn up and this was to be the start of a long and good relationship between the two companies. It was while negotiating for this contract that Gerard and Anton Philips realised that there were risks attached to being dependent on the knowledge of other companies and their patents. The introduction of a new patent law in the Netherlands made this concern even more urgent, and this would lead to the decision to set up their own physics laboratory.

Technological and Industrial Developments in the Netherlands (1890-1920)

The first years of the Philips company coincide with a period in the development of technology and industry in the Netherlands which, for a number of reasons, can be called a period of important changes.³ Around 1890 a railway infrastructure had been created. The transition to steam power had been made, a number of applications had been found for electricity, and mechanised production had started to grow to a large scale. Not only in the Netherlands but also in other European countries a second period of economic prosperity started around 1890, after a first period of welfare between 1846 and 1873 that had been followed by a depression. A number of new key technologies emerged: electricity was brought to the whole country, cars and planes powered by combustion engines were invented, the telephone, a number of new industrial chemical products (such as pharmaceuticals, synthetic threads, detergents and petrochemical products), and steel production became widespread. The electrical and chemical industries were both knowledge-intensive sectors, and in this respect it can be understood why scientific laboratories were set up, first in the chemical and later in the electrical industry. It was the first-mentioned type of technology, electrification, that was of particular importance to Philips. In the Netherlands this process started around 1880, and by 1930 it had to a large extent been completed.

Another important factor was the emergence of multinational companies, in particular in the years 1880-1920. Philips is an example of this. Other Dutch companies that grew substantially in this period are known today as Shell ('Koninklijke Petroleum'), Akzo and DSM. These were all chemical companies. The rapid growth of these companies can be explained by the growth in world trade, the emergence of new technologies creating possibilities for new products, and the stimulation of a competition process that gave rise to mergers and co-operation. For Philips this latter factor became visible in the vertical integration of the bulb production that was taking place during this period. In 1915 the company built a glass factory of its own so that it could become independent of German glass suppliers. Although the practical need for this arose from WWI rather than from a conscious vertical integration strategy, the advantages of vertical integration, such as improving product quality, soon became evident. Multinational company growth created a need for different types of management (such as differentiated line and staff management). The Incorporated Company as a judicial form was also more common than before. Here, too, Philips can be given as an example. This factor also clarified the emergence of industrial laboratories where it was particularly the large, multinational companies that had the resources required to set up such laboratories.

The Emergence of Industrial Laboratories

An analysis by Hutter⁴ has shown that in the years 1905-1930 there was a substantial increase in the number of scientific laboratories in the Netherlands. Hutter studied six categories of laboratories in his analysis: laboratories in universities and colleges, government laboratories, laboratories funded by governments and industries and/or private institutes, the laboratories of private institutes, industrial laboratories and laboratories funded by associations and foundations. In the case of all these types of laboratories, numbers started to increase around 1905. If laboratories for clinical diagnosis are included in the survey, the rise of the number of private laboratories starts earlier, around 1890.⁵ This development in the Netherlands was not unique. In other countries, too, scientific laboratories were being founded. The role of scientific experimentation and measurement in technological development had clearly increased. Although the term 'Second Industrial Revolution' may be too emphatic for this phenomenon,⁶ at least a certain 'scientification' of technology can be claimed to have taken place, in particular in the fields of chemical technology and electricity. In the USA several of the larger industrial companies started up their research laboratories at the beginning of the twentieth century. The General Electric lab was set up in 1900, the chemical company Du Pont in 1902, AT&T between 1910 and 1912, Eastman Kodak in 1910, and West-

inghouse in 1916. In Germany, Siemens set up its *Physikalisch-Chemisches Laboratorium* in 1905; Telefunken started its lab in 1914. In England, the Marconi Research Centre was founded in 1912 and the General Electric Company's lab in 1919. L.S. Reich⁷ mentioned new patent legislation as an important practical motive in the foundation of such labs. In the Netherlands a new patent law was passed in 1910 and the new law became effective on June 1, 1912. There had been patent legislation in the Netherlands before, but that had been abolished 43 years prior to the 1910 legislation. In the first months following June 1, 1912, the percentage of Dutch applications for patents was very low compared with the number of foreign applications (12% Dutch against 88% foreign). Foreign companies were very keen to establish sound patent positions in the Netherlands that would give them sufficient freedom to act on the Dutch market. For Philips in particular, the activities of the *Patentgemeinschaft* forced the company to consider the need for creating its own patents.

Bernard Carlson has described the emergence of industrial laboratories, not only in the Netherlands but worldwide, as a transition from one situation in which innovations were a matter of individual inventors working in isolation and often on the basis of a brilliant spark of insight towards a new situation in which inventing was seen as an activity that takes place in groups of people and by systematic study. 'From heroic invention to industrial science' was the title he chose to indicate this transition.⁸ Thomas Hughes is fairly critical about the negative impact that organised research can have on the creativity of individuals.⁹ Alfred Chandler, on the contrary, saw the emergence of the industrial laboratories as a natural, and perhaps even necessary, consequence of the overall changes that took place in the growing industrial companies in the early twentieth century.¹⁰ The practice of industrial laboratories has shown that both Hughes and Chandler were right in a certain respect. The tension between freedom in doing research and the need to manage these activities described in Chapter 1 in fact mirrors the two perspectives that they present.

Three types of tasks may be distinguished for industrial laboratories.¹¹ In the first place, there is the task of quality control where existing production processes are concerned, the preparation of chemical materials, and the solving of day-to-day practical problems ('trouble-shooting'). A lot of testing and quality control measurement were done in private laboratories. The more precise definition of variables that was needed for measurements in these labs yielded the opportunity for more theoretical foundations to be established for the measurements, and this, in turn, demanded more scientific schooling of the laboratory workers. This increase in the role of scientific theory for the laboratory later caused development work to be defined as a separate laboratory task. Secondly, there was the task of

knowledge acquisition for innovation and improvement based on existing insights and theories (this may be called knowledge for a 'development' task). A third type of task was the acquisition of new knowledge of phenomena that may result in new long-term developments (often called 'fundamental' research). The categories of tasks described above can be used to study the nature of the Philips Nat.Lab. in its early years.

Different Types of Work in the Nat.Lab.'s Early Years

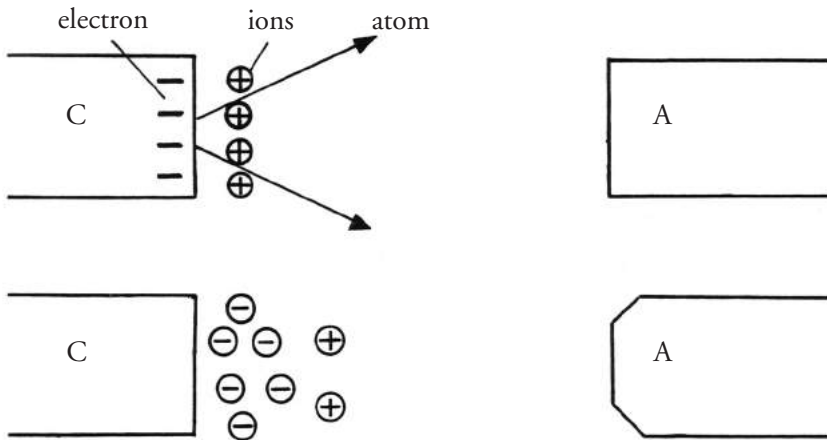
When Holst heard of his appointment with Philips, he perceived his primary tasks to be the following: 'I am to be given a whole laboratory to equip, and I shall carry out all manner of investigations that will teach us the formula of the incandescent lamp.'¹² This task was directly related to the aim of safeguarding the company's patent position in lamps as the main reason for starting up the laboratory. According to Heerding,¹³ the second part of Holst's task description referred to the half-watt lamp. By the time that Holst was appointed, Philips technicians were able to produce copies of that, but many of its characteristics were not understood, and as a consequence it was not possible for them to improve on this type of lamp. Heerding claims that this problem was one of the motives for starting the Natuurkundig Laboratorium. From Holst's words it can be deduced that quality control was not to be Holst's primary concern. In fact, a testing department in which lightbulbs were tested already fulfilled that task. For the solving of practical production problems, there was a chemical laboratory, located one floor above the room where it was intended that Holst would create his physics laboratory.¹⁴ J.C. Lokker, who had joined the company in 1908, was in charge of this chemical lab. In practice, the lab analysed and prepared chemicals for filaments. The nature of this laboratory had changed a little when one of its scientists, L. Hamburger, had started to work on understanding the phenomena on which the half-watt lamp was based. Hamburger's work had not yet resulted in a full understanding of the half-watt lamp's functioning. Gerard Philips expected physical research to provide a different viewpoint and to yield new insights that would enable the company to become less dependent on external knowledge and patents owned by other companies. Hamburger's half-watt lamp studies were of a physical-chemistry nature. His studies into the phenomenon of gas discharges had already brought him into the field of physics.¹⁵ The disciplinary boundaries between the chemistry lab and the physics lab were therefore not very clear, which was why it was not surprising that Holst and Hamburger published an article together.¹⁶ In terms of the types of tasks carried out, the boundaries were not always clear either. The mere existence of the chemical lab did not mean that Holst was freed from doing testing, measuring and troubleshooting work. One of Holst's activities in the early years of his lab was to

develop set-ups for photometry measurements for testing purposes. Furthermore, in 1915 when the Philips company started up a glass factory of its own, the physics lab assisted the production process by measuring the transparency of different types of glass. Not long after that, a gas factory was also set up because the argon and nitrogen required for filling the light bulbs could no longer be imported from Germany. Here, too, the Nat.Lab.'s assistance was called for.

Three years after the physics laboratory had begun, Holst felt the need to make explicit the way in which 'his' laboratory was different from the chemical laboratory. By then, Hamburger had already left the chemical lab (commonly known as Lab V, because it was on the fifth floor; the physics lab likewise became known as Lab IV because it was on the fourth floor), and he was in charge of a Physics-Chemistry lab.¹⁷ On January 15, 1917, Holst in one of the weekly staff meetings that he had started convening that year, presented a survey of what he considered to be the physics lab's tasks. Besides emphasising the physics of the half-watt lamp, he mentioned the need to search for possible new light sources. In particular, the phenomena of gas discharges and fluorescence had captured his interest. Gas discharges constituted undesirable effects in filament lamps. To reduce the evaporation of the filament, a gas, for example argon, was put into the lamp. But it had been discovered that large currents could be passed through the gas rather than through the filament. This could blow supply fuses. One of Holst's trouble-shooting tasks had been to search for different gases or gas mixtures to reduce this effect. Together with an assistant, A.N. Koopmans, who had joined the lab in 1916.¹⁸ Holst had found a mixture of argon and nitrogen that served this purpose reasonably well. The phenomenon of gas discharges was also to serve as the basis for a different type of light source: the gas discharge lamp. Neon tubes already existed, but they only produced red light, which meant that they were suitable for advertisement purposes but not for in-house lighting. Holst wanted to explore the possibilities of turning the gas discharge lamp into a domestic product.

The nature of that work was different from testing and quality control work. This can be seen when we analyse the approach Holst took. He worked with E. Oosterhuis, the Nat.Lab.'s second scientist, who had joined Holst on April 16, 1914. Hamburger had done experiments with gases such as nitrogen, hydrogen, argon, neon and helium and had found that a mixture of helium and neon yielded the best results for gas discharges, but that the efficiency was low for these gases. Holst and Oosterhuis adopted a different approach and tried rather to describe the phenomenon itself, in terms of what happened in the tube cathode and in the gas. They found that in a cold-cathode tube the breakdown voltage – at which the discharge starts – depended on the material of which the cath-

ode was made. This led them to believe that the discharge was initiated by positive ions that released electrons from the cathode.¹⁹ Here we see how Holst and Oosterhuis did not randomly try out various gases, but tried to gain an understanding of the electron behaviour in order to explain the phenomenon of gas discharges. This sort of approach may be termed ‘basic’ in that it aims at understanding the basic principles underlying macroscopic phenomena (the alternative term ‘fundamental’ will be reserved for research that is not (yet) related to a concrete product). Furthermore, Holst and Oosterhuis found that at a low cathode temperature, sputtering took place (i.e. that cathode material is dispersed to other places), but that this effect decreased when the cathode temperature was increased, and an arc discharge was produced. Holst and Oosterhuis again tried to understand these phenomena in terms of electron and ion behaviour. The ions would be attracted to the cathode at low cathode temperatures, while at higher temperatures many electrons could escape from the cathode and neutralise the ions. This prevented the ions from reaching the cathode and causing sputtering. Their investigations led to the tungsten arc lamp, which they presented to the Nederlandse Natuurkundige Vereniging (Dutch Physical Society) in 1921. It is not entirely clear whether



Upper part of figure

Cold cathode. Positive ions are attracted towards the negative cathode – sputtering of the cathode takes place.

Lower part of figure

Hot cathode. Thermionic emission of electrons takes place from the cathode, and a negative space-charge is formed in front of the cathode. Positive ions arriving are neutralised, and no sputtering takes place. There is always an excess of electrons which allows the discharge to continue.

Figure 2. Cold and hot cathodes (from Garratt 1976 Vol. 1, p. 81).

it was the understanding of electron behaviour that initiated this new type of lamp, or whether it was just the practical know-how that Holst and Oosterhuis had gained through their experiments that led to this. But somehow it was the search to understand the phenomenon that had led to a new light source, and this was to give Holst a reason for emphasising the relevance of that sort of research for his laboratory and for the company.

The phenomena-oriented and development-oriented types of research activities carried out by Holst and Oosterhuis resulted in a number of patent applications, constituting one of the original intentions for starting the physics laboratory. In a list compiled by the Philips Patent Department, we find the following titles for the first five years of research work: Improvement of electrical light bulbs for projection purposes (submitted June 11, 1914, Holst); Device for the renewing or refilling of the gas in discharge tubes (August 30, 1918, Holst and Oosterhuis); Method for making discharge tubes, light bulbs, vacuum tubes, etc. by using alkali or earth alkali metals to remove the last residual gases or to purify inert gases (September 23, 1919, Holst and Oosterhuis); Transfer current guide means for large electrical currents, suitable for transfer through a glass wall (October 9, 1919, Holst and Oosterhuis); and Electrical lamp with a capacitor that can be integrated into it (December 5, 1919, Holst).

From these titles we can already see that the phenomena-oriented and development-oriented types of research activities started to broaden the research programme, making it extend from light bulbs and gas discharges to other types of gas-filled tubes. The second mentioned patent was applicable to various sorts of tubes, like X-ray tubes, Geissler tubes, and tubes for Moore light.²⁰ But in these early years the scope remained limited to light bulbs and tubes. For that reason there was not yet an urgent need for a large lab population. According to Bol, Gerard Philips used to visit the lab every Saturday morning at 11 a.m. to see what was going on.²¹ There is no doubt that Gerard fully supported the lab, even though he and Holst may not always have had the same ideas about its research agenda.²²

The Small Population of the Lab in the Early Years

In April 1914, Holst and Oosterhuis were the only two scientists at the Nat.Lab., and it would remain that way until 1923, apart from the short periods during which S. Weber, A.N. Koopmans²³ and G. Hertz worked in the lab. Holst's and Oosterhuis's background fitted in with the nature of the work that was expected from them.

Holst had been born in Haarlem on March 20, 1886. His father was the director of a shipyard, and after finishing secondary school Holst worked there for a while. Apparently, the practice of mechanical engineering had not appealed to him very much, because he soon went to

study electrical engineering at the Zürich Eidgenössige Technische Hochschule (ETH) in Switzerland, but this too was not what he was looking for, and after two years he switched to physics. In 1908 he was awarded his diploma. After that, he worked as an assistant of Professor H.F. Weber. From 1910 onwards Holst worked at the Kamerlingh Onnes low-temperature physics laboratory in Leyden until he moved to Philips. The Kamerlingh Onnes laboratory had an excellent reputation in experimental physics. Holst was still working on his Ph.D. thesis when he applied for the position at Philips. In 1914, he successfully defended this thesis at the ETH, where he had also studied. The subject of the thesis was the thermal properties of ammonia and methyl chloride. We do not really know why Holst left Leyden to go to Philips, but his move is remarkable given the reputation of the Kamerlingh Onnes lab.²⁴ Casimir, one of his successors in the Nat.Lab., described him as a man of enthusiasm, who would regularly make witty remarks during discussions, straightforward, but at the same time almost shy.²⁵

E. Oosterhuis had studied at the University of Groningen. He had worked at the same physics laboratory in Leyden that Holst had worked at. His thesis was about the thermo-electrical Peltier effect. S. Weber was the third member of the scientific team to join the lab in 1914. He only stayed until 1916. He too came from the Kamerlingh Onnes lab in Leyden. Maybe it was felt that the physics lab in Leyden provided appropriate preparation for work in the Nat.Lab. This preparation involved a combination of experimental abilities and a focus on the understanding of phenomena that explain the behaviour of matter. The work that Holst and Oosterhuis did (a combination of practical measurements for troubleshooting and searching for an understanding of the underlying phenomena) indeed required such abilities.

In 1920, G. Hertz was persuaded to join the Nat.Lab. At that time, he was already well known, notably for his experiments with J. Franck on line spectra. He could therefore afford to ask for a salary of 20,000 Dutch guilders, which even exceeded Holst's salary. Although he was offered only 15,000 guilders, this was still a considerable amount for that time. Hertz was specifically asked to study line spectra in gas discharges. His presence is a clear signal that the understanding of phenomena related to the Philips products was seen as an important task for the Nat.Lab. in the early years.

Assistants were appointed to do the more practical work. From Holst's collection of notebooks, it can be learned that he worked with R.K. Kloppers, who had been appointed in 1914. In 1915 C. Bol was appointed as an assistant, in 1917 J. van Beest and in 1918 G.J. Sizoo.²⁶ The assistants received additional schooling from the scientists: Holst taught mechanics and thermodynamics, Oosterhuis taught electron theory and radiation

theory, Weber lectured on kinetic gas theory and Koopmans – who had come from the chemical lab after Hamburger had left in 1917 – lectured on Maxwell's theory.²⁷ This is an early example of a role that the Nat.Lab. would also play as a place where Philips employees received (further) scientific training.

Cornelus Bol is an example of an assistant who during his years at the Nat.Lab. succeeded in making considerable progress in scientific expertise. Trained as a carpenter by his father, he had also learned to do glass and metal work. He had been an assistant in the low-temperature laboratory in Leyden, where Holst and Oosterhuis had worked. Later he had taken a course to become an electrician. In the USA he gained a bachelor's degree in science. His work in the Nat.Lab. would gradually move beyond the simple practical experimental level that assistants normally worked at.

The Transition into a Period of Growth and Diversification

To summarise, the early years at the lab give the impression that it was a small laboratory of a hybrid nature in which a variety of tasks were carried out, such as quality control and trouble-shooting, development-oriented research and research aimed at gaining a deeper understanding of phenomena. The phenomena-oriented and development-oriented research activities focused on light bulbs and related phenomena, but would soon diversify to other types of bulbs. This situation remained the same for eight years.

In 1923 the lab moved to new premises, and this was the start of a period of enormous growth. This growth was related to the diversification of the company. In 1922 the company moved into the business of radio tubes.²⁸ In terms of production technologies, that was not a difficult step. A new staff member, B. van der Pol, was appointed in 1922 to work on radio tubes. In 1922 Philips also set up a working relationship with X-ray Ltd in the UK to produce X-ray tubes (here, too, there were parallels with the production of light bulbs), and A. Bouwers in the Nat.Lab. started to work on that.²⁹ These two new products marked the beginning of an enormous diversification and growth process that the Philips company was to go through. The Nat.Lab. had to do the relevant supportive work, and so it started growing as well. In 1923 an end came to the early years of the laboratory. There was continuity in that the research programme was largely determined by the company's product range. Until 1923 there was only one product, namely the light bulb, and that was why the lab could remain small. In 1922 Gerard Philips retired. Until then he had been formally responsible for the laboratory. This shows the direct involvement of the company's directorate in the Nat.Lab.'s existence. After Gerard's retirement, Anton Philips also had direct contacts with the lab's staff. For the

entire 1914-1946 period, the communication links between the lab and the rest of the company were close. In this respect, too, the year 1923 did not mark a point of discontinuity. But there was change in that the strong growth of the lab intensified all sorts of tensions and problems that are characteristic of a professional organisation.

PART I

The Nat.Lab. as a Growth and Diversification Factor
in the Philips Company (1923-1946)

3. Developing a Research Organisation for a Diversifying Company[†]

3.1 Philips and Technological and Economic Developments in the Netherlands in the 1923-1946 Period

The Netherlands was neutral during WWI. As a consequence, relatively few resources had to be spent on military issues. At the same time though, because of the war it was difficult for the country to import goods from other countries. Countries involved in the war often feared that goods would find their way to the enemy via the Netherlands. The country became isolated as well, and as in other countries, this created the need for substitutes for all the goods that could no longer be obtained elsewhere.

For Philips this isolation resulted in the starting up of X-ray research in the Nat.Lab. As in other European countries, the first post-WWI years reflected positive economic development. During those years Philips profited in the light bulb industry from the fact that other countries like Germany, England and France had made little progress during the war. In 1920-1923 the Netherlands went through a short period of economic depression, but the effects of this were not very dramatic due to the fact that the international trade did not decline.² For Philips profits dropped sharply in the years 1921 and 1922, but this was partly as a result of the company's own financial policy (large write-offs on the exploitation account).³ Light bulb prices dropped, but this was compensated for by an increase in productivity. In 1924 the company's financial results surpassed those of 1921. For the country as a whole, that year also marked the beginning of a new period of economic growth. In 1923-1929 it was particularly the Dutch industrial sector that benefited from the economically favourable circumstances.⁴ For Philips it was the formation of the Phoebus cartel (formally the General Patent and Business Development Agreement) which was at this period an important factor in the consolidation of Philips' position in the light bulb market.⁵

During the same period the company became involved in the new up and coming radio industry. In 1924 Anton Philips decided to extend the production of radio receiver tubes. In 1926 he decided to start producing complete radio receiver sets. Consequently, a number of existing technologies had to be integrated into one product, because a radio set also requires electrical circuits and loudspeakers. Later the company also started producing transmitter tubes. It thus evolved from a device-producing

company to being a company involved in the complete radio system. Radio sets were produced in the Nederlandse Seintoestellen Fabriek (Dutch Signalling Apparatus factory) in Hilversum, a city near Utrecht. Philips had a 60% share in this factory. The sets sold well, and between 1927/8 and 1929 the production rate doubled (from 6,000 per year to 12,000 per year). The company grew rapidly. In 1922 there were about 5,000 employees working at Philips, but by the summer of 1927 this number had grown to about 8,000. In 1928 the company had more than 20,000 employees. Via the radio, the company moved into other sound reproduction fields and started dealing with amplifiers and equipment for sound films and also making plans to move into the sound recording industry.⁶ According to Blanken, the growth and diversification of the company were facilitated by the fact that Philips, unlike most other light bulb-producing companies, was not connected to a mother company, which meant that Anton Philips was free to spend the profit made by the company on starting up new businesses.⁷

In the early 1930s the country was dragged into the worldwide economic crisis that began in 1929. Compared with other European countries, the influence that the first years of the crisis had on the Netherlands was minor, but when in 1933 the situation had started to improve in other countries, the Dutch economy had still not recovered. In 1936 there was a brief revival, but 1937 and 1938 were again problematic years.⁸ The improvement seen in 1939 would only have a brief impact because by 1940 the Netherlands was involved in WWII and was occupied by the Germans.

The economic crisis had great consequences for Philips, too. Blanken described how in the 1930s the company suffered from a combination of internal organisational problems due to the rapid growth of the company and to the change in the economic climate.⁹ Drastic reorganisations had to be made, in which about 10,000 employees were dismissed and a new budget system for the whole company was introduced. At the same time the company was able to make use of the opportunities provided by the new radio market, thus softening the blow of the economic crisis in its early years. Later on, it became evident that further diversification would be necessary if the continued economic problems were to be counterbalanced, and in 1937 the company again decided to extend its product scope. The choice fell particularly on the fields of telecommunications and products for industrial applications.¹⁰

The success of this strategy of diversification could be attributed to the fact that the consumer market kept growing in spite of the economic crisis. Between 1929 and 1939 the domestic use of electricity doubled.¹¹ The Nat.Lab. was expected to fulfil a supportive role in developing products for that market. What was known as an Orienterings Commissie (Orientation Committee) was established to discuss the way in which the com-

pany's product policy should be developed. The Nat.Lab. was represented in this committee. This will be dealt with in more detail in section 3.5.

During this period, the climate for scientific labs was generally positive. Not only industries but also the government recognised the importance of scientific research. In 1930 the TNO organisation for Applied Scientific Research (Toegepast Natuurwetenschappelijk Onderzoek) was set up by the government. A new academic programme for Technical Physics was also started up at Delft Polytechnic, which increased the possibilities for contacts with the academic world for industrial labs such as the Nat.Lab.

3.2 Supporting Company Diversification as a Nat.Lab Goal

In the early 1920s the Philips company experienced enormous growth. The product portfolio extended from light bulbs to radios and many other products. What part did the Nat.Lab. play in that diversification? That is the question that will be focused on in this section. How did the company's directorate (in practice this was Anton Philips) influence the Nat.Lab.'s research policy? What made the Nat.Lab. different from factory laboratories, such as the Philite laboratory, which was mainly involved in testing and control activities? To investigate this, the process of product diversification in the company and research diversification in the Nat.Lab. will now be described.¹²

From Light Bulbs to a Variety of Tubes

The first step in the process of diversification was to go from light bulbs to a variety of tubes. From a physics and technology point of view, that is not a big step. In all cases glass, vacuum and gas discharges are important.

In the first place it is appropriate to mention here the gas discharge lamps. The early days of work on gas discharges have been described in the previous chapter. To make a gas discharge lamp, not only gas discharges were needed but also a mechanism to transform the gas discharge radiation into visible light of the right colours. In 1923 the physicists W. de Groot and C. Zwikker were appointed to work on this in the laboratory. These two scientists found that when combined with uranium derived from uranium glass as a fluorescent material, mercury gas discharge yielded good results.¹³ The green light that this lamp produced illuminated the Philips light tower in Eindhoven for a number of years. In 1923 Anton Philips had seen neon lamps in Paris, and he had decided that his company should start producing such lamps as well. Then Holst got the electrical engineer R. Vermeulen and the physicist G. Zecher to work on the construction of these lamps.¹⁴

The X-ray tubes constituted a second extension of the product range.

Around 1900 such tubes had been developed by the General Electric Company. Clearly, the Nat.Lab. had initiated a new and uncertain business for the Philips company. During WWI Holst had accepted requests from hospitals to repair their X-ray tubes. By 1918 the lab had made such progress in the understanding of the principles of X-ray tubes that it was able to manufacture its own tubes. In fact, these tubes were copies of a German type that the hospitals could no longer buy. In the years 1922-1924 A. Bouwers worked on the development of a new type of X-ray tube, that was later to be called the Metalix. Thanks to the method that Holst and he had developed (see further on in this section) for connecting glass and metals via chromium-iron alloys, Bouwers had been able to create an X-ray tube that had a small metal window through which only the X-rays could emerge. In addition, the tube was more compact than the usual tubes. Through his enthusiasm, Bouwers was able to convince Anton Philips that the company should start producing this new tube.¹⁵ According to Blanken, Anton wanted to give the company the image of being a technologically advanced company. As the Metalix was not a consumer product but only made for specialists, generating profit was probably not one of Anton's motives in this particular case. From a commercial point of

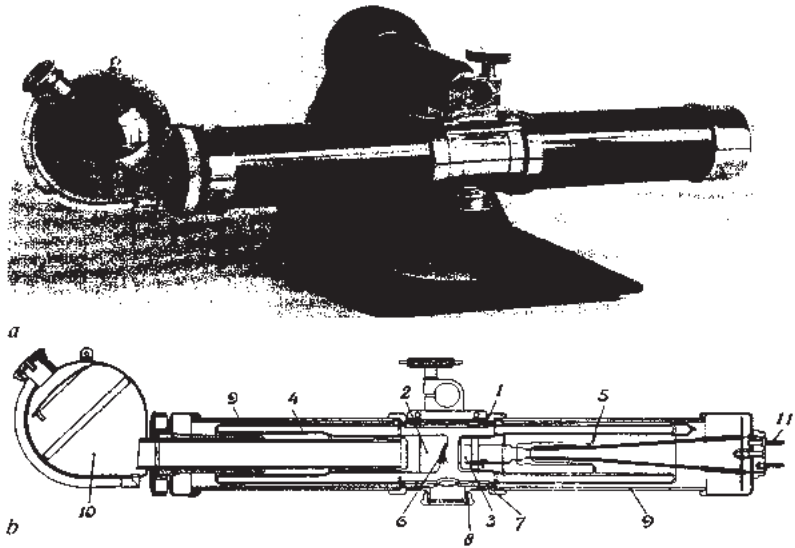


Figure 3. The Metalix (from Philips Technical Review Vol. 42, p. 352). Electrons are emitted by cathode 3 and hit the tungsten target 6 on anode 2. This causes X-rays to be emitted that leave the tube via window 8. The rest of the X-rays are shielded off by a lead shield 7. The tube is mounted in a ferrochromium envelope 1 and Philite (a sort of bakelite) cylinders 9, and is water cooled (10 is the cooler). 4 and 5 are glass connections between the anode and envelope, and between the cathode and envelope.

view, this was not a very successful business for Philips, but Anton Philips decided, nevertheless, to continue involving his company in it. Thus, the X-ray tube was to become one of the company's first products in the professional field.

In third place there were the radio receiver tubes. Until 1924 research into receiver tubes in the Nat.Lab. more or less followed developments being continued elsewhere, namely by RCA, Marconi and Telefunken.¹⁶ In September 1922, B. van der Pol joined the Nat.Lab. to lead research in this field. Van der Pol had studied mathematics and physics and already had a good reputation. It is evident that Holst wanted him to join the staff because of his specific expertise in this area. Van der Pol started his work in the Nat.Lab. by developing new types of receiver tubes. In particular, he endeavoured to improve on the low energy use of the tubes that RCA and Marconi produced. Initially, the work did not yield results any better than those produced by RCA and Marconi, but by concentrating much of the total research effort on this problem – according to one of Holst's notebooks, 9 of the 16 scientists were working on this – the study into the effect of a layer of barium oxide on a tungsten wire had, by 1924, yielded a procedure for making what were known as oxide cathodes. The oxide cathodes had a higher electron emission for the same amount of input energy. The Nat.Lab. was thus able to catch up with developments unfolding in other companies.

From Radio Receiver Tubes to the Whole System of Radio

The decision to produce complete radio sets, and to make transmitter tubes as well, meant that the Nat.Lab. would substantially extend its research programme. One by one we will consider the elements of the total radio system that the laboratory worked on, namely the receiver, the transmitter, and the aspect of propagating of the signal through the atmosphere.

The signal in the radio receiver had to be amplified. For this purpose another type of tube was required, namely an amplifier tube. An important invention in the field of radio amplifier tubes was the Pentode. The Nat.Lab. scientist B.D.H. Tellegen, who had been educated in Delft as an electrical engineer and had joined the Nat.Lab. in 1924, had developed this tube in 1926.¹⁷ The search for better amplifier tubes was not only a matter for Philips. GE also was working on this, and in this company the work of A.W. Hull and N.H. Williams led to the tetrode around 1920, which had four instead of three electrodes as in the triode that had been invented by Lee De Forest in 1907. The Pentode, with five electrodes, was a further improvement, because the problem of undesired secondary elec-

tron emission had been solved. More frequencies could be amplified, and the distortion of the amplified signal was reduced. The Pentode patent was to be one of the most important patents in the pre-WWII Nat.Lab. history period. All over the world Pentodes would subsequently be produced under licence. The invention of the Pentode was the outcome of a combination of trying to understand the phenomena on which the amplifier tubes were based, and trying to find a creative way of solving the problem that was caused by these phenomena. The Pentode led to closer contacts with Bell Labs, because it had become evident that this tube was useful for telephony, which was one of Bell's core areas.

Apart from amplifying tubes, an electrical circuit was needed. Theoretical work on the amplification of oscillatory signals was done by Van der Pol. He investigated the concept of positive feedback: part of the output of the amplifier tube was fed back into its grid. That caused oscillations with a fixed amplitude. Van der Pol found out which non-linear equation could be used to explain this. This equation became known as the Van der Pol equation.¹⁸ It enabled Van der Pol to analyse the behaviour of amplifying circuits. It could be applied to many more things than just radio circuits. All sorts of oscillations could be analysed by means of the Van der Pol equation. Later, Van der Pol constructed a model for oscillations in a heart.¹⁹ Apart from having positive feedback, there was also the concept of negative feedback, in which case part of the amplifier output was fed back to the input signal, but in an opposing phase. This reduced the influence of undesirable fluctuations in the signal due to variations in for instance the supply voltages. In 1928 Posthumus started doing calculations on this concept in relation to radio circuits, and the physicist J.J. Zaalberg van Zelst later implemented the results in real amplifiers. This resulted in more stable amplifications.²⁰

Another electrical circuit was needed for tuning. The first radio sets brought out by the company in 1927 were of the 'tuned radio frequency' type. With such receivers, the signals went through a number of amplifying stages, each of which was tuned to the same frequency. The group led by Oosterhuis worked with this type. Van der Pol's group focused on a different principle: the super-heterodyne.²¹ With that type the incoming signal was mixed with the output of a built-in oscillator tube so that both the sum and the difference signals could be produced. The difference signal was then selected and amplified. Both types had their pros and cons. When it became more desirable to have a broad range of tuning frequencies (20 MHz to 150 MHz), it was the super-heterodyne that won the battle.

Transmitter tubes were intended for a totally different market than receiver tubes. Radio receiver sets were to be sold to consumers, but transmitter tubes for radio broadcasting were destined for professional use. With this

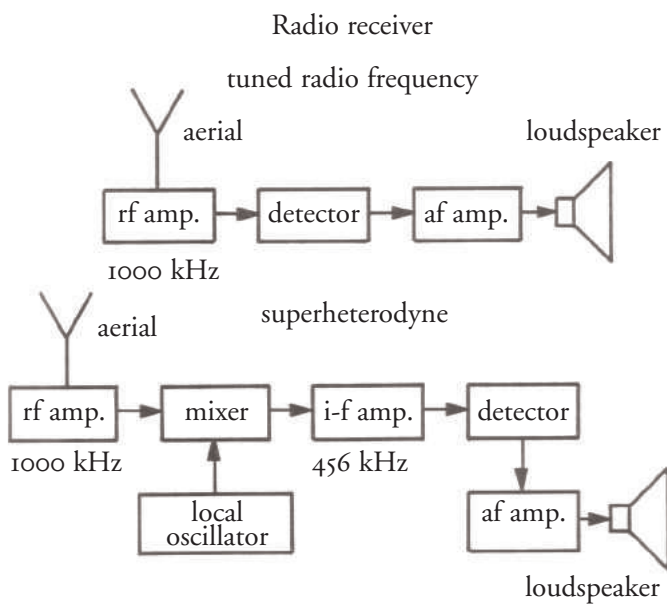


Figure 4. Comparison of the tuned radio frequency (straight) radio receiver and the super-heterodyne (super-het).

The super-het uses a fixed intermediate frequency (usually around 460 kHz) for the stages providing most of the amplification. This eliminates most of the problems of tracking tuneable circuits and allows band-pass circuits to be used (from Garratt 1976, Vol. 2, p. 85).

product, too, the Nat.Lab. was active in supporting the technological realisation of the company's decision. A new topic was thus introduced into the Nat.Lab.'s research programme. An important step forward was to be found in the chromium-iron melting technique, developed in 1923 by Holst and Bouwers. The story goes that they made this discovery by accident, after they had heard glass blowers complaining about the chromium-iron alloys (of which the glass blower's pipes were made). Those alloys easily melted into the hot glass. Holst and Bouwers realised that this same principle could be used to connect metals to glass. This was relevant to all sorts of tubes whereby a glass bulb had to be fitted onto a metal socket. This process enabled the Nat.Lab. to make water-cooled transmitter tubes of more than 20 kW.²² The Mullard Radio Valve Company in England expressed an interest in this process and negotiations between Philips and that company led to Philips taking a financial interest in the Mullard company. Many years later, after WWII, the Mullard lab became part of the Philips research organisation. As nationalistic feelings were quite strong at that time, Philips initially avoided associating its own brand name with the Mullard lab.

Not only high power, but also high frequencies for radio tubes was an important transmitter tube research topic. The main reason for initiating this research was because of the increasing number of frequencies that were needed for the various transmitters and because wider bandwidths were needed. When television broadcasting began, this issue became even more urgent. In 1925 J.J. Numans was appointed to do research in the field of transmitters. He developed a crystal-controlled transmitter that had a crystal frequency of 1656 kHz and controlled a carrier wave of 9.936 MHz.²³ The transmitter was used in 1927 in a short-wave radio telegraphy transmitter.²⁴ This transmitter could send signals to Bandung, Java, in the Dutch East Indies. On May 31, 1927, Queen Wilhelmina spoke into the microphone of this transmitter.

One type of tube that seemed to be particularly promising for transmitting high frequencies was the magnetron, a cylindrical diode with a magnetic field parallel to its axis. Its main application was radar. During WWI a lot of work was done on radar both on the German and on the British side.²⁵ When K. Posthumus at the Nat.Lab. started to work on the magnetron tube, precisely how it operated was not yet fully understood. Posthumus, who was educated as an electrical engineer and had joined the lab in 1924, found that the frequency was limited by the time it took an electron to travel within the magnetic field from the cathode to the anode via a curved trajectory. He then came up with an alternative design in which the anode was split into pairs. On September 6, 1933, he submitted a patent application for this new device.²⁶ The new magnetron tubes were used to set up a radiotelephony link between Eindhoven and Tilburg, a city about 30 kilometres away.

Finally, there is the matter of the propagation of the signal between a transmitter and a receiver. This area was particularly studied by Van der Pol and his group. Van der Pol and Van der Mark, for example, studied the phenomenon of round-the-world echoes in relation to the properties of the ionosphere. Those echoes appeared to cause radio signal cross-modulation. Knowledge gained in the area of wave propagation was also used for determining the properties of aerials. Another development that took place after 1927 was the emergence of frequency modulation (FM) as an alternative for amplitude modulation (AM). Again it was the Van der Pol group who worked on this innovation. The concept FM was invented by Armstrong in the USA, but met with much resistance there. Van der Pol was impressed by the sound quality of the experimental FM broadcast, and Armstrong offered to sell a licence to Philips for the symbolic sum of \$1, in the hope that that would help his design to be implemented.²⁷ As it happened, the *Commerciële Afdeling* (Commercial Department) in Philips was not interested in the offer because the FM system would require quite different standardisation norms. Van der Pol therefore start-

ed experimenting and analysing the approach mathematically. It was not until after WWII that FM came to be generally accepted as a new standard. Van der Pol's work gave Philips a strong position at an early stage.

Other Areas Related to Radio: Acoustics, Perception and Recording

There is one other radio receiver set component that has not yet been discussed, namely the loudspeaker. This component was also a new product for Philips when the company decided to move into the area of complete radio set production. Moreover, it brought the company into an area that went beyond radio, namely acoustics. With the introduction of loudspeakers, the company became involved in sound production in auditoriums, concert halls, and other such venues, where acoustic properties were important. In 1924 Vermeulen started working on the improvement of a Western Electric speaker in the Nat.Lab., and in 1925 the physicist A.T. van Urk joined him.²⁸ According to Garratt, the Commerciële Afdeling (Commercial Department) doubted if the Nat.Lab. would come up with the necessary improvements, and so it started to look for an alternative. In 1926, however, the Commerciële Afdeling was forced to go into production using the Nat.Lab. design. Blanken demonstrated that during this period Anton Philips's influence was everywhere,²⁹ and more than once he expressed his confidence in the Nat.Lab (e.g. in the case of the X-ray tubes). Therefore, it might well have been he who forced the Commerciële Afdeling to adopt the Nat.Lab. design. In 1928 another Nat.Lab. design, the 'Sevenstar', started being produced. Another improvement was the moving coil system (invented by Rice and Kellog in 1924), which was given the nickname 'Meesterzanger' ('Master singer'). For this, contrary to what was common in those days, a permanent magnet was used namely an electromagnet. The success of these permanent magnets could be attributed to the materials research that had yielded a new type of steel, Ticonal. To support the research into speakers, not only into the cones but also into the baffle, the Nat.Lab. became active in the area of acoustic measurements. Thus, microphones became a research topic of the Nat.Lab. The acoustics of auditoriums and music halls were also investigated. All the measurements made were complemented by theoretical considerations. In 1929, M.J.O. Strutt, for example, did calculations relating to the frequency characteristics and directivity of the speakers, which, for that time, were quite innovative.³⁰ Acoustic advice was provided for several buildings, including the Utrecht town theatre and J. Walther Thompson's Bush House BBC studios in London.

A second field of research to accompany the sound (re)producing techniques was sound perception. This field was investigated by P.J. Bouma (not to be confused with H. Bouma who played an important part in this

field in later years) and J.F. Schouten, both of whom had studied mathematics and physics. Bouma also went on to do research into the field of colour perception (for lamp development). Much later J.F. Schouten became the first director of the Instituut voor Perceptie Onderzoek, IPO (Institute for Perception Research).³¹

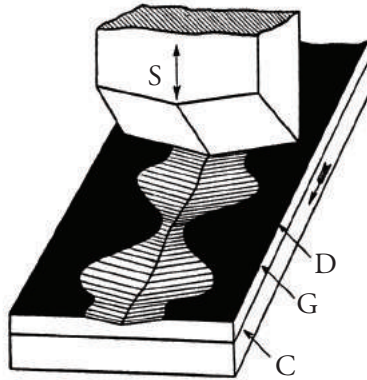
A third field that Philips became involved in due to its relation to radio via sound was recording. In 1928 the company's directorate decided to move into the areas of sound film and gramophone.³² In discussions on this matter, Holst suggested that these were appropriate areas to expand into because they were mass consumer product fields. He was not enthusiastic about television and facsimile, because according to him they were not to become mass consumer products.

In the USA the relationship between the radio and gramophone industries had become quite obvious: in 1929 the Victor Talking Machine Co. merged its gramophone activities with the RCA company to become the RCA Victor Co.³³ Radio and gramophone were seen as complementary to each other, and sometimes the two were combined in one appliance. The British Columbia Gramophone Co. Ltd. approached Philips and asked it to make a similar move. The director O.M.E. Loupart considered that the Nat.Lab. was not ready to make substantial contributions in this field and therefore held that co-operation with Columbia was necessary. But Anton Philips considered that his company was strong enough to produce its own radio-gramophones.³⁴ The company's Technisch Commerciële Afdeling (Technical-Commercial Department) also advised the company to start producing gramophone records, but the Nat.Lab. doubted that entering this field would be successful, given the strong position of other companies, unless a real innovation could be made. In 1930 a study was set up into extending playing time, reducing needle noise and improving the material that gramophone records were made of. In the meantime, Columbia found a new partner and Philips was only able to establish contacts with smaller companies, like Decca Ltd. in the UK. Capacity problems prevented successful entry into this market in the years 1927-1931.³⁵ Later on, these problems were solved, and entry into the market was realised.

Thus, Philips became involved in mechanical sound recording (gramophone). In the early 1930s a different system for sound recording, namely for sound films, was invented by J.A. Miller in the USA. In 1929 at a director's meeting, Holst had recommended that the company should involve itself in the sound film industry because of the electro-acoustic aspects. Although there was serious doubt among the directors, due to pressure from outside (L.C. Barnstein, owner of a film distribution company, had made a strong plea for Philips involvement), it was then decided that a

certain amount of sound film equipment should be produced.³⁶ After that decision was made, film technology was also studied in the Nat.Lab. J.F. Schouten did some work on sound film diffraction,³⁷ and J.J.C. Hardenberg investigated film transport.³⁸ Hardenberg's work was already linked to Miller's invention. This new Miller technology combined mechanical and optical ways of recording sound. Philips became interested in this system, because it maintained that it would open up the possibility to secure a better position within the market. At that time Philips was having to compete with international cartels that owned important patents in the film industry field.³⁹ The Miller system was a real innovation: a wedge-shaped stylus wrote a signal in an opaque layer on a strip of transparent film. The way in which the signal was reproduced was similar to the way in which a sound film was reproduced (with a lamp and a photocell). When Holst saw the system being demonstrated on April 25, 1932, he found it very promising and was subsequently able to convince the Philips directorate that co-operation with Miller would be useful. A Philips-Miller, Inc. was set up in the same year, and the Nat.Lab. started investigating the system. Vermeulen worked on the film transport mechanism, Van Urk designed the electromagnetic drive for the cutter, W. Six, an electrical engineer who had joined the lab in 1927, developed a special amplifier, A. Cramwinckel, also an electrical engineer, and Verheijen developed stylus material, Westmijze⁴⁰ experimented with a system in which the width of the track rather than its transparency could be used to reproduce the sound, and C.J. Dippel and his group worked on the development of new film material.⁴¹ According to Blanken some pressure from the company directorate had been necessary to make this come about, because in 1933 Miller had complained about the slow pace at which the Nat.Lab. developments were taking place.⁴² In 1936 a high quality functioning system was presented in Paris and it was the BBC in England that was the first to order Philips-Miller equipment. Some other broadcasting companies followed suit, but soon there was a concern about the low profitability of the product.⁴³ After WWII it soon became evident that magnetic recording systems would beat the Philips-Miller system onto the market. According to Garratt, a lot of knowledge that had been gained from the Philips-Miller research could later be directly be applied to the work on magnetic recording.⁴⁴

One development in sound techniques where the Philips-Miller system seemed to be promising was in stereophony. With this technique left and right channel signals had to be recorded, using the same medium, which was easier with the Philips-Miller system than with gramophones. K. de Boer, a physical engineer, began his studies into stereophonic sound in the Nat.Lab in 1937.⁴⁵ Both the options of phase lag and intensity differences were tested using a dummy bought from a fashion shop. Several demon-



Principle of sound recording bij the Philips-Miller system.

Figure 5. The Philips-Miller system (from *Philips Technical Review* Vol. 6, p. 80).

On a strip of celluloid film (C), a gelatine layer (G) and a thin non-transparent covering layer (D) are deposited. A wedge-shaped cutter (S) moves up and down according to the sound signal and cuts a track. For playing the music the track is read optically, in the same way as is done with photographically recorded sound film.

strations were given, and people recognised the spatial effect of stereophonic sound. Yet, it would not be until 1958, when Philips produced its first stereophonic record player, that this technique would be exploited commercially. It is just one example of an area in which the Nat.Lab. was involved at a very early stage, but where there was a great time lag between ‘invention’ and ‘innovation’.

From Radio to Radiotelephony, Facsimile and Television

Radio waves not only were used for broadcasting, but also for carrying telephone signals. Radio telephony thus became an alternative to cable telephony. This was particularly interesting when with short wavelengths a large bandwidth appeared to be available for the telephone signals. But telephone cables were still needed to transport the signal to the houses, so the work on cables remained relevant. The Nat.Lab. worked not only on radio telephony, but also on cable telephony. Most of the work carried out was done on the attenuation and distortion of telephone signals in cables. In 1899 Pupin had invented a coil that improved the quality of signal transmission in the cables. In the Nat.Lab. W. Six, an electrical engineer, led a group that was responsible for working on the improvement of the magnetic core material for these Pupin coils. Commercially this was a very

successful activity, and it proved to be a stimulus for enhancing the work on telephony.⁴⁶ For still higher frequencies amplifiers needed to be placed at certain distances from each other. Tellegen's Pentode and Posthumus's use of negative feedback were employed in the development of those amplifier circuits. This development was carried out in co-operation with the Dutch Telecom company (PTT). The co-operation came about as a result of a recent change in the PTT's originally very negative attitude towards Philips.⁴⁷

In 1942 H. Rinia, an engineer who later became one of Holst's successors, was asked to start working on facsimile. Facsimile also was an option for telecommunications. It was as old as 1850, although the machine produced by Frederick Bakewell in that time was based on reading metal letters from a surface by means of a stylus and therefore far removed from what later on was meant by facsimile. Image transmission using the photoelectric properties of selenium was realised for the first time in the mid-1870s by Carey in Boston, but it was not a commercial success. In Germany Arthur Korn was able to produce a commercially feasible machine in 1902; the emerging popularity of photography in those days probably was a cause of its success.⁴⁸ In 1924 RCA in the USA sent a picture by facsimile across the Atlantic and back, and in 1926 a commercial facsimile service was started. In 1928 Holst had advised against the company's involvement in this area, because it was not a consumer product, like light bulbs and radio sets.⁴⁹ But later he expected that it would replace mail correspondence, because of the slowness of the postal system. The idea was that documents would be scanned, the information transformed into an electrical signal, and this signal then transmitted just like radio signals. At the receiver end the information would be written onto a film. Like stereophony, the idea was realised technically,⁵⁰ but the exploitation of the 'fax' would not come about until much later. The main obstacle for general acceptance probably was a lack of standards. A breakthrough came when in 1968 the United Nation's International Telecommunications Union defined an international standard. But by then, the Nat.Lab. had already long abandoned the idea.

Once Philips had become involved in transmitting audible information by means of wave signals, it was technically a logical step to move into television and to transmit audible and visual information. But commercially it was not such a logical step. In the late 1920s the company's directorate debated whether or not Philips should become involved in television. Later we shall see the role that Holst fulfilled in this debate. The outcome for the Nat.Lab. was that some television research activities were included in the research programme. A Nat.Lab demonstration of television was given on Saturday, December 22, 1928, by Druyvesteyn, who for that pur-

pose used the existing Nipkov concept. According to Blanken, the discussion became for Holst a turning point in his ideas about the Nat.Lab.'s mission. From then on, he would get the Nat.Lab. involved in new product fields to respond to Loupart's and Gaarenstroom's demands for a more innovative attitude in terms of product diversification.⁵¹ In that connection Blanken also referred to Holst's inaugural lecture at the University of Leyden, where he had been appointed professor to a named chair in 1930. Yet in later years, Holst still rejected new fields, reasoning that the links with the company's existing activities were too tenuous. Both before and after the television debate, Holst's idea about the goal of the Nat.Lab. seems to have focused on the notion that the Nat.Lab. should follow the company's ambitions with respect to the product range and provide new or improved technological directions in order to realise those ambitions. Even in the case of the hot air engine which was an exceptional research topic in the lab (see further on in this section), Holst was primarily concerned with radio when he decided to take that topic to the lab.

The Role of Materials Research

We have seen how the research programme originally focused on the development of devices, namely light bulbs and tubes. When radio came, the research programme moved towards what today would be called research concerning the whole radio 'system'.⁵² Next to device and system research, a third category of research can be distinguished, namely materials research. This type of research dealt mainly with those materials that were already used in system elements devices in the company's product range or which could constitute possible alternatives for such materials. So, as the device and system research programme diversified in the way described earlier on, so also did the range of materials to be investigated. Knowledge of those materials could sometimes lead to new applications. We will now go through the whole research diversification step by step again, but from the point of view of materials research.

At first, the materials research was closely related to light bulbs: various metals were examined as possible alternatives to tungsten. Two chemists, A.E. van Arkel and J.H. de Boer, had joined the lab, in 1921 and 1923, to conduct this sort of materials research. Of course they required analytical methods. The physicist H.C. Burger had already started using X-ray diffraction as an analytical method, and W.G. Burgers, a chemist, took over the X-ray crystallography side of the work in 1927.⁵³ In 1933 electron diffraction was added as a means for analysing metals. Several phenomena were studied. It was particularly the recrystallisation of metals that was of interest because what caused good or bad grain structures to come out of this process was not well understood. Van Arkel used X-ray analyses to

study this phenomenon in aluminium. For the production of tungsten wires a new process (deposition from the gas phase) that yielded wires with single crystals was developed in the Nat.Lab. These wires had better mechanical properties than polycrystalline wires.

Another material that was of interest, from the original light bulb perspective, was glass. During WWI Philips had opened a glass factory of its own. As there was already sufficient knowledge on the processing and properties of glass, the Nat.Lab did not do much research on this. Perhaps a second reason was that the analytical methods of X-ray and electron diffraction were not appropriate for glass, because glass is an amorphous and not a crystalline material. It was only after J.M. Stevels returned to the lab in 1940 that glass became a serious topic for research in the Nat.Lab.⁵⁴

The gas discharge lamps took the Nat.Lab. into other materials areas, namely luminescent materials (such as phosphor). When De Groot started his work on these types of materials, the solid state theory of energy gaps had not yet been developed, and so his work was mainly empirical. After 1930, the solid state theory spread throughout the scientific world. This inspired Nat.Lab. scientists to review previous empirical data in the light of this new theory. Thus, De Boer, enthused by an article on lattice defects by Schottky shown to him by E.J.W. Verwey, a chemist who had started his work in the lab in 1934 and who had later become one of Holst's successors, developed a model for explaining certain phenomena with KCl. When this was heated in potassium vapour, it went violet and demonstrated photoconductivity, and at higher temperatures it became semiconductive.⁵⁵ When the same material was hit by X-rays, the emerging violet colour disappeared at higher temperatures. Such theoretical work was complemented by practical work carried out by the chemist F.A. Kröger, who was successful in producing several new compounds that showed fluorescent behaviour. The phosphors were originally used for gas discharge lamps, but later also for various types of screens (X-ray screens, television and radar screens).

The electron emission induced by heating (thermionic emission) that took place in the bulbs led to a new research area for cathodes in various sorts of tubes. This field was also supported by materials research. To that end hafnium and zirconium were studied. Hafnium was still a rather new material at that time, and De Boer had to obtain it from ore, for which a fractional recrystallisation process of at least twenty stages was needed. Unfortunately, hafnium proved to be unsuitable as an emissive material for cathodes. In general, having a better understanding of the processes did not produce many practical results. The most important invention in the field of thermionic emission was that of the L-cathode, named after

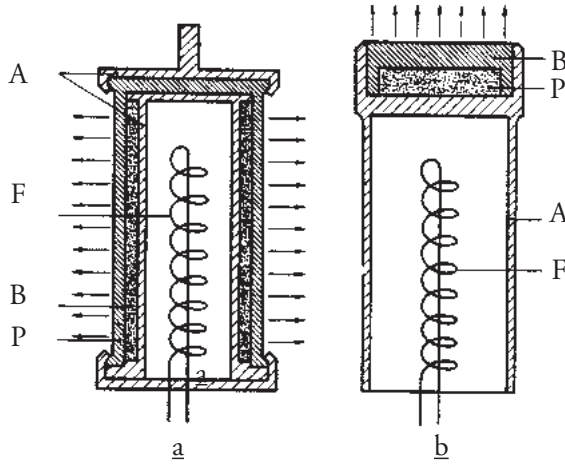


Figure 6. The L-cathode (from *Philips Technical Review* Vol. II, p. 342).

Two basic forms are shown in cross-section: (a) with a cylindrical emitting surface and (b) with a flat emitting surface. Both types have two chambers, one hollow and one filled with a tablet of barium-strontium carbonate (P). The second chamber is surrounded by a wall of porous tungsten (B), and both chambers are surrounded by a wall of molybdenum (A). Electrons are emitted from the barium-strontium carbonate layer when the filament (F) provides heat.

H.J. Lemmens,⁵⁶ a technician who had invented this cathode.

A second type of electron emission was photoemission. With photoemission the electrons are emitted from light that hits the cathode. This phenomenon was used to detect light intensity in photocells. In 1934 M.C. Teves and F. Coetier used photoemission to develop what came to be known as the image converter or image intensifier.⁵⁷ With this device, light hit a photocathode, causing electrons to be emitted. The electrons would be pulled towards an anode that was covered with a fluorescent layer, and the image would then be reproduced on the fluorescent layer. The advantage of this device was that the wavelength of the light produced by the fluorescent layer could be different from the wavelength of the original image. An infrared image thus could be converted into an image of visible light. This invention had possible military applications (e.g. for seeing in the dark), which was why further research into it was stopped at the outbreak of WWII. After the war, the same device was used for X-ray imaging by Teves and T. Tol. The new screen produced an image that was about a hundred times brighter than that of normal X-ray fluorescent screens. Consequently, the X-ray doses given to the patient could be reduced.

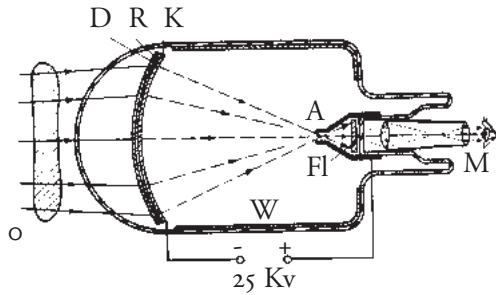


Figure 7. The image intensifier (from *Philips Technical Review* Vol. 14, p. 34).

X-rays pass through an object O and hit the fluoroscopic screen R that is supported by D. The X-rays cause the photocathode K to emit electrons, which move to the perforated anode A due to an electrical field, and then hit the small viewing screen Fl. The image is viewed through a microscope M. Using the photocathode and the accelerating electric field increases the intensity of the image brightness.

A third type of electron emission that had to be studied in the Nat.Lab. was secondary electron emission. As we saw from the description of the Pentode, this phenomenon was to be avoided in normal radio tubes, because it distorted the signal. But the same phenomenon could be used for amplifying purposes. H. Bruining, who had studied mathematics and physics and joined the lab in 1933 searched for suitable emissive materials with that idea in mind.⁵⁸ He found that magnesium oxides in particular were usable. Later, Teves and the electrical engineer J.L.H. Jonker developed photomultipliers that were used for amplifying television signals while the Nipkov technique was still being used. The photomultipliers were used to replace the photocells because of their faster response to changes in light intensity.

All the materials research described so far has been related to light bulbs and tubes. The two industrial fields in which Philips then became involved were those of radio and telephony. Here, too, materials research accompanied device and system research. In the cases of radio and telephony, it was mainly magnetic materials that were investigated. Such materials were relevant both to permanent magnets and to cores in electromagnets. In the case of radio, these magnets were used in loudspeakers. With telephony they were needed for the (Pupin) coils in cables. The permanent magnets used 'hard' magnetic material, and the electromagnet cores used 'soft' magnetic material. The difference between the two lies in the fact that hard magnetic materials retain magnetic polarisation induced by an external magnetic field to a greater degree than soft magnetic mate-

rials. In the Nat.Lab. G.J. Sizoo, B. van der Pol, A.T. van Urk and W. Elenbaas⁵⁹ had done some early work on magnetic materials. The research into magnetic materials really took off in 1932, when L. Snoek took over Elenbaas's work in this field. Van Urk and Elenbaas had made some valuable contributions to the design of permanent magnets and to the explanation of the role of the geometry of the magnet in magnetic flux. The approach adopted by the lab for doing magnetic materials research was similar to that for studying gas discharges. Practical design and problem-solving work were combined with experimental and theoretical work. The purpose was to gain more knowledge about the phenomena that formed the basis to the functioning of the products. Six worked on practical problems with Pupin coils for telephone cables, while Snoek and Burgers tried to gain a better understanding of magnetic materials and the recrystallisation of such materials. At first, most work focused on Permalloy, an alloy composed of iron and nickel. This material was used for Pupin coils. For magnets in loudspeakers it was originally cobalt steel that was used. In 1931 a better alloy composed of iron, nickel and aluminium became available. In the Proeffabriek (pilot factory) the chemical engineer H.J. Meerkerk van Embden had developed a process for magnetising this material, but with little understanding of what it exactly involved. Snoek and Burgers investigated the process, and in 1935 they published a paper on it. The results showed that adding cobalt and titanium would improve the material. The new alloy was called Ticonal. Snoek's work dealt particularly with the processes required for preparing the material. His research was complemented by Verwey's work concerned with understanding the phenomenon of magnetism in materials. Work on ferrites (not a metallic, but a ceramic material) received new impetus in 1940 when a piece of Japanese material proved that it was possible to make ferrite cores with low losses. This prompted Snoek to continue his investigations after a period of despondence. From then on, the work flourished again, leading to the Ferroxcube material that after 1950 was to be applied widely in coil cores for various products.

Unexpected Side Paths: Pharmaceuticals and Hot Air Engines

So far, each step in the diversification of the research programme seemed quite logical in relation to the company's product range diversification. The outcome was a very broad research programme, but there was always a more or less direct relation with lamps, tubes or radio. There were, however, two remarkable exceptions to this. Although the origin of these research fields was the relationship with the company's products, these parts of the research programme soon became isolated islands. The two research fields in question were pharmaceuticals and hot air engines.

Once Philips' role in the medical sector had been established with X-ray tubes, a further extension was considered feasible by W.J. Waterman, head of the company's 'Hollandse Afdeling' ('Dutch Department'). The Koninklijke Fabriek van Cacao en Chocolate C.J. van Houten & Zoon (Royal Factory of Cocoa and Chocolate C.J. van Houten & Son) had expressed an interest in another Nat.Lab. invention (made by a certain Dr. Reerink), namely that of producing Vitamin D by radiating ergosterine with ultraviolet light from a low-pressure mercury sunlamp. This was all to result in the starting up of a new company, N.V. Pharmaceutische Producten Mij. Philips-Van Houten (Pharmaceutical Products Philips-Van Houten, Inc.). In this case Nat.Lab. research output had led to the generation of new business for the company. The related research field, though, was quite removed from mainline electronics activities, and the business did not turn out to be successful.⁶⁰

The same could be said for the Stirling hot air engine research. This research was initiated because of the need for a small energy source for radios in developing countries. Besides its compactness the Stirling engine was flexible in terms of energy input, and also it did not produce much noise. The Nat.Lab. took the lead in developing this source of mechanical energy for the company. But in terms of disciplines and the relationship with mainstream (electronics) research, this was quite an odd research field for the Nat.Lab. Here, too, no commercial successes were ever achieved.⁶¹

In this section we saw how in the Philips company's growth and diversification process in the period between the Wars, the Nat.Lab. played a facilitating role. The laboratory was expected to determine the technological directions in which the company should go in order to extend its product range. In the company's 1926 Annual Report it was stated that 'new important inventions are continuously being made in our laboratories, so that in the coming year we can introduce new Radio field products onto the market'.⁶² According to Blanken, both the Commercial Department and the Nat.Lab. afterwards claimed to have induced Anton Philips' decision to turn Philips into a set-making company. At the same time Blanken emphasised the entrepreneurial attitude of Anton himself. In any case, the Nat.Lab. was involved from an early stage in the development of the company's product portfolio.

To summarise, there was a strong growth in research that was related to the development of new products. Part of that research aimed at gaining a better understanding of phenomena on which the functioning of new products could be based. Compared with the early years, the task of trouble-shooting and quality control had relatively diminished.

3.3 *Increasing the Laboratory's Means*

In the previous section we saw how the ambition of the Nat.Lab. – to conduct research to enable the company to realise its product diversification – was to lead to a substantial extension in the range of research topics. This extension was made possible because of the growth of the lab population and the facilities. Holst was given the opportunity to appoint new scientists, and new buildings and equipment were provided for the lab.

In 1919 Holst and Oosterhuis were again the only two scientists at the laboratory (some others had worked at the Nat.Lab., but only for a short time). In a memorandum written in January 1921, Holst mentioned a third staff member, who was exclusively concerned with manufacturing.⁶³ Maybe this was P.G. Cath, whose activities are not further mentioned elsewhere. Apart from the scientists, there were eight assistants in the lab. Holst proposed to the Philips directorate that the staff be extended to eleven or twelve doctors and qualified engineers and fourteen assistants. In 1922 the lab staff comprised twelve scientific graduates, and a total of 33 people worked there.⁶⁴ From a drawing produced in 1916 we know that the lab did not occupy many rooms: there were five rooms for the scientists and an office for Dr. Weber, there was a mechanical workshop, a battery room, a small library, a dark room, a pumping room, and two rooms for photometry.⁶⁵ It is evident that even though small extensions had been made, the situation was insufficient to serve the needs of the number of people working in the lab. Besides that, Holst had expressed the need for a vibration-free room for experiments, which was not available in the old building. That building was close to a factory.⁶⁶ In January 1921 a report was issued by the Nat.Lab. management (Holst and Oosterhuis) in which new lab facilities were proposed. In 1922 plans were made to build a new laboratory in a vibration-free area.⁶⁷ The Strijp district in Eindhoven was selected as the location for the new laboratory. In the years 1923 and 1924 the research activities were moved from floor IV of the old building to the new laboratory in Strijp. The map of the laboratory⁶⁸ shows that by then there were several research areas (there was a chemical laboratory, a transmitter room, and a room for photometry).

After the lab activities had been moved to the Strijp facilities in 1923/1924, the lab population started to grow considerably. In 1924 the premises had a floor surface of 2,700 m². Already in 1926, 5,700 m² had been added to house all the people and equipment. A drawing of the 1926 extensions⁶⁹ shows that at that time there were lab sections for X-ray, acoustics, materials testing, radio, and a chemical department.

In 1930 another extension was added, thus creating a total area of 17,700 m². Until that extension had been built, the buildings had been single-

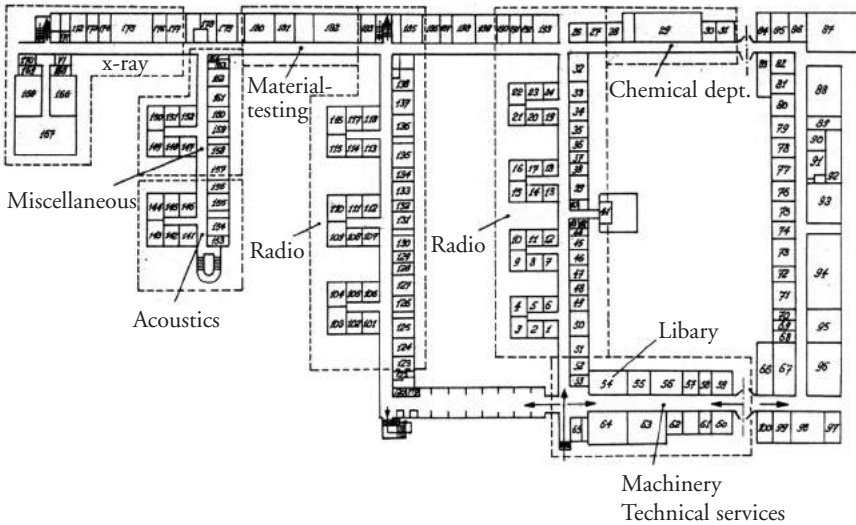
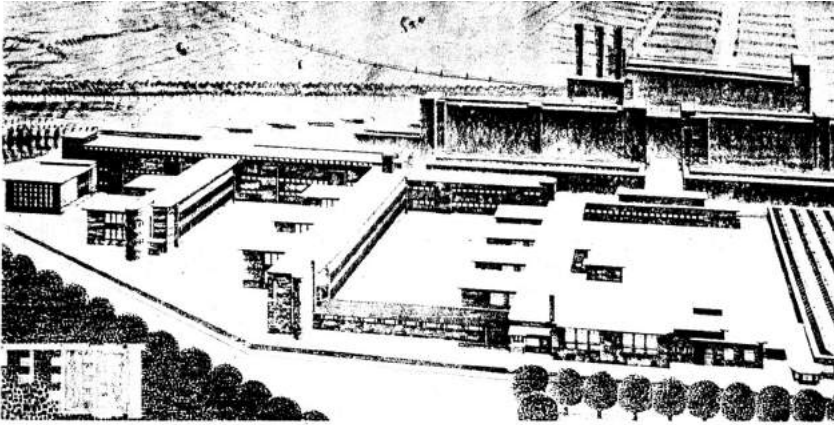


Figure 8. The extended Nat.Lab. in 1926 (from Garratt 1976 Vol. 1, p. 127).

floored. In 1930 a second floor was built on a number of corridors in the original building. A lecture room for several hundred people was included in the new facilities. In 1935 the workshops moved into the pilot factory building, which again made extra space available for working in. This meant that the many small workshops that had been set up over the course of time (at a certain time there had been 16 of them!) had to be merged into one central workshop. Jansen-Gratton, whom F.J. (Frits, the only son of Anton) Philips had made responsible for this operation, later intimated

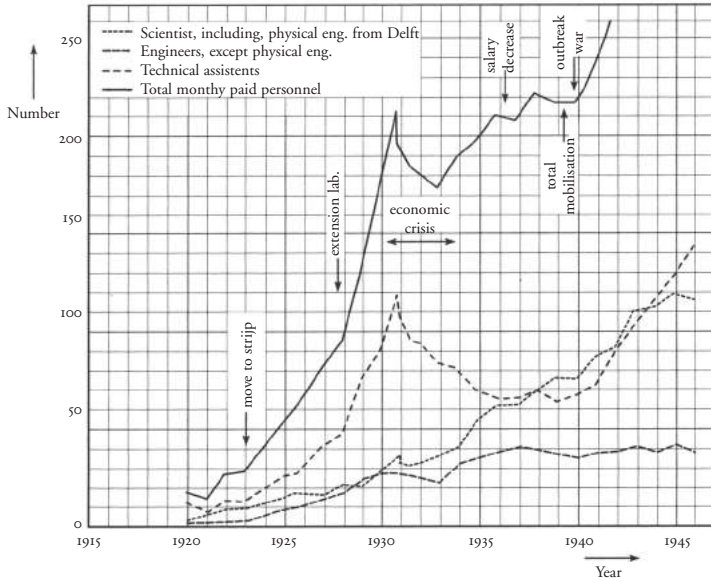


Figure 9. The Nat.Lab. staff in the 1920-1946 period (from Garratt 1976 Vol. 1, p. 221).

that he had received Holst's support for this and that he had very carefully communicated with several scientists in order to convince them that there would be advantages attached to their having 'their' own workshop integrated into the new, central workshop in the pilot factory building.⁷⁰ This operation is illustrative for what Holst, who was officially appointed Director of the lab in 1926 by Anton Philips,⁷¹ was expected to accomplish during the years when the lab grew so enormously: a group of independent researchers had to be transformed into a lab population working on one collective research programme, which had to be managed and guided. In the next section we will focus on the sort of tensions that accompanied the formation of an organisation in the laboratory.

The increase in personnel numbers was interrupted by the economic crisis of the early 1930s. A diagram of the number of scientists and assistants in the 1920-1945 period shows that the number dropped from about 210 in 1930 to about 170 in 1933. In fact, this was quite a modest reduction compared with what occurred in the rest of the company, where the total staff numbers were reduced from 22,672 employees in November 1929 to 9,534 employees in May 1932. According to Verff, several of the dismissed Nat.Lab. assistants even found a place elsewhere in the company.⁷² From then on growth resumed but was not as vigorous as before. In 1936 there

was another stagnation (only for that year), and in 1939/1940 the war mobilisation prevented growth. In 1941 the lab population started to grow again, but the reason for that was not the lab programme or company expansion (see Intermezzo I). A breakdown of the total growth pattern into scientists and assistants shows that it was the number of assistants which was chiefly responsible for the changes in this growth pattern. Over the years the number of scientists did not change much.

The growth in the level of means at the Nat.Lab. indicates that the company's directorate had confidence in the role of the Nat.Lab. The Nat.Lab. was given a fair share in the growth of the company.

3.4 The Culture and Structures for the Diversifying Research Programme

Within the ever-growing laboratory, various tensions became evident that are to be expected in any organisation of professionals. As the lab population grew, a formal management and organisation structure became necessary. In a professional organisation this kind of thing usually causes tension between the self-conscious professionals who want freedom based on expertise and the need to ensure that all these professionals will together yield satisfactory results. There was a similar tension surrounding the acknowledgement of leaders on the part of other employees. Perhaps scientists are inclined to base such acknowledgement on a leader's proven scientific expertise, but some leaders perhaps needed to be accepted on the basis of their managerial reputations. Some people, Holst in particular, were forced to reconsider their roles. For them, tension grew between their original work (doing research, which was probably where their heart was) and fulfilling the new task of doing managerial work. In the research work there was always tension between an academic orientation and an industrial orientation. Altogether, this means that when studying the organisational structures and the culture of the lab, we can expect to see various sorts of tension.

Creating an Academic Culture for an Industrial Research Lab

When selecting the research topics for the Nat.Lab., Holst was very keen to ensure that a close relationship developed between the Nat.Lab.'s research programme and the company's product portfolio. This shows Holst's industrial awareness. In order to make a valuable contribution to the company, Holst wanted to have a laboratory that was filled with researchers who had excellent scientific reputations. Within the lab he created an academic culture, in which scientists felt they were free to do high-level scientific research. His management ideas remind us of Robert Mer-

ton's later (1942)⁷³ description of institutional imperatives for scientific research, namely universalism, communism, disinterestedness and organised scepticism. The universalist and communist norms can be recognised in the fact that Holst stimulated publications in academic journals as an independent and open forum for judgement of the Nat.Lab.'s research output; disinterestedness was there to the extent that Holst tried to avoid direct involvement of factories with the Nat.Lab.'s research plans, but on the other hand the company's interests were most certainly for Holst a factor in his own decisions about the lab's research programme;⁷⁴ organised scepticism is apparent in that Holst did not put pressure on his people to come up with 'quick and dirty' research work for the sake of a hasty transfer to the factories.

Holst took various steps to achieve an academic culture in his laboratory. He organised colloquia in which the world's most famous physicists were able to share their views with Nat.Lab. scientists. He stimulated his scientists to publish in academic journals. In so doing, Holst hoped that the Nat.Lab. would get such a good reputation as a serious scientific research institute that good scientific university graduates would see working in the Nat.Lab. as a serious career option. There were certainly reasons for him to be concerned about the situation. It was not easy to find sufficient scientists in the Netherlands in the early years of the Nat.Lab. In the 1914-1918 period, because of WWI, young scientists were being called up for military service. In addition, there was the simple fact that the country was much smaller than the USA, where labs such as GE and Bell could recruit their scientists from a far larger population.

So to make it attractive for young scientists to work at the Nat.Lab., Holst had to search for ways of establishing a scientific reputation for the lab. Apart from having specific know-how in the field of spectra, the scientific status of G. Hertz can be seen as an important reason for Holst's wanting to attract him to the Nat.Lab. in 1920. Another way to raise the scientific status of the lab was by inviting famous scientists to lecture there. Before 1923 it had been particularly Paul Ehrenfest who had frequently visited the lab to give lectures. He came from St. Petersburg, but at that time he was Professor of Theoretical Physics at the University of Leyden. In the years from 1920 to 1923 he visited the Nat.Lab. quite often. His name often crops up on the colloquium lecturers list: 29 (!) times for lectures between November 1920 and February 1923⁷⁵ (on average he lectured once every month). The names of Ehrenfest and Hertz illustrate what sort of know-how Holst was interested in obtaining: both these scientists were involved in research into fundamental phenomena at the atomic level. At the beginning of the 20th century, this field of physics was very much in motion, when quantum physics was emerging to replace 'classical' physics,

which had been accepted as unquestioned for centuries. Holst wanted the Nat.Lab. to be in touch with these dramatic changes in the field of physics, even though their relationship to Nat.Lab. research was only indirect. After the 1923/1924 move the series of colloquia was continued, and many well-known scientists participated.⁷⁶ In November 1923 Lise Meitner spoke about the physics of the atomic nucleus. In December 1923 Albert Einstein delivered a presentation entitled: 'Does the contemporary theoretical physics, which is based on partial differential equations, offer possibilities for solving the riddles of quanta?'. In 1924 H.A. Kramers visited the lab twice to speak about ionisation caused by collisions and radiation. In the same year Sommerfeld spoke about the intensity of spectral lines, and Debye spoke about electrolytic conduction. In 1925 Born came from Göttingen to speak about the electrical interpretation of chemical affinity and cohesion forces in solid states. The list continues with names like Franck, Appleton (on radio transmission), Pauli, Goudsmit, Brillouin, Stern, Geiger, Lawrence, Watson Watt, Uhlenbeck, Hahn, Langmuir, Aston and Joliot. There were about twelve colloquia per year during the 1924-1942 period, except in the years of economic crisis (1930-1934), when there were considerably fewer (in 1932 it was only Ehrenfest who came twice to speak) and when WWII broke out in 1939, there were no longer foreign presenters. The list of names and subject titles shows that the colloquia provided the opportunity for Nat.Lab. scientists to stay informed about all the latest developments in their disciplines, directly from the people who were leaders in the respective field at that time.

The international orientation of the Nat.Lab. colloquia was not unique. In general, Dutch physicists were in good contact with their – often famous – colleagues in other countries.⁷⁷ In Leyden Ehrenfest also organised colloquia with international guest speakers, which were not unlike those given at the Nat.Lab., and perhaps the visits made by world-famous colleagues were partly due to his intervention. In the period between WWI and WWII, the discipline of physics flourished in the Netherlands in terms of participation. This was even the case during the 'Second Golden Age' of Dutch science (1870-1914) with its Nobel Prize winners and names like J.D. van der Waals, H. Kamerlingh Onnes, H.A. Lorentz, P. Zeeman, and J.H. van 't Hoff. In the 1910-1930 period the number of science professors grew from 39 to 79 (including professors for named chairs), and in the years 1900-1930 the number of students rose from 439 to 1784.⁷⁸ It was not difficult for the students to find positions at universities, and the scientific status of the Nat.Lab. was important for attracting their attention to the industrial laboratory, which was seen as a new, interesting option for a serious scientific career. At this time, serious contributions to the discipline as a whole were being made in the Netherlands. In particular, Leyden and Utrecht were centres where high-level

physics research was being done. In Leyden W.H. Keesom and W.J. de Haas continued the work of Kamerlingh Onnes (who retired in 1924) on low-temperature physics, and from 1934 onwards H.A. Kramers complemented their experimental work in a sound theoretical way. In Utrecht L.S. Ornstein established a respectable tradition in spectroscopy and intensity measurements. The Nat.Lab. took in graduates from those institutes and thus profited from the knowledge that was developed there. The theoretical work done in Leyden was useful for the understanding of electron and ion behaviour, which formed the basis for several products that were investigated at the Nat.Lab. The spectroscopy work carried out in Utrecht was also relevant to the Nat.Lab. research fields, both for materials research (H.C. Burger had studied there, and in the Nat.Lab. he greatly stimulated the use of spectroscopy techniques) and for work on lamps and lighting. Such relationships were less direct when it came to the discipline of chemistry. In the Netherlands Van 't Hoff had initiated a tradition in physical chemistry that was taken over by E.J. Cohen and H.R. Kruyt in Utrecht.⁷⁹ It was Kruyt in particular who stimulated contact between his institute and industry (he was involved in the foundation of the Dutch organisation for Applied Scientific Research, TNO), but that contact was mainly with the chemical industry in the Netherlands. Kruyt later became particularly interested in colloid chemistry, but in the Nat.Lab. this was of limited use (at the Nat.Lab. J.H. de Boer tried to do some work on that, but found it difficult to relate it to other work in the laboratory; later, C.J. Dippel would take up the subject again).

There was also a continual flow of persons taking their knowledge in the opposite direction (from the Nat.Lab. to the universities): sometimes a Nat.Lab. scientist would leave the lab to become a professor at a university. In the period leading up to 1946, this was the case for G. Hertz (1925), H.B. Dorgelo (1929), C. Zwikker (1929), G.J. Sizoo (1930), A.E. van Arkel (1934), W.G. Burgers (1939), M.J. Druyvestein (1945), J.Th.G. Overbeek (1946) and N.G. de Bruin (1946).⁸⁰ All left Philips to become full-time professors. The concept of a part-time professorship in combination with a position in the Nat.Lab. was exceptional before WWII. Only Holst and Van der Pol were offered a position as named chair professors in Delft and Leyden. Holst made an important personal contribution to the Dutch academic world when he and Ornstein, among others, established the Dutch Physics Association (Nederlandse Natuurkundige Vereniging) in 1921. He was also instrumental in establishing the Physics Department at the first Dutch Polytechnic in Delft, in 1929. Zwikker and Dorgelo (mentioned above) were among the first professors there.⁸¹

Another way to disseminate the newly gained Nat.Lab., scientific knowledge was by publishing. Apart from the fact that famous scientists visited

the lab, publishing in scientific journals, preferably leading ones, was an important way for the Nat.Lab. to display a certain degree of scientific status. An administrative system designed to manage the stream of publications was set up by M. Drijver, who had come from one of the factories.

The total stream of publications grew as the number of scientists working in the lab grew. Therefore, Registers of Publications started being published. In those registers we see one entry for 1914, but from then on, the number of publications produced per year increases steadily until 1937 (236 entries), declines rapidly after 1940 and recovers again in 1946, the year after the end of WWII. The numbers show that publishing the outcomes of scientific research was something that was taken seriously in the Nat.Lab. culture.

But Holst was not only keen to stimulate the scientists to keep up with all the latest developments in science and to contribute to science by publishing. He also wanted to stimulate a constant awareness of the need to contribute to the company's industrial position. The outcomes of scientific research – for which he had built up a research culture – should be made available to the company. There were at least three ways in which this was stimulated. Firstly, the scientists were expected to submit ideas for patents; secondly, Holst created a special journal for passing on research output to industry; and thirdly, contact was maintained with other parts of the company. The first and second ways will be discussed here below. The third way will be dealt with later on in this chapter.

Scientists were pushed not only to publish but also to come up with possibilities for patents. For these purposes so-called white cards were created. Scientists could fill in these cards, which contained the data that were needed by the Patent Department whenever a patent request was submitted. Holst would then decide whether or not a 'white card' should be sent to the Patent Department. Only if that was not the case were the scientists free to publish their work. The Patent Department had been officially set up in 1921, and its department head was E. Hijmans.⁸² By 1922 the department had a staff of seven people.⁸³ This department transferred the information on the 'white cards' to the 'yellow cards' and did research into the novelty and patentability of the invention described on the 'white card'. Finally, a decision would be taken on whether or not to submit the invention to the national Patents Office in order to apply for a patent. If this was decided against, there was still the opportunity for the scientist to prepare a publication about the work. If it was decided that a patent should be applied for, then the scientist had to postpone his publication until the national Patents Office had registered the application. In 1929 each scientist submitted 3.1 white cards on average and in 1930, the year of the economic crisis, the average was 2.4 cards. During the 1931-1940 period each scientist submitted between 1 and 1.5 white cards per year on aver-

age. After that, there was only a temporary recovery of the peak rate for 1933 (of 2.5 white cards per year), while otherwise the number of white cards per scientist per year remained less than 1.5.⁸⁴ The number of inventions listed on white cards that made their way to the national Patents Office amounted to around 50% of the total number of white cards.

In addition to all the external journals and conferences and internal reports and accounts, the Nat.Lab. scientists were given a special opportunity to publish material when, in 1936, the *Philips Technisch Tijdschrift* (*Philips Technical Review*) started being published. In the Introduction to the first issue, Holst explained why the Nat.Lab. had decided to start publishing a journal of its own: 'The Philips Research Laboratories are continually receiving an ever-increasing number of enquiries and requests from many quarters for more detailed data and particulars of the extensive range of Philips products, and especially for information as to their specific characteristics and practical applications. A large proportion of these enquiries comes from the engineering world, and it is hoped by means of this periodical to establish contact with these circles.' Two things can be read from these words. Firstly, the periodical was not meant to be exclusively about Nat.Lab. research, although the colophon of the journal mentioned that it was 'edited by the research laboratory of the N.V. Philips Gloeilampenfabrieken, Eindhoven, Holland'. It became the journal's practice to publish articles written by authors from other parts of Philips in the *Philips Technical Review*. Most of the articles, though, originated from Nat.Lab. authors. Secondly, the journal's target group was not primarily the scientific world, but the industrial world and 'the whole engineering profession'. The articles therefore would present the subject matter 'as simply as possible'.⁸⁵ The journal could not be seen as an alternative to publishing in scientific journals which was probably why there was not a decrease in the number of other publications produced by the scientists. The first issue of the *Philips Technical Review* appeared in January 1936, and the normal frequency was to be twelve issues per year (it was only in 1942 that nine issues came out, and none were published in 1943-1945). The journal was published in four languages: Dutch, English, German and French. Holst, Oosterhuis and Verff formed the editorial board, which shows that the journal was seen as an important activity.

The researchers were thus stimulated to become Janus-faced. They were expected to look both in the direction of new scientific developments and in the direction of the company's industrial interests. This two-faced aspect was often to be realised by a combination of scientists. A.E. van Arkel and J.H. de Boer were chemists who wrote a book together on the chemical bond as an electrostatic phenomenon, but their individual activities reflected a dichotomy in interests. Van Arkel was more interested in pure chemistry and in 1934 moved to the University of Leyden to become

a professor in inorganic and physical chemistry. De Boer was more interested in the industrial aspects of his discipline, and when he left the lab, he had 152 Dutch patents under his name. Likewise, there were two groups working on radio technology, one led by Van der Pol that had a more theoretical (mathematical) character and the other led by Oosterhuis that was more practically oriented. Such a combination did not always provide a solution to the tension between an academic and an industrial approach. In the case of the ferrite research, the theoretical contributions made by Verwey and the practical contributions made by Snoek remained separate and were not really combined. The relationship between Bol and Elenbaas in relation to gas discharge lamps illustrates how the differences between more academic and more industrial approaches could even lead to personal tensions (see section 4.1). Of course, this was not a necessary consequence of the Janus-faced nature of the laboratory. Evidently, though, integrating academic and industrial orientations and making these the two main orientations in the culture of an industrial research laboratory was not always easy.

Structures for Managing the Growing Research Programme

In creating a Nat.Lab. culture, Holst had to strike a balance between an academic and an industrial atmosphere, as was illustrated in the previous section. Likewise, in creating an organisational structure for the Nat.Lab., he had to find a good balance between freedom for the researchers and a certain mechanism that would mean control over the research efforts as a whole.

In the early years this structure was informal, and because the number of employees was small, Holst could easily keep track of all their work. But as the lab population grew, Holst had to introduce certain more formal structures. He was thereby confronted by the fact that he was dealing with an organisation comprised of professionals. In such an organisation it is to be expected that people claim a lot of freedom because of their level of expertise. The Nat.Lab. population included a number of strong characters, for whom that rule certainly held. Two examples of such persons were A. Bouwers and B. van der Pol. In the case study on X-ray research (Chapter 5), we shall see that Bouwers was able to project himself sufficiently to have his own budget within the Nat.Lab. Surveys on paper sometimes created the impression that his group was in a position that singled it out from the rest of the lab. Undeniably Bouwers did a lot of important work for the lab: while he was there, his group published 83 papers and yielded 70 patents. Bouwers had also received several awards in the academic world.⁸⁶ Because of this proven expertise, Bouwers was able to more or less retain this independent position. The same can be seen with Van der Pol.

Most of his work was of a mathematical character and had to do with the propagation of radio wave signals. He published more than 200 papers on that subject. His work made him an expert and gave him worldwide fame. He, too, could claim freedom based on his proven expertise. Several people who knew both those men have mentioned that Holst and Van der Pol had quite different natures, which sometimes gave rise to tensions between the two.⁸⁷

Holst certainly recognised the merits of the strong characters in his lab and gave them opportunities to manifest themselves. In the pre-WWII period there was no formal hierarchy between the scientists (there was between scientists, assistants and technicians, as we will see later on). So a formal promotion of excellence where the scientists were concerned was not yet available to him as an instrument to do that. In defining the research programme, however, he did show his appreciation of the leading scientists. In a memorandum entitled 'Division work discussion groups for the year 1932', nine groups were differentiated, and a list of scientists arranged in alphabetical order for each group was given, except for the first name, which apparently referred to a leader.⁸⁸ Thus, we have Radio A (apparatus) and Radio B (lamps) under Oosterhuis's name given first, Radio C (experimental) with Van der Pol, Acoustics under Van Urk, Chemistry A (solid states) under Van Arkel, Chemistry B (photochemistry) under De Boer, X-ray under Bouwers, Gas discharges under De Groot, and finally a 'rest' group with Holst's own name at the top of the list. In a 1933 survey of the research programme recorded in one of Holst's notebooks, we find nine groups each of which corresponds to a certain field of research. In some cases Holst had not named the group according to its content, but after the name of a person leading it.⁸⁹ This confirms the idea that Holst saw some persons as the 'primes inter pares'.

Apart from the informal instrument for differentiating between employees mentioned above, Holst had the more formal instruments of 'vakgroepen' and salaries. The status of the employee was reflected by the 'vakgroep' (salary scale). A scientist started in scale 6 and could be promoted to scale 7. Assistants started in scale 3 or 4 and could be promoted to scale 5. Assistants had no academic degrees, but were high school graduates. Although there was a certain relationship between salaries and vakgroepen, being in a higher vakgroep did not necessarily mean that one received a higher salary. Neither an employee's vakgroep nor salary was made known to other employees. The scientists and assistants were paid monthly. There was a third group of employees, the technicians. Those were craftsmen who did workshop activities. They were paid weekly. There were more things that indicated differences in status between scientists and assistants on the one hand and technicians on the other hand. The way the working times were recorded was arranged as follows: for the

scientists and assistants a porter did this, but the technicians had to punch a time clock card. Another difference was: scientists worked 43 hours a week, assistants 43 or 45, and technicians did 48 hours a week. Even very subtle symbols were used: in the 'bath corridor', a nickname given to a corridor that looked like the bottom of a swimming pool,⁹⁰ door mats in front of certain doors indicated where the rooms of persons of a higher rank were situated. To give a final example: scientists and assistants wore white coats, and technicians wore yellow coats. Thus, a certain hierarchical structural for the lab was created and maintained.

When the lab grew, Holst needed some persons to assist him with his increasing managerial tasks. He created a separate chief of personnel position. In 1933 J.M. Verff was offered this position. Before that he had been Otten's secretary (Otten was one of the company's managing directors and was to become Anton's successor). According to Verff, Holst could no longer take care of the administration of 200 employees of his own, at the same time keeping in proper contact with the approximately 50 scientists. Verff was also asked to take charge of the contacts with the commercial department and the factories.⁹¹ This would allow Holst to spend time contacting researchers in order to keep track of the research that was going on. At first, he could maintain this contact by occasionally visiting individual researchers, but when the number grew, he had to look for more formal ways of maintaining that contact. For that purpose Holst set up a system of reporting procedures for the scientists. Soon after the move to Strijp, each scientist was supposed to record his work in two notebooks: a small one for the experimental data, and a larger one for the analysis of those data and for theoretical reflections on the results. These two books served as resources for the writing up of the scientists' publications. Results that for whatever reason were not thought to be suitable for external publication (in journals, books or conference proceedings) were published as internal reports and accounts. Such internal reports enabled Holst to keep track of the scientists' work. An administrative system designed to manage the stream of publications was set up for Holst by M. Drijver, who had come from one of the factories. In 1936 Holst also established a 'working discussions' procedure that enabled him to keep track of what happened within the various research groups. Scientists had to present their work to Holst and to colleagues during these sessions and they were thus stimulated to sustain a certain quantity and quality of work. In that year there were about sixteen such sessions per month. A one-page report was produced from each working discussion, which was registered as an internal publication. When the number of participants was ten or less – which was usual for all groups, except for the radio groups – the sessions would be held in Holst's room.⁹² This reflects his direct involvement in the working discussions. In fact, the procedure sketched above was an adaptation of an

existing habit for working discussions that were of a more open character. In a 1935 memorandum Drijver complained about the fact that so many people attended these working discussions (at least thirty, but sometimes even up to sixty and that the reports of these sessions had often resembled complete publications.⁹³ When the number of participants and the size of the reports was reduced in 1936, this must have made the working sessions much more suitable as an instrument for managing the lab programme as far as Holst was concerned.

Thus, we have seen that in this period a structure developed that had both informal and formal aspects, and that the emphasis shifted from informal to more formal structures. Holst saw this as necessary for achieving his goal of enabling the company to diversify its product portfolio. This was something that was not possible without some hierarchical control over the work of the individual researchers. In the first post-WWII period an important step in the formalisation of the hierarchy of the lab was taken when formal group leaders were appointed. Another extra layer emerged when Holst retired and was succeeded by three directors, one of which was later given a position in the company's Board of Management that was formed within the new company structure (see the *Intermezzo* on the years 1940-1946). This formalisation left untouched the freedom that researchers had to select their research topics. The formal structure of the lab thus provided instruments and mechanisms so that the management could control the procedures of communication (publishing and presenting) and payments, but it enabled the researchers to give free rein to their expertise.

3.5 *Influencing the Company*

In the previous sections we saw how the goals, culture and structure, as characteristics of a professional organisation, were influenced by external contacts in the scientific world but also within the Philips company. Now we will consider how the Nat.Lab. influenced, or sought to influence, the company. Both ways of looking at the Nat.Lab. contribute to our understanding of the role that the Nat.Lab. wanted to fulfil within the company (the main research question of our historical study). The goals, culture and structure of the Nat.Lab. inform us about the type of laboratory that Holst and others considered to be appropriate when it involved contributing to the Philips company. The way the Nat.Lab. went about trying to exert an influence on the company tells us about people's ideas and about how it was thought that the Nat.Lab. could make people's efforts fruitful to the company. We will focus on two ways of contributing to the company that we find were practised in this period: in the first place we will consider Holst's contributions to management discussions on the

company's product policy, and in second place we will consider the relationships which existed between the Nat.Lab. and the factories in order to gain an impression of the transfer of research output from the Nat.Lab. to production in the factories.

Holst in the Directorate and Orco Meetings

Philips directorate meetings were held from 1928 on.⁹⁴ Originally, it was only Anton and Gerard Philips who were in charge of the company. When Gerard retired in 1922, Anton was the only official director. Although he was not afraid to give his employees considerable responsibilities, he had great influence on the company's policy. This situation remained the same even when the company grew to 46 factories and 69 sales departments in 1935.⁹⁵ To co-ordinate all the various activities, staff departments were set up. There was a Commercial Department, a Financial Department, a Patent Department and a Secretariat, which operated as the company's chancellery. Departmental influence increased as the size of the company increased. From these departments future directors emerged, such as P.F.S. Otten, H.F. van Walsem, O.M.E. Loupart and F.J. Philips.⁹⁶ We find their names, and Holst's name, in the attendance lists kept of directors' meetings.

The minutes of these meetings show that Holst certainly did not stimulate active participation on the part of the company in any new areas. According to him, the Nat.Lab. had just about the capacity to support the further development of existing products. Nevertheless, he did recognise the need to extend the product range, but mainly in the direction of mass consumer products (e.g. electrical gramophone and sound amplifiers).⁹⁷ In 1929 Holst warned that sound film would probably not yield any profit for the company. On the other hand, he saw sound film as an opportunity for entering new related areas.⁹⁸ A month later, Holst expressed very pessimistic feelings about the feasibility of the radio broadcasting business in the Netherlands, given the divided situation in the country.⁹⁹ A year later Holst warned against extending the range of radio sets in the company's product portfolio. According to him standardisation was necessary (e.g. by designing a set of basic components that could be used in all types of sets). So we see an ambivalent attitude: on the one hand, Holst recognised possibilities for new business openings but on the other hand, he often felt the need, during the directorate meetings, to express his concerns about the feasibility of those same possibilities.

In 1932¹⁰⁰ a special series of meetings known as the Orco meetings was initiated for the purpose of discussing the company's development. Orco stood for *Orienterings Commissie* (Orientation Committee).¹⁰¹ Apart from discussing product policy, the Orco also had some authority to take

decisions. The purpose of the Orco was to co-ordinate activities undertaken by the various entities within the company. The company's directorate was represented in the Orco, as well as the Patent Department (Hijmans), and from 1933 on the Nat.Lab was also represented.¹⁰² In 1934 it was mentioned that in specific fields sub or 'article' Orcos could be set up.¹⁰³ These sub Orcos did not have the authority to make decisions but were required to prepare decisions to be taken by the Orco. By the beginning of WWII, the Orco had weekly meeting fixtures: it met every Monday afternoon at 2.30 pm.¹⁰⁴ By the end of WWII, a number of article Orcos had been set up.¹⁰⁵ There were Orcos for Lighting, Electron tubes, Apparatus for domestic use, Telecommunications, Devices, Medical equipment, Image and sound techniques, Industrial applications of electronics, Products for industrial applications, Measurement equipment, Metals, Chemicals, Pharmaceutical articles and food, Related factories (paper, glass). This list reveals a correspondence to the Product Divisions that were set up after WWII (see the first Intermezzo).

As in the meetings for directors, we frequently find that also in the Orco meetings Holst participated in discussions about future needs for the company. Again he expressed hesitation about the feasibility of various new issues. When in 1932 Loupart presented a list of products that could be sold in the USA to start up competition with GE, only a few of the listed products were approved by the meeting (X-ray tubes and equipment, and photo flash lamps), and this very small selection was at least in part based on Holst's advice.¹⁰⁶ In 1934 he expressed his doubts about the Miller system.¹⁰⁷

Holst also participated in more specific committees such as the Lighting committee, where various lamp applications, such as those intended for use in traffic, like the water cooled 'Bol' lamp, were discussed by Nat.Lab. and factory representatives, in preparation for the company's participation in the world exhibition in Paris in 1937, to name but one purpose.¹⁰⁸ In those meetings concrete agreements for courses of action to be taken were often made. In such committees Holst was able to play a more stimulating role because it was exploitation of existing products that was dealt with and not the exploration of new products areas.

There is one issue that needs to be described separately, and that is Holst's stance in the company's policy towards television as a possible new product field, because Holst's ideas on this were controversial. Holst was sceptical about the possibilities of exploiting television commercially in the Netherlands.¹⁰⁹ Indeed, until the mid-1930s his scepticism seemed to be well grounded because the quality of the images was still very poor at that time, while it was expected that television sets would be quite expensive. But the invention of the iconoscope as a fully electronic technique for

reading optical images represented a major step forward. This was recognized by his colleagues at Telefunken in Germany and EMI-Marconi in the USA, who immediately abandoned work on the mechanical scanning devices and started working on the iconoscope. Holst, however, at first underestimated its importance. During an Orco meeting in 1933, he expressed the opinion that this new device would be insufficient to cause a real breakthrough. A year later he underlined the large costs involved in building a network of television transmitters in order to further underpin his hesitations about the prospect of television as a commercial product. His opinion was supported by the outcomes of a study trip to RCA in the USA, made by Van der Pol and Van der Mark, two of his Nat.Lab. researchers. According to them, RCA did not expect a general introduction of public television within two or three years.¹¹⁰ Nevertheless, in 1935 a test transmitter was built in the Nat.Lab., and the technical feasibility of television was demonstrated to journalists. In the Apparatus factory a television group was established, which co-operated with the Nat.Lab.¹¹¹ Loupart, one of the company's directors, who was particularly concerned about the company's commercial affairs, had strongly argued in favour of such a test transmitter. After the demonstration Holst remained doubtful about the commercial feasibility of television. As an alternative he suggested putting all the company's efforts into developing a 'home cineac', a distribution system whereby people could receive films at home to watch at times that suited them. Holst's strategy was to closely watch what happened in the USA, but not to spend too much time and money on a research programme within the company as yet. This caused great concern within the Commercial Department, because they feared building up a growing backlog compared with other countries, such as the USA, the UK and Germany. In 1937 television activities within the Nat.Lab. increased. It is not clear if any extra impetus was provided from the company's television committee to suddenly give rise to this. It may also be that the replacement of mechanical television by electronic television was seen as sufficiently promising to justify increased efforts, although Holst at first did not have high expectations of the invention of the electronic scanning tube (see also section 6.2). Anyway, it is not probable that this increase would have happened without Holst's approval. Perhaps here the opinion expressed by Casimir and supported by Blanken¹¹² to the effect that Holst completely lacked any awareness of the potential of television as a commercial product should be reconsidered. It at least seems to conflict with the fact that Holst saw a potential for 'image telegraphy' in countries such as China and Japan – where there were no letter but picture types of character systems – which would, at the same time, enable television to be realised at little extra cost. This shows that Holst was not opposed to television as such,¹¹³ but that he just saw financial barriers in Europe.¹¹⁴ In 1938 television was accepted as being 'important' statuswise by the

Nat.Lab., not in the least at the emphatic request of the leader of the apparatus factory, P.R. Dijksterhuis.¹¹⁵

Although Holst's role in the case of television remains a dubious one, the role of the Nat.Lab. in the field of television was no different from other areas: the Nat.Lab.'s programme supported the sort of product diversification that the company wanted. Holst wanted the Nat.Lab. to have more the innovative role in the company of providing new technological ways of realising the existing product portfolio rather than leading it into entirely new product areas.

The Changing Relationship with the Factories

In Chapter 2 we considered the different types of work that can take place in an industrial research laboratory. Research can be focused on the understanding of phenomena, but there can also be development work and testing activities. We saw that in the early years all three types of work were carried out in the Nat.Lab. This was not surprising given the close relationship between the Nat.Lab. and the factory within a company that was still rather small in those years. In the period described in Chapter 3, the company diversified and grew, and this affected contacts between the Nat.Lab. and the factories. The changes in these contacts, and their consequences for the type of work being done in the Nat.Lab., will be the subject of this section.

In the pre-WWII period of the Nat.Lab.'s history, the Nat.Lab. embraced pilot production. This 'Proeffabriek' (pilot factory) provided a link between research in the laboratory and production in the factories. This was already the case before the lab moved to the Strijp premises. In a letter from Holst to Gerard Philips, dated August 10, 1922,¹¹⁶ Holst alluded to the production of 300 audions per day. All these receiver tubes were destined for foreign markets. The Nat.Lab. produced rectifier tubes and 2 kW transmitter tubes for the Nederlandse Seintoestellen Fabriek (NSF). Near the premises on the Emmasingel, a special pilot factory had been built for the 'test' production of new devices. In this pilot factory small-scale production lines were set up to enable the Nat.Lab. to experiment with production and to gain the knowledge required for possible improvements and transitions to large-scale production. Thus, the pilot factory did indeed serve as a bridge between the lab and the factories. Sometimes only small quantities were needed so that production took place in the pilot factory and there was no intention of transferring to large-scale production facilities. After the move to Strijp, a pilot factory was again added to the lab buildings, and it fell under the technical leadership of P.F.S. Otten, who was later to become one of the company's presidents. J.

Bruynes was appointed scientific head of the pilot factory. Bruynes was responsible to Holst, which shows how close the connection was between the pilot factory and the Nat.Lab. A 1927 survey of the 'Proeffabriek' departments shows that the pilot factory was involved in the production of a variety of artefacts: loudspeakers, low frequency transformers, power supply units, neon lamps, capacitors, rectifier tubes, X-ray tubes, transmitter tubes, inert gas fuses, and resistance amplifiers. There was a bureau for production planning. Part of the factory was a design department with a drawing room. In the 1930s the activities were reduced, and by the beginning of WWII virtually nothing of the pilot factory remained. The pilot factory's decline could partly be attributed to the economic crisis, the consequences of which started being felt by the Philips company in the latter part of 1930, and partly to the fact that various factories had set up development laboratories of their own, which did the same sort of work as the Nat.Lab.'s pilot factory. For the Nat.Lab. this meant that work which was closely related to production issues started being done in the lab again.

'Normal' production took place in the factories. At first, the Nat.Lab. carried out many investigations in order to support the factories directly when production problems emerged. Not much of such research was done in the factories themselves, but this was to change in the course of time. Gradually, the factories set up their own laboratories. The need to co-ordinate the activities of the various labs can be understood if one bears in mind that in 1936 there were 32 laboratories.¹¹⁷ The Nat.Lab. was the largest lab on the list with a budget of NLG 1,312,000, which amounted to about 1% of the total turnover. The other labs spent NLG 3,250,200 which was almost 3% of the company's turnover for that year. In this list Bouwer's X-ray lab is mentioned as a separate lab. The second largest lab was the design group for Apparatuses and Loudspeakers (headed by Laman Trip). Next in size was the Test Department for Radio Lamps (Van Gessel). The range of labs reflected the variety of activities within the company: there were development labs for television, sound, cinema, welding rods, telecom articles, and there was a lighting lab, a glass lab, a lab for enamel wires, a Philite lab, and a lab for armatures.

Often Nat.Lab. representatives attended factory meetings, where production problems were discussed. For example, when the problem of the quality of quartz for lamps was discussed in a glass factory meeting, Elenbaas was present, and it was agreed that the Nat.Lab., the glass factory and the lamp factory would co-operate to work on this.¹¹⁸ There was also the co-operation between the Nat.Lab. and the factory labs regarding the matter of electrical components.¹¹⁹ Early in 1929, for example, the Nat.Lab., the Apparatus factory and the factory for capacitors had meetings to discuss the application of new capacitors for power supply, and until mid-

1928 quality measurements for such capacitors were performed at the Nat.Lab. In the early 1930s the Nat.Lab. and the development group at the factory for electrical components studied mica capacitors with deposited layers. In 1934 the Nat.Lab. assisted in manufacturing problems for film resistors at the Apparatus factory. Finally, the co-operation in 1938/1939 between Dr. Claassens¹²⁰ (Nat.Lab.) and the development group of the electrical components factory and the apparatus factory on pressed resistors made from phenol resin and graphite should be mentioned. A more elaborate example of the issue of co-operation between the Nat.Lab. and the factories and their development groups is to be found in the 'Agreement between the apparatus department of the physics laboratory (i.e. the Nat.Lab., MJdV) and the apparatus laboratory on the normal course of development for receiver apparatus'.¹²¹ This agreement reveals the interplay that existed between Nat.Lab. and the development lab for radio receiver sets. The course described was the following: first, the apparatus factory, the commercial department and the Nat.Lab. had to decide together what type of receiver sets would be developed. In the case of the sets that could be developed in the apparatus factory, the circuit diagram would be discussed during one meeting in which both the apparatus development department and the Nat.Lab. participated. For each set that had to be developed jointly between the Nat.Lab. and the apparatus development department, a Nat.Lab. scientist would come up with a circuit diagram that would be discussed with the apparatus development lab. The elaboration and details (giving the exact values of resistors and capacitors, etc.) would then be done by the apparatus development department, and H. Rinia (Nat.Lab.) could be consulted in this connection. Provisional versions of the circuit diagram would be circulated both within the Nat.Lab. and within the apparatus development lab. A final version was then signed by the development department. Then a chassis would be designed and discussed with the Nat.Lab. A prototype would be made in the development lab and investigated by the Nat.Lab., and if necessary, changes were proposed; a second stage of chassis development would then take place within the apparatus development lab. The apparatus development lab took care of updating the principle scheme in the event of changes. If the apparatus development lab wanted to make changes during the second phase, these changes always had to be discussed with the Nat.Lab. Thus, there was an explicit division of tasks and responsibilities between the Nat.Lab. and the apparatus development lab in a collective development process.

The fact that the Nat.Lab. maintained contacts with the labs and factories can also be deduced from the fact that a special debate was held on the co-operation between the Nat.Lab., the Patent Department and the Commercial Departments. The Patent Department's work could be hampered

if the Nat.Lab. passed on information on new inventions to the Commercial Departments. In the discussion that took place, the Nat.Lab. insisted on maintaining contact with the Commercial Departments. These contacts were necessary whenever the Commercial Departments' wishes and suggestions had to be taken into account in conjunction with the development of new products at the Nat.Lab.¹²² There are also minutes of the 'laboratory meetings' that were held nearly every week in the 1938-1944 period that show that there were frequent contacts between the Nat.Lab. and various factories. Sometimes the means for exchanging information were discussed explicitly. In 1939, for example, it was agreed that a certain Mr. Hardenberg would attend the weekly Nat.Lab. lunchtime meetings on behalf of his factory.¹²³

The existence of a pilot factory closely related to the Nat.Lab. illustrates that in the first period of the Nat.Lab.'s history, the lab was directly involved in production issues and problems. Again, it was Anton Philips himself who gave personal support for this Nat.Lab. activity. As Garratt suggested,¹²⁴ probably in that period, in particular in the early years, there was not much scientific expertise available outside the Nat.Lab. for solving production problems. Holst later admitted that the lab scientists were not production experts, but that on the other hand the production experts were certainly not brilliant when it came to scientific know-how.¹²⁵ The establishment of the factory laboratories would change this situation. The factories would then have their own research facilities. In the early years of the Nat.Lab.'s history, all the different types of work that can be found in a research lab – fundamental research, development work, the solving of practical production problems – were continued in that lab. Later on, the factory laboratories took over the more production-oriented research activities and part of the product development tasks as well. In that respect the broadening of the Nat.Lab.'s scope, in terms of research topics, went hand in hand with a narrowing of the range of activities.

The emergence of labs in the factories was in itself logical given the company's expansion. Already at an early stage of this development, though, the danger was recognised of the Nat.Lab.'s becoming a much more separate entity within the company than it used to be before the days of the factory laboratories. In a letter dated March 25, 1936, J.M. Verff expressed his concern that too much isolation of the Nat.Lab. would mean that perhaps factory labs would duplicate the work of the Nat.Lab., but with less qualified personnel.¹²⁶ Verff feared that the transfer of research output to the factory laboratories would be delayed because the factory labs would repeat the research, but in their own way. Lack of interest in the factory labs would also hamper the taking over of developments that might be important to the company. The increased demands being placed on the

Nat.Lab. (more research into telecom articles, transmitters and transmitter lamps, more television research) could instigate a further spread of research activities in the factory labs because of the lack of capacity in the Nat.Lab. In Verff's opinion this would enhance the disadvantages of decentralised research. He suggested placing a number of the factory labs plus the Nat.Lab. under one directorate, but his advice was not followed. Soon after Verff's proposal, the telecom development group wrote a letter to Otten, one of the company's directors, to emphasise the importance of their independence. According to De Fremery, author of the letter, the flexibility that was needed for the development group to respond adequately to market developments would not be guaranteed if the group was tied to the Nat.Lab.¹²⁷ The tendency for independence grew stronger, and after WWII the factories and their labs became formally independent Product Divisions (see the first Intermezzo). Verff's concerns about the too independent position of the various labs did not result in any concrete action, and the reaction of the telecom development group betrayed why: maintaining too strong a tie with the Nat.Lab. was seen by this development lab as standing in the way of flexible market action. When the independent position of the factories was formalised with the creation of the Product Divisions after WWII, this perceived distance between the Nat.Lab. and the factories' development labs would be continued further and Verff's fears would prove to have been prophetic, as we will clearly see in the next period of the Nat.Lab.'s history, between 1946 and 1972.

4. Research for Diversification in Action¹

In the period described in Chapter 3 (1923-1946) the Nat.Lab. activities were closely linked to the company as a whole. When deciding upon its research topics, the Nat.Lab. adhered to the decisions made by the company's directorate. The communication lines between the company's management and the lab were short. In some cases there was direct involvement on the part of Anton Philips. The Nat.Lab.'s main task was to enable the company to realise the diversification of its product portfolio as decided upon by the company's directorate by developing knowledge on the natural phenomena underlying the products and by establishing a good patent position. Holst stimulated the high-level scientific status of the lab. He also took part in discussions on the future of the company, and so the Nat.Lab. also played a part in the process of establishing a desirable product portfolio for the company as a whole. Holst often took a rather cautious viewpoint, and frequently expressed doubts about the commercial feasibility of new products. In the case of television, for example, the company's directorate urged that more television research be done, and Holst had to go along with this, in spite of his own hesitations about it.

To make the laboratory fulfil the role described above, Holst had to find a solution to two puzzles: in the first place he had to strike a balance between academic and industrial orientations in the scientists' attitudes, and in the second place he had to strike a balance between giving top scientists the freedom they required to achieve great things, while maintaining sufficient control over the lab population's work to ensure that their contributions would add up to a useful totality.

The Nat.Lab. was a hybrid lab. Not only phenomena-oriented research, but also development work and practical problem-solving in relation to production problems took place. In the early years the Nat.Lab. also had its own pilot production facilities. Later on, the development of new products was achieved in co-operation with the Nat.Lab. and the factory's development laboratories, as in the case of radio set development. In other production problem cases the Nat.Lab.'s assistance was sought. Here, too, we find short communication lines between the Nat.Lab. and the rest of the company. Altogether, this gives the impression that the Nat.Lab. was a 'natural' part of the whole company. The changes in the company during the 1923-1946 period, therefore, had a direct impact on

the nature of the Nat.Lab. The company's product diversification resulted in an enormous extending of the range of research topics. Simultaneously, the coming of the factory laboratories caused a narrowing of the range of research activities because these laboratories took over some of the development and production problem-solving work that had previously been done in the Nat.Lab.

In this chapter general characteristics of the Nat.Lab. in the 1923-1946 period will be illustrated by taking three case studies to show what the Nat.Lab.'s research into diversification in action was like. In each case those characteristics will be highlighted in the introduction and conclusion sections of the case study in question. The case studies have been selected in such a way as to reflect the research scope. With some research topics, such as television, the work in the 1923-1946 period will be described in conjunction with the introduction to the case studies of later periods (in Parts II and III).

4.1 Gas Discharges and Mercury Lamps

The first field of research that the Nat.Lab dealt with, was the incandescent filament lamps field. Very soon, though, the research was to extend to other fields, and gas discharge was one of the first phenomena to attract the interest of the Nat.Lab. researchers Holst and Oosterhuis. As has already been mentioned, even a famous physicist like Gustav Hertz was attracted to the Nat.Lab. to work in this field. In this case study the research done into gas discharges and the mercury lamps resulting from that research will be described in some detail.²

In this period, in the first place, there was direct contact between the company management and the research activities. In particular, it was Anton Philips who personally kept in touch with the research activities. In the second place, research activities were closely related to factory problems. Both aspects show how firmly the research lab was embedded in the company as a whole. The case study also illustrates that the influence of individuals could be very strong in the pre-WWII period, in particular while the lab was still small-scale. For Holst, this meant that there was a certain tension between giving strong characters room for individualistic work and trying to make them fit their activities into the research programme as a whole. Finally, this case study was chosen because research into lighting devices throughout this period remained an important part of the research programme and led to many publications. According to Hutter,³ the Philips researchers even published more articles on gas discharge research than their colleagues at the Universities of Utrecht and Delft put together. The Nat.Lab. publications show that at Philips the focus was on the gas discharge lamps, whereas in the universities the focus

was on the phenomena in the lamps. Yet, the Nat.Lab. publications did contribute substantially to the understanding of the phenomena. This reflects Holst's strategy to combine scientific research and the company's interests as much as possible.

For Philips, the direct context of the work on gas discharge lamps was the European market. According to Heerding,⁴ the work of other European companies was of more direct strategic importance to Philips than the work that went on in the USA. In the USA patents were mainly used to protect the domestic market, whereas in Europe patents played a role in the efforts of companies to invade other countries' markets. Scientifically, though, contacts with the USA, and in particular with GE, were of importance for both parties. GE researchers, Whitney included, went to Philips to learn about the work on the metal filament lamps. Vice versa, Philips researchers visited GE. There were also licence arrangements between Philips and GE. Heerding claims that the increasing administration of GE patents was even one of the motives for Philips to found its patent department.⁵ GE allowed Philips to take licences on the condition that it would not try to invade the USA market. In Europe, Philips' main concern was to work its way into the various cartels to establish its market position. In particular, the Phoebus cartel was important for Philips. The German Osram firm had taken the initiative for this cartel. The idea was to create stability in the light bulb market. Within Philips it was assumed that for most participants in the cartel the aim was to limit Philips' expansion in the light bulb market. Yet, Anton Philips, although initially hesitant, decided to take part in the cartel with the expectation that Philips would gain from it in the end. In the cartel the world light bulb market (except for the USA and Canada) was parcelled among its European participants. The formation of the Phoebus cartel was supported by GE, because it was expected that coherence between the European competitors would yield better opportunities for negotiations in which the USA market could be protected from European influence. GE stimulated Philips' participation in the cartel by setting Philips free from paying certain licences if it would agree to become part of the cartel.

The Development of Theories about Electrons

Research into gas discharge phenomena in the Philips Nat.Lab. was based on existing knowledge about electrons. This knowledge was founded on a long tradition of scientific study into the phenomena of electricity, magnetism and light. Electrical discharge phenomena were studied by Musschenbroek with his Leyden jar as early as the mid-eighteenth century, and later by Coulomb and Volta. In the nineteenth century Michael Faraday experimented with glow discharges. Maxwell and Heinrich Hertz (not to

be confused with Gustav) made a mathematical translation of what Faraday and others had determined experimentally. All these researchers had considered discharges at the macro-level. Micro-level research experienced a breakthrough at the beginning of the twentieth century when Thomson discovered the existence of a negatively charged particle: the electron. The classical approach to describing electron behaviour was soon overthrown by the emergence of quantum physics. This new knowledge provided an explanation for the line spectra in gases that had been observed by Wheatstone, Foucault and others in the nineteenth century. A theoretical framework for considering the phenomena of gas discharges was therefore available to the Nat.Lab. researchers when they entered the field in 1915.

Low-pressure Gas Discharge Lamps

As we saw in Chapter 2, Hamburger, who was stationed in the chemical lab, or 'Lab V', was the first to study gas discharges. His intentions were purely practical: by examining gas discharges he could identify gas remainders in vacuum lamps, but he also became interested in getting to know more about the phenomenon itself. In 1917 he received his Ph.D. for a thesis entitled 'On light emission by gases and mixtures of gases through electrical discharges'. In the preface to his thesis, Hamburger acknowledged the work of Holst, who had helped him to combine different measurement methods. This shows that in 'Lab IV' Holst had also become interested in gas discharges. Holst had two motives for that: in the first place, he needed to know about gas discharges to improve the filament lamps in which this phenomenon was to be reduced. In the second place, Holst believed that the gas discharges could be used for a new type of lamp. For some reason, out of the several possible options, Holst chose mercury lamps for experimentation. Kloppers, a lab assistant, was instructed to repeat the Franck and (G.) Hertz experiment to study the excitation potential of mercury. Later on, Holst revealed what his idea had been. He had wanted to try out all combinations of glass and melting wire, tungsten or something similar, and an inert gas or vacuum, or whatever might be technically usable. Hertz had joined the Nat.Lab. in 1920 and had carried out experiments to test the value of the Bohr atom model in order to explain the gas discharge phenomenon. Hertz's work resulted in 28 articles, most of which dealt with the excitation and ionisation potentials of different gases. Thus, we see that the research was a practical search for both possible new products and a scientific explanation of the phenomenon of gas discharges, which used current knowledge in the discipline of physics.

The same combination can be found in the work on low-pressure gas discharge lamps that went on in the GE labs in the same period. W.R. Whit-

ney, E. Weintraub and C.P. Steinmetz were the leading figures in that part of GE research. Their work led to important successes in the ignition mechanism for such lamps, for which they acquired several patents. Weintraub also published some articles about the phenomena in gas discharge lamps that reveal his theoretical interest. As was mentioned in the introduction to this case study, the combination of scientific ambition to contribute to theory development and the awareness of the company's interests were also characteristic for the work at the Nat.Lab.

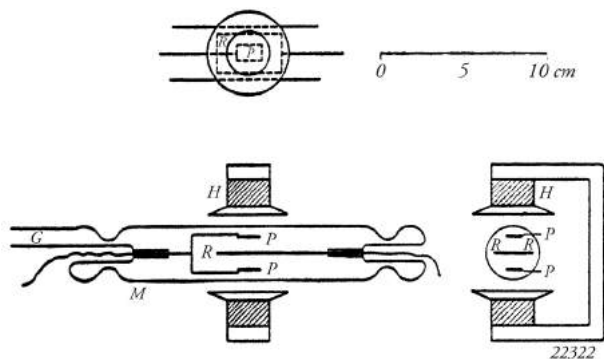
In 1923 the Nat.Lab. gas discharge research was continued in the new building in the Strijp district. Not only Holst and Oosterhuis, but also De Groot, Dorgelo, Penning, Zecher, Uyterhoeven, Druyvestein, Elenbaas and Kruithof took part in the gas discharge research. Holst and Oosterhuis experimented with gas under low pressure between two flat, parallel, unheated metal plates. They discovered that in glow discharges, small layers could be observed (later to be called Holst-Oosterhuis layers), which according to them could be explained by assuming that the distance between two layers represents the distance that the electrons travel before they have acquired the lowest gas excitation energy.

W. de Groot joined the lab in 1923. He studied the relative intensity of spectral lines in gas discharges. He also studied the possibility of determining the concentrations of ionised and excited atoms in the gas in question via absorption. Before 1935, when research into low-pressure mercury lamps (without fluorescent powders) had been completed, he had published 35 articles.

H.B. Dorgelo had worked with Ornstein in Utrecht and had written a Ph.D. thesis on 'The intensities of the components of multiple spectral lines' in 1924. He arrived at the Nat.Lab. in 1924 and left again in 1927 to become professor at the polytechnic in Delft. He published 24 articles during his Nat.Lab. period.

F.M. Penning joined the lab in 1924. He was able to show that electrons in low-pressure mercury discharges submitted to direct current can reach very high velocities because of the high-frequency oscillations in the gas. Furthermore, he measured the Paschen curves in gases with low concentrations of impurities. Finally, he studied the influence of magnetic fields on gas discharges. For that purpose he developed what was later to be called the Penning manometer. Penning was a prolific author: 47 of his articles were published before 1935.

G. Zecher and W. Uyterhoeven were both appointed to do practical research. They did not publish much (in total 7 articles before 1935). Zecher started experimenting with mercury lamps in 1924 using neon as the ignition gas (the 'blue lighting tubes'). As we saw in Section 2, neon tubes were already used for advertising purposes, and Philips had been produc-



Philips' Vacuum-meter with two plate-type cathodes P_1 and P_2 and ring anode R in the field of a permanent magnet H.

Figure 10. The Philips Vacuum meter by Penning.

The magnetic field produced in valve M by magnet H makes the electrons oscillate in spirals between cathodes P_1 and P_2 before hitting ring anode R. On their way the electrons hit gas molecules and cause gas discharges. The number of discharges is a measure of the pressure of the gas (from *Philips Technical Review* Vol. 2, p. 207).

ing neon glow lamps since 1917. Zecher was not very involved in theories but tried rather to find relationships between the practical parameters such as tube diameter, gas pressure and current intensity.⁶ Later, he also experimented with argon-filled tubes. Purely on the basis of trial-and-error, he found that a pressure of 20 mm Hg and an argon percentage of 10% yielded the best results for a lamp. He was not concerned with the theoretical explanations for those observations. It was Penning who went on to use some of Zecher's results to search for such explanations.

M.J. Druyvestein joined the lab in 1927. He too had studied with Ornstein in Utrecht. As a result of his research, he was able to establish more about the distribution of electron velocities in an electric field, by taking into account that these electrons had a non-negligible drift velocity on top of their random velocity. This distribution is now known as the Druyvestein division. In the 1914-1935 register of publications, we find 20 titles under his name.

W. Elenbaas started working at the lab in 1930 after he had received his Ph.D. from Ornstein for a thesis on 'Intensity measurements in the helium spectrum'. His work will be discussed later on when we come to discuss high-pressure gas discharge lamps.

In 1934 Kruithof was the last new physicist to be attracted to work on gas discharges. He had received his Ph.D. in Utrecht for work on electron excitation of hydrogen molecules. He chiefly worked with Penning.

Holst soon realised that a lamp required more than a discharge of gas. He started looking for fluorescent powders that would yield light in the right colours. In particular, it was white light that was seen as desirable, because people were used to having that sort of light from filament lamps. The idea of fluorescent lamps was not new: in 1852 Stokes had already studied fluorescent light, and at General Electric it was Steinmetz who in 1902 had made a lamp that was covered with fluorescent powder on the outside. GE had already acquired a good patent position in the field. Other firms that were active in the field of fluorescent lamps, were Claude in France and the Hygrade Sylvania Corporation in the USA. In 1917 Holst experimented with willemite zinc silicate as a fluorescent powder. The same material was used in the GE labs in that period. In 1923 De Groot created a high-voltage tube with fluorescent uranium glass (bought in an art shop). In the 1933-1939 period it was Zecher, in particular, who experimented with different tubes and powders. As we saw above, he was a practitioner and tried out various possibilities to find an optimal practical solution for in-house lighting. Zecher discovered that although fluorescent glass was sufficient for producing a lamp, the addition of fluorescent powder increased its efficiency. Zecher also did duration experiments to test the technical lifetime of the lamps. He measured light output as a function of time and used various commercially available fluorescent powders. Another practical problem that Zecher tried to solve through experimentation was to find the right connecting voltage for the lamps. To that end he developed a resonance circuit.

More practical problems were studied by others in the Nat.Lab. For example, new fluorescent powders with an after-glow effect were tested to solve the problem of flickering when alternating current instead of direct current was used for the lamps. Likewise, the practical problem of interference with radio waves was studied by C. Verburg.

In 1934 a new department was set up in Philips for the purpose of dealing with the industrialisation of the gas discharge lamps. The name of the department, Philora, was a blend of the name Philips and the word *auro-ra* (an allusion to the colour of the morning red sky, which was appropriate, because the first lamps Philips produced were sodium lamps – without fluorescent powders – that produced red light when started up and yellow light once the lamp had warmed up). In the Philora department the problem of the atomising of cathodes and the resultant blackening of tube walls was studied. This study was very practically oriented, and no theoretical studies were done like in the Nat.Lab. The Philora lab also did a great deal of quality testing. All the research and development efforts on gas discharge lamps were thus divided over the Nat.Lab. and the Philora department.

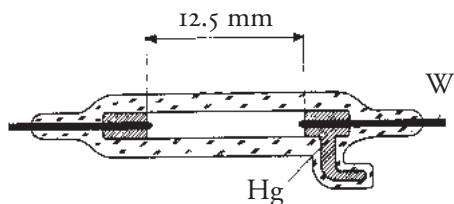
To sum up, the research into low-pressure lamps – both with and without fluorescent powders – constituted a mixture of theoretical and practical work, carried out by several individuals, each of whom had their own research areas.

High-pressure Mercury Lamps

In the 1930s a new type of gas discharge lamp emerged in the Nat.Lab. research: the high-pressure mercury lamp. The idea was not new. Küch and Retschinsky in the W.C. Heraeus lab in Hanau, Germany, has already published about their work on high-pressure mercury lamps in 1907. Their work shows the same combination of scientific ambitions and awareness of practical use that we noted for the work at GE and Philips.⁷ In the UK, the Westinghouse Cooper Hewitt Company had started production of high-pressure mercury lamps in 1908. In the Nat.Lab. it was particularly the physicist Willem Elenbaas who did a lot of work on this type of lamp. Most of Elenbaas's work can be characterised as the search for relationships between design parameters, like the electric current through the lamp, the lamp diameter, the gas pressure, and the parameters in the physicists' theory of this type of lamp. Elenbaas did both theoretical and experimental work. He studied the instability of high-pressure mercury lamps and found that this work helped to reduce the amount of mercury so that all the mercury was vaporised as the lamp burned. Elenbaas also developed an auxiliary electrode to facilitate electron emission during the starting-up of the lamp, at a point when the other electrode was not yet hot enough. In 1934 such lamps were being produced in the lab's Pilot factory. This was known as a HO (the H stood for Hg, mercury, and the O for oxide cathodes, the self-heating cathode that Elenbaas had developed). The next step was to develop a one-coil transformer to make the lamp suitable for 220 V and 120 V. It was thought that this lamp would be suitable for street lighting and capable of replacing the sodium lamps. The problem, however, was that the lamp could only be used vertically to ensure that the glass wall did not melt (the lamp had to be operated at a very high temperature). In 1937 the lamp was produced with Osram glass, so that it could be installed horizontally as well. So from the research into high-pressure mercury lamps we see evidence of the same sort of combination of theory development and practical problem-solving which had also characterised the work on low-pressure mercury lamps.

Super-high-pressure Mercury Lamps

The main reason for moving to higher pressures in mercury lamps was to search for a better projection lamp. In 1935 Nillesen of the Cinema Department had requested such a lamp. Elenbaas had discovered the



Water-cooled super-pressure mercury vapour lamp of capillary form (SP type) [MD]: it is rated at a power of 1000 W for an arc length of 12.5 mm, the vapour pressure being of the order of 1000 atm.

Figure 11. Water-cooled super-pressure mercury vapour lamp (from *Philips Technical Review* Vol. 18, p. 170).

influence that pressure could have on the surface brightness of the lamp. Bol, who was a good practitioner, had been able to construct a lamp with a pressure of some hundred atmospheres. Such pressures yielded a surface brightness of 180,000 candela/cm². By cooling with current water, Bol and his assistant, Lemmens, were able to prevent high temperatures causing damage to the lamp.

Another practical problem they were able to solve was that of getting the electrode through the quartz glass. Bol claimed that his contributions to the super-high-pressure mercury lamp had been so vital that he could claim to be the inventor. This gave rise to a conflict with Elenbaas who, for instance, pointed out that he already thought of the idea of cooling on the basis of theoretical considerations before Bol and that it had been he who had suggested to Bol that more mercury should be used per centimetre of tube length. The quarrelling that followed is illustrative of the prestige that certain individuals attached to having one's name connected to an invention. We also see that in this case the theoretical and practical approach was followed by two people simultaneously and that they both reached the same conclusions. Although there are no sources to prove that Elenbaas had indeed suggested cooling to Bol, Hutter shows that Elenbaas's theoretical considerations could very well have led to that idea. Bol had never seemed to be very interested in theory and tended to work mostly in a more practical way.

About 1940 the work on gas discharges and gas discharge lamps faded out.⁸ Elenbaas left the Nat.Lab. in 1942 and was soon put in charge of the lighting development laboratories. In 1940 Druyvestein moved to metal physics, and shortly after the war ended he became a professor in Delft. Penning moved on to short-wave research. According to Druyvestein,

there was a general feeling that gas discharge research was more or less a thing of the past in that no more fundamentally new discoveries were expected. There is no evidence to support the notion that WWII significantly affected lamp research.

The gas discharge lamps case study showcases several characteristics of the Nat.Lab. in the 1923-1946 period. The fact that during this period individual scientists and assistants were keen to manifest themselves and to have their names recognised can be illustrated by certain anecdotes that were later told by Druyvestein and E.G. Dorgelo (an assistant of De Groot's, not to be confused with the scientist H.B. Dorgelo). Dorgelo had worked for De Groot and Bol on sodium lamps. In particular, it was the problem of the short lifetime of the sodium lamps that had been studied by Dorgelo. Dorgelo had developed a series connection for Bol's lamps. When he went to the notary to register the invention, Bol was there too, and Dorgelo feared that Bol might claim the invention for himself, but Bol declared that the invention was totally Dorgelo's. Later, though, when Dorgelo came up with another invention for De Groot, De Groot then demonstrated it to Holst and gave no credit to Dorgelo. Evidently, the relationships between scientists and assistants varied considerably.

Likewise there was a competition between Bol and Elenbaas, as described earlier (when the super-high-pressure mercury lamps were discussed). Elenbaas later also confessed to having felt irritated when he had advised Bol to cool a lamp with water and Bol later never mentioned Elenbaas, but instead claimed it as his own idea.

A further aspect that characterised this period was the direct contact that existed with other parts of the company. For gas discharge lamps, too, there were many contacts within and outside Philips. Direct contact was maintained with the Philips factories. An assistant, M. Bandringa, later explained that when glass was needed for the work in the lab, he would go directly to the glass factory and get it there. In a similar way he obtained tungsten wire and spirals from the factory on the Emmasingel. Officially, there were forms to be filled in, but often delivery was made and forms were never bothered about, which illustrates how informal the contacts were.

In some cases there was serious criticism on the Nat.Lab. from the factories. In 1937 the Cinema Department complained that the Nat.Lab. had seriously underestimated the lifespan problems of the super-high-pressure mercury lamps. In 1952 a member of the Lighting Factories Directorate, Deenen, mentioned in a memorandum to the company's Board of Management that in his opinion, the Nat.Lab. lagged behind developments outside Philips. According to him, most real inventions were taking place at GE in the USA and General Electric Company in the UK, and had to

be bought by Philips. Probably that assumption was true. In historical records on gas discharge lamps, the name Philips is hardly ever mentioned when new inventions are described. There were regular contacts, though, with GE, the General Electric Company, and also with Osram, Siemens and British Thomas Houston. Patents were exchanged, there was co-operation when it came to defining standards for lamps, and the companies used each other's products. Another important contact was KEMA (the Dutch company in charge of approving electrotechnical devices and materials). All Philips products needed the KEMA hallmark to be sold. There was a special KEMA committee for lamps. Holst took part in this committee and was often able to influence the establishment of the criteria required for the hallmark.

Evidently, for several of the people who worked on the gas discharge research, this was a continuation of what they had been studying before they joined the Nat.Lab. That would seem to be in contradiction with one of 'Holst's rules' that Casimir formulated (see section 4.4), which stated that Holst usually made people work in fields that were totally unknown to them. According to Casimir, that was meant to prevent scientists from being hampered in their creativity because of already having certain fixed ideas about a given field of research. But in the case of gas discharges, Holst apparently took a different approach and allowed people to work on their own particular area of expertise. At the same time, Holst did gather together a group of people with varying backgrounds so that a wide variety of aspects of gas discharge phenomena could be studied. Holst wanted the gas discharge research to aim at understanding all the aspects of the phenomena, while at the same time ultimately resulting in the creation of a new lamp. Both aims were achieved to a certain extent. Several of the Nat.Lab. scientists contributed to the scientific discipline and are remembered because the outcomes are still connected with their names (Holst-Oosterhuis layers, Penning manometer, Druyvestein distribution, the Elenbaas-Heller equation). The research work also provided important support for the development of the various types of gas discharge lamps. As has been illustrated the work that was done combined theoretical work on electron theories and practical 'trial-and-error' activities to find the right combination of materials and conditions.

4.2 X-ray Tubes

The second case study taken from the 1923-1946 period relates to the development of X-ray tubes.⁹ In the previous case study we already saw how important the role of individual scientists and assistants was at the Nat.Lab. Probably, the X-ray tubes case study will form a climax in that respect. The role of scientist named Bouwers is one of the interesting fea-

tures of this case study. Bouwers was able to establish a rather independent position for himself. In a survey of the work conducted by the various labs in the mid-1930s, we find that his X-ray Department is mentioned separately from the rest of the Nat.Lab., so that it almost appears that he had his own laboratory.

A second reason for choosing the X-ray tubes as a case study has to do with the type of customer for whom the products were developed. In the case of gas discharge tubes, the customers were consumers. In the case of the ferrite magnetic materials, our third case study (see section 4.3), the customers were within the company: they were the developers and users of permanent magnets in products such as Pupin coils for telephone wires and magnets for loudspeakers. In the case of the X-ray tubes, the customers were external professionals: medical specialists who used the X-ray tubes for diagnosis and therapy. In that respect the X-ray tubes might be seen as one of the first Philips products in the professional area (the transmitter tubes that were developed for and in co-operation with the Dutch PTT are another early example).

The X-ray tubes case study is also suitable for illustrating the general characteristics of the relationship between the Nat.Lab. and the company as a whole. The work that the Nat.Lab. contributed to product diversification was characteristic for the Interbellum period. As with so many new developments of that period, we see here again a close connection between the Nat.Lab., production, and the Philips management, and in this case also, with Anton Philips in particular. In the case of the X-ray tubes, the contact between Anton and the Nat.Lab was sometimes directly between Anton and Bouwers and not via Holst. This gave some people the impression that Bouwers sometimes worked behind Holst's back. Bouwers was also one of the very few Nat.Lab. scientists to be made a procurator of the company in 1933, which gave him a status almost like Holst's. Bouwers had his own secretary, which again was something unusual in the laboratory. In terms of product diversification, there was no financial incentive to develop the X-ray tubes. No profits were made, but nevertheless Anton saw this product as a desirable extension of the Philips product portfolio, and the Nat.Lab. gave him the opportunity to realise this.

These three features formed the main ingredients for interesting developments within the Nat.Lab. These developments took Philips into the medical systems business sector, which is still one of its main fields.

The Introduction of X-ray Technology to the Nat.Lab.

X-rays were discovered in 1895 by Wilhelm Conrad Röntgen, a German scientist who discovered that a screen would light up when a beam emitted from a high voltage electron tube hit it. Around 1905 Coolidge, a GE scientist, developed special tubes for producing these kinds of rays.¹⁰ The

electrons were thus emitted from a cathode and hit the anode that was placed at a certain angle so that a radiation beam would leave the tubes at an angle. The energy of the radiation was sufficiently high to go through certain materials but be stopped by other, more solid, materials. This characteristic could be used to distinguish tissue and bones in the human body, and thus the tubes could be used for medical diagnosis. By 1913 the GE laboratory was manufacturing Coolidge X-ray tubes on a small scale. A second, later application of the X-rays was for materials diagnosis: an analysis of the way X-rays are scattered by crystals gives information about the lattice structure of the crystal in question. In 1899 the University of Amsterdam installed the first professorship for X-ray sciences in Europe. In 1901 the new professor for this chair, Wertheim Salomonson, established the Dutch Association for Electrology and X-ray Sciences, which shows how early the Netherlands was involved in the application of X-rays. This makes it all the more surprising is that X-ray tube production did not take place in the Netherlands, even though that technology was an issue that certainly had Salomonson's interest. In Germany, the Müller company in Hamburg submitted a first patent application for an X-ray tube with a water-cooled anode in 1899, and went on to successfully produce the new tube. At the 32nd General Assembly of the Dutch Association for Electrology and X-ray Sciences, held in 1917, Salomonson regretted having to speak about his failed attempts to find Dutch companies willing to produce X-ray tubes. Scarcely a year later he was happy to report in a Dutch journal for medicine that Philips had started producing X-ray tubes. Why did Philips decide to involve itself in this line of business?

Holst had been confronted with the need of physicians to have their X-ray tubes repaired and maintained. During WWI these same physicians found themselves unable to buy new tubes from German companies, such as Müller. As far as the Nat.Lab. was concerned, the X-ray tube did in some respects resemble the other tubes (lamps) that they were used for manufacturing, because they too contained a combination of glass, metal and vacuum. The Nat.Lab. was thus able to use its existing expertise to help physicians. Meanwhile, Holst established some contacts in the medical world to get to know more about this technology. In December 1917 he went to the Antoni van Leeuwenhoek Hospital in Amsterdam and talked with G.F. Gaarenstroom, brother of J.H. Gaarenstroom, procurator and later a Philips company director. Holst made a survey of the numbers of tubes being purchased and of the companies producing them (among those listed was the Müller company in Germany). From the fact that Holst took notice of both the technological and the market aspects, we can again see how closely related these two aspects were in his eyes. Holst wanted to gain a better understanding of X-ray tubes, which would be useful for the repair and maintenance of tubes and would also enable development work. In 1919 Holst and Oosterhuis submitted a first patent

proposal on X-ray tubes (with an automatic regeneration device). The new tube contained a device for replacing the gas filling that leaked away as electrical discharges took place in the tube. In 1922 a second patent was applied for by Holst. This patent had to do with the connecting of glass and metal that caused such trouble when it came to producing tubes.

In 1918 a small-scale production line was set up by Holst in order to supply new tubes for Dutch physicians who could not procure them from Germany. The scale of production was small enough to be realised within the Nat.Lab. while at the same time production remained closely related to research. There was a pilot factory for this sort of small-scale pilot production. A sales brochure dating from 1919 shows what types were available: one with a tungsten anode that was not water-cooled and a water-cooled type for in-depth therapy. In 1920 Holst took part in a medical exhibition in Utrecht where he presented the tubes produced in the Nat.Lab. In that year Albert Bouwers joined the Nat.Lab. He came from Ornstein's academic laboratory in Utrecht, and he particularly wanted to be involved in X-ray research. With his arrival, a new era of Nat.Lab. X-ray research was to commence.

Bouwer's First Years at the Nat.Lab.

When Bouwers started his work in the Nat.Lab. on X-rays, people already had some knowledge about the harmful effects of X-rays. It was known that the skin and organs could be damaged. This gave Bouwers a motive to work on a tube that would have a special device to protect people against such effects. The tube that Bouwer developed also had to be safe for the medical assistant using the tube and working with high voltages. In 1924 Bouwers came up with a patent proposal for a new tube that was to become known as the Metalix. The name of the tube derived from the metal tube wall that prevented X-rays from being emitted in undesirable directions. The X-rays were only allowed to pass through a small window. The connection of the metal wall to the glass part of the tube was possible thanks to Holst's earlier invention of the metal-glass connecting technique. Apart from this metal wall, the principle of the tube was still the same as that of Coolidge's tubes, and those had been patented. Bouwers cleverly got around those patents by filling the tube with helium under a 1/1000 mm Hg pressure (Coolidge's tubes were high vacuum). Quite unexpectedly, the helium also diminished the electrical charge that had in the past always emerged on the inside of the glass bulb. The metal wall enabled easy earthing of the tube so that charge flash-over was prevented. All in all, the Metalix had a number of advantages compared with previous X-ray tubes. This was generally recognised when Bouwers presented his invention. When Bouwers demonstrated the Metalix tube at a large international fair in Stockholm, the press even referred to his invention as

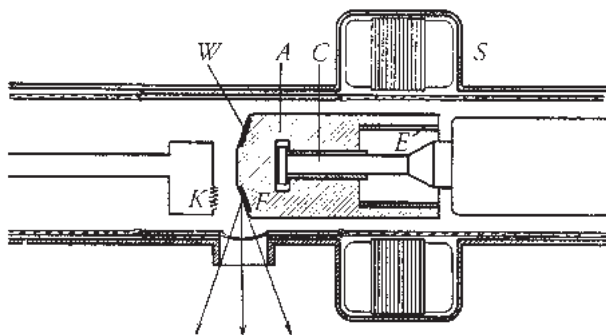
the only true innovation on display at that event. Industrialists and physicians all responded to it positively. Tests carried out by various experts confirmed the positive effect of the design on the amount of radiation coming out of the tube. Soon the negative aspects of the design started being noticed: the tubes often had quite short lifespans. When the tubes were used frequently cracks would appear in the chromium iron of the protective hood on the glass wall, and the tube would start leaking.

Nevertheless, in general the reputation of the Metalix was good. However, Bouwers was soon to be confronted with a phenomenon that was unusual for the Nat.Lab., and for the Philips company as a whole. Until then, Philips had been used to producing mass consumer goods. With such products there is no direct contact between the company and the customer, and in general the customer has no technical expertise. With the X-ray tubes this was different. It was not only the case that the number of tubes produced was small, but it was also the case that the customer was an expert in the medical field. That was why Bouwers was sometimes challenged by users to come up with sophisticated solutions to very specific problems. A good example of that was the request by Dr. Koningsberger of a botanical laboratory in Utrecht for the delivery of a tube that could be used to investigate cell walls in plants. In order to be able to deal with such specific requests and questions, a number of physicians were brought in to contribute to Bouwers's work from their specialist perspectives. Thus, people such as Dr. Daan and Dr. Van der Plaats temporarily worked at the Nat.Lab. (Van der Plaats received his doctor's title for a thesis based on the work that he did there).

In 1927 a second major step forward was made by Bouwers. In that year he produced a new type of tube with a rotating anode, which was to become known as the Rotalix. This tube solved the problem of the deterioration of the anode due to the constant heat that was produced by radiation. Because the anode rotated, the heat was no longer concentrated in a certain area. This made it possible to increase the voltage so that sharper images could be produced. Although the idea as such was not new (a certain Mr. Breton had come up with the idea in 1898), it had never been successfully applied in a functioning X-ray tube. The tube also had its negative sides: bearings were needed for the rotating, and they could get overheated. Yet this tube too, just like the earlier Metalix, gained favourable acceptance among physicians, in particular for lung disease diagnoses.

Finding a Position on the X-ray Market

Although the positive characteristics of the Metalix and Rotalix tubes were generally acknowledged by the users, it soon became evident that the market for such products was problematic. As has already been pointed out, X-ray tubes were not mass consumer products, like most other Philips



Construction of a 'Rotalix' tube (diagrammatic). The discharge space between the cathode K and the anode A is completely surrounded by metal. The cathode is mounted eccentrically in the tube and constructed in such a way that a line focus F occurs along a line of revolution of the conical front surface of the anode, upon which a layer of tungsten W is applied. The anode turns about the axis C. It is driven in the manner of the stator windings S. The iron field ring E causes as many lines of force as possible to pass through the copper of the anode.

Figure 12. The Rotalix (from *Philips Technical Review* Vol 3, p. 295).

products. There was also sharp competition from the Müller company in Germany already referred to in this section. Unlike Philips, Müller had a well-established market channel and aggressively marketed its own tubes. In order to do away with this competition, Philips decided to buy shares in the Müller company, and in 1927 it even bought the entire company. Since the early 1920s there had been some contact with Müller. Müller had taken advantage of that contact by using production methods for the X-ray tubes copied from Philips. It was evident, though, that Müller also had expertise of its own, because soon after it had been taken over by Philips, knowledge started being exchanged between the companies. From 1928 onwards monthly meetings were set up for that purpose.

Originally, it was the idea that research should be concentrated in Eindhoven (at the Nat.Lab.) and production should be carried out in Hamburg (where Müller was located). This division between research and production appeared to be problematic. So both Hamburg and Eindhoven maintained their research as well as their production departments. There was frequent contact between the two production sites. In 1930 the production moved from the Proeffabriek (the pilot factory, which in fact was part of the Nat.Lab.) to the Apparatenfabriek (the apparatus factory). By the end of the 1930s the Proeffabriek had to be called upon again to provide its expertise because there were various technical production problems in the Apparatus factory.

Philips tried to penetrate the American market, too, but there again the company was confronted with sharp competition, namely from GE. Major reorganisation within the American Philips factories in 1938 did not lead to sufficient improvement in the situation.

The financial reports on X-ray tube and systems production show that this was not a profitable line of business for the company at all. In the 1923-1930 period a total investment of NLG 3,800,000 was made. This amount consisted of investments in lab buildings and equipment (about NLG 405,000), investment in the factory (NLG 243,000), lab costs (NLG 1,212,000), stock (NLG 1,600,000) and a loss of NLG 340,000. Between 1928 and 1930 around 12,000 tubes per year were produced. In the following years the situation certainly did not improve. In 1933 almost 7,000 tubes were produced, and there was a turnover of NLG 4,771,000 and a loss of NLG 991,000. By 1938 the losses had further increased to NLG 1,253,000. Although there are no figures for the total Nat.Lab. costs and turnovers for that year, it can be assumed that this amount was quite substantial. This of course raises the question of why this activity was continued. In a meeting convened between Holst, Bouwers and Anton Philips, it was said that 'the necessity is recognised of allowing the X-ray Department to develop in a way that fits in with our current position in the field of the X-ray technology'. This quote suggests that Anton shared the Nat.Lab. interest in the technology of X-ray equipment. This is borne out by the fact that he let his own personnel undergo X-ray examinations in those years. Whatever Anton's motives for doing this may have been – either to prove the usefulness of X-ray technology, or just to show his humanistic attitude towards his personnel – it at least illustrates his commitment to X-ray technology. Interviews with former scientists from the X-ray Department have revealed that for Bouwers technology was the driving force for his dealing in X-ray technology. Market requirements such as operational safety were not a primary concern for Bouwers. For example, he rejected the idea of oil lubrication for X-ray tubes so vehemently that irritation was expressed about this at an Orco meeting in 1938. Some people also asserted that the financial production losses, at least in part, were due to the fact that Bouwers constantly came up with technological innovations, which meant that production constantly had to be adapted to these new ideas. Such unrest of course did not stimulate profitability for X-ray tube production.

Production was continued when the country was occupied in WWII. The German Verwalter decided that the X-ray tubes should be exported via Müller in Hamburg. On December 6, 1942, the allied airforces bombed the Philips factories in Eindhoven. The X-ray factory was hit, and most of the facilities were destroyed. After the war, the X-ray activities were subsumed under the new Product Division of X-ray and Medical Apparatuses. At last, the production finally started to yield a little profit. It would secure Philips' position in the medical equipment field for many years to come.

The X-ray tubes case study is an intriguing one that leaves the reader somewhat bewildered. Could it really be true that Bouwers had succeeded in freeing himself from Holst's supervision, even though as the lab's manager Holst seemed to have been able to influence every other part of the research programme? And how was it possible that the clever entrepreneur, Anton Philips, approved of an activity that kept yielding substantial losses for so many years? That this activity was not profitable can be explained by the fact that sales were disappointing. But why then continue? Was it, as Blanken suggests, that he expected to get grips on the German radio patents by acquiring the Müller company with its X-ray expertise?¹¹ Or was it his humanistic concern about the availability of X-ray technology for Dutch hospitals? Having seen the data, we can only conclude that no final answers to those questions can be given. Whatever the best interpretation of the data may be, the X-ray technology story in the Philips company provides a good example of the role fulfilled by colourful individuals in a technological innovation process.

4.3 *Ferrites*

Ferrites are non-metallic magnetic materials that can be used for permanent magnets and for cores in electromagnets. Research in the field of ferrites is an example of materials research instigated by practical needs (magnetic material for Pupin coils in telephone cables and in loudspeakers).¹² Ferrites particularly suited the purpose because they have a high electrical resistance and therefore low Eddy currents. Eddy currents arise from high frequency signals in magnet cores; the overall result is energy loss. The need for those materials was not established through trial-and-error but by investigating the magnetic properties of a certain group of materials, the ferrites, that had $\text{MeO} \cdot \text{Fe}_2\text{O}_3$ as their molecular formula (whereby Me is a bivalent metal oxide, like Cu or Pb). This is similar to the way in which gas discharges were dealt with. The expectation was that having a better understanding of the underlying phenomena would result in better designs.

In the case of gas discharges, Elenbaas was a key person. Here, too, we find such a key person, namely Snoek. In the gas discharge case we saw that individuals such as Elenbaas and Bol were very eager to have their name linked to an invention. This should make us cautious about too easily presuming that inventions were the domain of those individuals who claimed responsibility for them. Usually scientific research is teamwork, and it would be a distortion of the true facts to make it look as if one or two individuals could have done all the work. In the case of the ferrites, we should not only focus on Snoek but also take into account the contribution made by Verwey and De Boer.

Ferrite Research in the Nat.Lab. Prior to 1936

In 1909 Hilpert of the Technische Hochschule in Berlin published an article on the possibilities opened up by using ferrites for magnet cores. He also acquired a patent for this work on using ferrites for magnetic cores. Hilpert had found that ferrites were particularly useful where high frequencies were concerned. It is not clear whether his work was implemented at that time. In Japan in 1930, Kato and Takei embarked on their own ferrite research. Not only the properties, but also the structures of ferrites were investigated. It appeared that ferrites had a spinel structure. Spinel is the mineral $\text{MgO} \cdot \text{Al}_2\text{O}_3$. The Japanese researchers were also able to acquire patents for their work. In 1940 Snoek got hold of a piece of the ferrite material that the Japanese had developed. He did some X-ray and chemical analyses, and after that ferrite research really started to take off in the Nat.Lab. During the 1940s the Nat.Lab. was the most prominent lab that worked on ferrites. Outside the Nat.Lab., only a few others, like Adelsköld and Hoffman, made contributions of any significance.

Before that, incidental studies into magnetic phenomena had been done for practical purposes. Philips' chromium foundry had been producing magnets of hardened steel since 1924. That material was used for magnets in loudspeakers. In 1931 the chromium-iron deliveries stopped, and a new product was sought to keep the factory active.¹³ After 1932 magnet steel for permanent magnets was not only produced in Eindhoven, but also in Blackburn (UK). Sometimes practical problems had to be solved, such as problems relating to the production of magnet cores for transformers and loudspeakers. In particular, there was the problem of Eddy currents causing serious energy losses with high frequencies (such as radio frequencies).

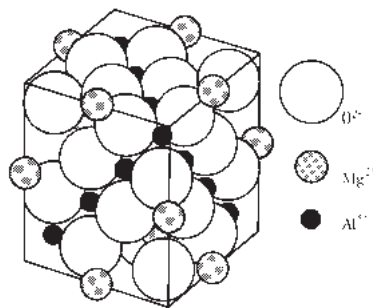


Figure 13. Unit cell of the spinel MgAl_2O_4 . The oxygen ions are much larger than the metal ions. The Mg ions are surrounded by four oxygen ions, the Al ions by six (from Garratt 1976, Vol. 2, p. 217).

According to Verwey, who joined the lab in 1934, Holst had identified this as a key problem. Practical ways of preventing Eddy currents forming in metals involved splitting up the core into slices of metal. Eddy currents could not run between the slices. Another option was powder cores. Here, too, the fragmented structure of the core prevented Eddy current formation. What is evident though is that these solutions complicated the production process. The core should rather be a homogenous material. This was what made highly resistant non-metallic magnetic material so interesting, because with such material Eddy currents were small. Already before he had obtained the Japanese material, Snoek had studied the material mentioned in Hilpert's patents. He was not satisfied with it because there were still substantial energy losses even though the Eddy currents were low. Snoek experimented with combined ferrites, like magnesium manganese ferrite, but his findings were not patentable because they fell under Hilpert's patents. Gradually, more scientists became involved in the ferrite research project. In 1934 Verwey started a study into recrystallisation and molecule grids. Van Bruggen, an assistant, did the practical work, both for Snoek and Verwey. The spinel structure was found to be of importance to energy losses, but the work did not yield usable results. Snoek and Verwey had rather different approaches. This sometimes gave rise to tension between the two, not unlike the type of tension seen between Elenbaas and Bol. Verwey tended to focus on theoretical considerations. Snoek, however, had a more practical attitude. He worked primarily on Pupin coils and not on magnetic phenomena as such. Verwey was sometimes reproachful towards Snoek because Snoek would not show enough interest in Verwey's crystallographic discoveries. Besides that, Verwey doubted if Snoek's claim that energy losses had been measured by the radio research colleagues was true, because his impression was that Snoek irritated those colleagues because of his stubborn behaviour. The articles both men published in the *Philips Technical Review* reflected their two different approaches. In 1935 Van Arkel, Verwey and Van Bruggen published two articles on the phase system of ferrites; in the same year Snoek published an article on the magnetic and electrical properties of single ferrites, which mainly focused on the homogeneity of the material. In addition, in 1937 Snoek himself expressed his opinion that their subject could be 'approached from different angles: if one takes the concepts of crystal structure and (ferro) magnetism in their most principle sense, then one could investigate how the fundamental magnetic properties depend on the properties of single crystals. Such a consideration, important as it may be for our theoretical insight, would teach us nothing about the size and shape of the magnetisation curve in dependence of factors such as heat treatment, purity, grain size and crystal orientation. In particular, it is the latter sort of considerations that are of interest in practice.' This remark fits in well with the suggestion that Snoek had adopted a very practical approach.

The effort put into ferrite research diminished in the years 1934 and 1935, probably due to the lack of success and maybe also because of the tensions between Snoek and Verwey. In 1937 the research was resumed again. In 1940, the analyses of the Japanese material gave the ferrite research in the Nat.Lab. a new impetus.

The Road that Led to Ferroxcube

In November 1936 a meeting was held about the progress being made in ferrite research. Snoek presented his work on materials for coil cores. Rinia who was involved in radio research also took part in the discussions. His work done in 1933 on the properties of high-frequency powder core coils for radio probably had been one of the reasons for starting the ferrite research. He had concluded that the Ferrocart coils that had been used until then were suitable for frequencies of up to about 400 kHz, but not for higher frequencies. Ferrocart coil cores consisted of a mixture of iron powder and a thickening material, built up in thin layers separated by paper sheets. Six was involved in telephone cable research. Then there was Meerkamp van Embden, who during the 1930-1939 period worked at the chemical lab, at the Nat.Lab. and in the magnet factory. He presented information about production problems. The meeting, and notably the presence of Meerkamp van Embden, illustrates the close relationship between the lab and production work in the factories. Meerkamp van Embden had himself explicitly expressed the need for close co-operation between the factory and the lab.¹⁴ From the minutes of this meeting Hoitzing concludes that from then on, ferrite research had become a separate entity within the total research programme.

In 1937 Snoek discovered that iron did not show magnetic after-effects when all the nitrogen and oxygen had been removed from the material. The after-effect was known to be responsible for a substantial part of the energy losses in Pupin coils, but a more important impetus came when Snoek got hold of Japanese ferrite material. Articles written by Takei in 1937 and in 1939 showed that Japanese ferrite research was quite advanced, but that up until then the Japanese industry had not expressed much interest in it. The fact that the Japanese had proved that it was possible to produce ferrite with modest losses gave Snoek added impetus to continue research into these sorts of materials. The analyses carried out on the Japanese ferrite material had made him aware of the relationship between the way in which the material was cooled and sintered and its oxygen absorption. Druyvesteijn later recalled a remark made by Snoek: 'I actually never noticed if we did the glowing by reduction or oxidation.'¹⁵ Hoitzing shows that Snoek could have been aware of the relevance of this difference from Verwey's work, but as we have seen, Snoek did not seem generally to be very interested in Verwey's theoretical work. From then on,

Snoek's work would focus on the preparation of the ferrites. In that respect, the research was of a different nature than the Japanese research that had been more concerned with the characterisation of the material. Snoek's aim was now to search for purity. The purity of the material before it was ground and sintered proved to be important for the magnetic properties of the ferrites in conjunction with the spinel structure of the material. In 1941 Snoek invented a procedure for preparing a ceramic ferrite material which involved baking a very finely divided mixture at a low temperature and then absorbing oxygen at low temperatures. The procedure (not the resulting material) was patented. The method was applied to copper zinc ferrite, and the trade name for the resulting ferrite became: Ferroxcube. The name consists of 'fer' for iron, 'ox' for oxide and 'cube' denoting the cubic crystal structure of the ceramic. The name was used for different types of ferrites: Ferroxcube I was copper zinc ferrite, Ferroxcube II was magnesium zinc ferrite, Ferroxcube III was manganese zinc ferrite, Ferroxcube IV was nickel zinc ferrite, and Ferroxcube V was the manganese ferrite for transformer cores. This list illustrates the versatility of the Ferroxcube: it could be adapted to suit the needs of the application. The outcome, however, remained a compromise: in spinel structure materials, high magnetic permeability ('magnetisability') is not compatible with low energy losses. The compromise, though, was good enough to result in very successful industrial Ferroxcube applications. Improvements in material properties were achieved by experimenting with other oxides. During WWII, Six co-operated with Snoek to use the outcomes of the Ferroxcube research for his work on telephone cables. Pilot Ferroxcube production was set up in 1941 in the ceramics department of the glass factory. Van Bruggen, Snoek's assistant, was transferred to that department. Snoek and Six would go to the factory to lecture on the properties of the ferrites from time to time. A third party involved in all of this was the Electro Technical Factory that carried out measurements for the factory.¹⁶ In the apparatus factory the materials were used in products.

During the war, the practical relevance of the work had to be kept hidden from the Germans. In the reports written for the Verwalter, the scientists neutrally wrote that ferrite research was about 'materials for powder cores'. No mention was made of the importance of the material for achieving low energy losses. As we will see in the first *Intermezzo*, fake reports were common during the war, and we can see that the same thing was happening here, too. Yet, work continued as if there were no Germans around. The war also provided the opportunity to reflect more on certain fundamental theories, such as the solid-state physics theory, that had been developed since 1930 but which, up until the war, had not had much impact on ferrite research, apart from the case of Verwey's and De Boer's work (but as we saw, Snoek did not make much use of that). According to Hoitzing, by the end of the war ferrite research had changed in the sense

that the material itself rather than its preparation had become the focus of study. According to Casimir, it was Holst who in particular began to emphasise the potential of solid-state physics for Nat.Lab. research (Casimir even assumed that Holst had invited him to join the lab because of his expertise in that area as a theoretical physicist).¹⁷

Further Research after WWII

After the end of the war, it was Went, Gorter and Wijn who pursued ferrite research. Meanwhile, Snoek concentrated on theoretical explanations for the losses. In 1950 he moved to the laboratory of a competing firm in the USA, known as Horizon Ltd. in Cleveland, Ohio. Snoek did not work there for long; he died within months in a car accident. In the same year a French scientist, Néel, published a theoretical explanation for the magnetism in ferrites that was based on the division of ions in spinel crystals. Snoek had been close to finding such an explanation, but his interests had never been very theory oriented, so he had not put a great deal of effort into elaborating his ideas on that. In 1970 Néel was awarded the Nobel Prize for his theory. If ever the Nat.Lab. was close to having a Nobel Prize winner in its ranks, it was probably here. We must not, however, forget that Verwey's theoretical work was just as important as Snoek's more practical work.

From 1948 onwards it was Went who led the ferrite research. Went had already worked with Snoek on manganese zinc ferrite. Until then, the main focus had been on ferrites for the cores of electromagnets. Now a shift was being made towards ferrites for permanent magnets. Before 1950 Ticonal was used for such magnets. Ticonal was an alloy of cobalt, titanium and copper, that had emerged from Nat.Lab. experiments. When, in 1950, cobalt and nickel became scarce because of the Korean crisis, a new material was developed that was given the name 'Ferroxdure'. Ferroxdure was a compound that consisted mainly of ferric oxide and barium oxide. When it came to the invention of Ferroxdure, serendipity had played quite a role. Since 1944 Jonker and Van Santen had been working on materials with the same crystal structure as Perovskiet (CaTiO_3). These materials had various interesting properties, like semiconductivity. One of these materials was hexagonal lanthanum ferrite. An assistant, Bannink, had made a mistake during the preparation of this material and had unexpectedly ended up with a magnetic material. Jonker guessed that hexagonal barium ferrite had formed in the process and this was confirmed by an X-ray analysis. The preparation was taken to the magnetic research group, and Gorter, who had already planned to work on hexagonal ferrites, found it to be very suitable for permanent magnets. It was even cheaper than the current compounds that were being used for permanent magnets.

For both Ferroxcube and Ferroxdure, a sound patent position was

established. It was mainly after WWII that this patent position was exploited. The importance of establishing a good patent position can be illustrated by the case of ferrites. The deployment of these ceramic magnetic materials became widespread and they were used in a great variety of applications, not only for the original one, namely in loudspeaker coil cores and telephone cable Pupin coils. For example, ferrites were used in electromotor magnets, dynamos, focussing magnets, magnets for oil filters and cyclotron magnets. One more type of ferrite was developed in 1955: Ferroxlana. This material was the outcome of a study into a ferrite suitable for higher cut-off frequencies without a specific product need. It did not achieve the widespread application that the use of Ferroxcube and Ferroxidure achieved. By this time other laboratories, outside of Philips, started to make substantial contributions to ferrite research.

The American Bell AT&T company was one of the companies that became interested in the Philips Ferroxcube patents (in particular for use in cores for carrier wave telephony). Years later, the licence contract agreed to in 1947 enabled Philips to apply Bell's transistor knowledge at reduced cost. So even indirectly, the ferrite patents were a great asset to the company.

The case of the ferrites is an example of materials research conducted in the Nat.Lab. The basic reasons for it were practical: the need for magnetic material for loudspeakers and Pupin coils. Ultimately, the insight yielded by the research had a much wider impact. Several new ceramic materials were developed that were applied to a variety of products, and not only by Philips, but also by other companies. As in the case of gas discharges, this knowledge was the result of a combination of theoretical considerations and practical experimentation and problem solving. Sometimes two approaches followed by different scientists gave rise to certain tensions between people, but in the end both had a contribution to make.

The case study also illustrates the connection between factory and research activities that we see so often in this period of the Nat.Lab.'s history.

4.4 Holst's Rules Reconsidered

After having examined three examples of research practice in the Holst period we now return to the general characteristics of the Nat.Lab.'s significance to the Philips company during the first period of its history (1914-1946, and including the early years).

Casimir, one of Holst's successors, once summarised Holst's research management according to what he called the 'Ten rules of Holst'. A critical reflection on these rules will help us to round off Part I with a sum-

mary of the Nat.Lab.'s role within the Philips company in the 1923-1946 period. The rules mentioned are the following:¹⁸

1. Engage competent scientists, if possible young ones, yet with academic research experience.
2. Do not pay too much attention to the details of their previous experience.
3. Give them a good deal of freedom and a good deal of leeway to their particular preferences.
4. Let them publish and take part in international scientific activities.
5. Steer a middle course between individualism and strict regimentation; base authority on real competence; in case of doubt, prefer anarchy.
6. Do not split up a laboratory according to different disciplines, but create multidisciplinary teams.
7. Give the research laboratory independence in choice of subjects but see to it that leaders and staff are thoroughly aware of their responsibilities for the future of the company.
8. Do not run the research laboratory on budgets per project and never allow product divisions budgetary control over research projects.
9. Encourage transfer of senior people from the research laboratory to the development laboratories of product divisions.
10. In choosing research projects, be guided not only by market possibilities but also by the state of development of academic science.

When we read these rules after having considered the goals, means, culture and structure of the Nat.Lab. and the way in which the laboratory searched for a proper way to make an impact on the company, it seems that these rules do not so much describe the lab's practice in Holst's period as reflect the emphasis on creating an academic culture for an industrial research lab. Indeed, this emphasis was important to Holst since he wanted to make the Nat.Lab. an attractive place in which to work for excellent scientists.

The list of rules fails to do justice to the fact that Holst at the same time always paid attention to the company's interests. The choice of research topics and the work done by the scientists was not entirely free but was highly influenced by the company's product portfolio decisions. The research lab enabled the company to realise the product portfolio diversification decided upon at the company's management level, where Holst often proved to be conservative when it came to integrating entirely new products. The tension between having an academic orientation and maintaining an awareness of the company's needs was one of the dilemmas that Holst had to solve when managing the laboratory. Indeed, he had little on which to base his ideas, because research laboratories in the electrical industry (apart from a few laboratories in the USA) were a rather new phenomenon at that time. He was faced with a similar lack of refer-

ences for comparison when solving another dilemma, namely that of the freedom given to top researchers and deciding how to create control mechanisms in order to make sure that the lab would amount to more than a collection of contributions made by creative individuals. This problem became more important when the lab saw substantial growth in the years between 1923 and 1946, as it went from just two scientists to a research organisation of about 300 employees.

One could query whether perhaps the rules were not just Casimir's projection of his own ideals onto Holst. In Part II we will see that some of the rules certainly did apply to the period in which he was in charge of the lab. But there is an alternative explanation for the phrasing of the 'Holst rules'. We have that seen in the 1923-1946 period, a number of transitions occurred that heralded the coming of a new period. It was during that time that Casimir joined the Nat.Lab. and became acquainted with Holst as a research manager. Perhaps this is what makes the 'Holst rules' a good transition to the next period, the years between 1946 and 1970. On the one hand, the rules give an impression of what the situation was like when Holst handed over management of the Nat.Lab. to his three successors, Casimir, Verwey and Rinia. On the other hand, the same rules illustrate the atmosphere of the lab in the new era after the WWII intermezzo.

Intermezzo I

The German Occupation and the Transition Years (1940-1946)

In part I the history of the Nat.Lab. in the 1923-1946 period has been described. In this Intermezzo the 1940-1946 period will be detailed. The Second World War and the occupation of the Netherlands by the Germans – in the southern part of the country from 1940 to 1944 – naturally created an unusual situation for the Nat.Lab. which for that reason is described separately here below. In the 1944-1946 period the Philips company structure was formalised. This would change the Nat.Lab.'s position. These years constituted the prelude to the next period of the Nat.Lab.'s history, 1946-1972, which will be described in Part II.

Already in 1934 there was concern at Philips that war might break out in Europe, even though there was no concrete threat of that yet happening. A draft plan for the evacuation of personnel and equipment was submitted to the Minister of Defence in 1935.¹ The ministry was only prepared to support this plan if Philips promised to initiate a special company that would work for the army. After some hesitation on Philips' side, the two parties arrived at an agreement in 1936. The evacuation plan mainly concerned production facilities, the patent department and the company's administration. In 1939, the company decided to transfer a part of its production to the city of Dordrecht, near Rotterdam, and another part of its production to Blackburn in England. Philips already had a factory building there. In the same year there was also rising concern about the research. A plan was made to get ready a number of empty Delft Polytechnic buildings that could be used by the Nat.Lab. in the event of a war and to move some of the activities to London. The development of new military instruments and equipment (such as infrared telescopes, anti-aircraft detectors, radar, high-pressure mercury searchlights, and short-range TV transmitters) could take place in Delft. In London a broader programme could be pursued, comprising both military and non-military research.² Holst wanted to have clarification about what sort of research the ministry expected to be constructed before he started equipping these premises. At that time the ministry was unable to give that sort of clarity, but later on the chairman of the Commissie voor Fysische Strijdmiddelen ('Committee for physical weapons'; a committee that worked for the Ministry of Defence), a Delft professor by the name of G.J. Elias, came

up with a list of ten items and passed it on to Holst. Later, by 1939, the threat of war appeared to be realistic: in September of that year the German army invaded Poland. On May 9, 1940 Otten received the message that it was strongly suspected that later that same day German troops would invade the Netherlands. Immediately, evacuation plans were activated, and Nat.Lab. equipment was transported elsewhere. However, the invasion took place so fast so that it was impossible to execute all the plans. Equipment could not be transported to its destination, and so had to be returned. Only the company's directorate escaped to England, with the exception of F.J. (Frits) Philips, who decided to return to Eindhoven. Frits Philips had been made managing director of the company, together with H.F. van Walsem and O.M.E. Loupart, when his father, Anton Philips, had retired on July 6, 1939.³ His official position became unclear when the other managing directors left, but in practice he had to be in charge of the company's Dutch factories. At the Nat.Lab. the start of the occupation created a strange situation: a lot of equipment had already been moved to Delft before the war started, and during an attack by the Germans in May 1940, people were unable to reach Delft and so had to return to Eindhoven. The equipment in Delft thus had to be moved back to Eindhoven.

That was how the period of occupation by the Germans began. As Blanken remarks, in terms of historical research this is a problematic period, because of the scarcity of reliable documents revealing what happened at that time. This also holds for the Nat.Lab. activities.

The Germans realised that an electrotechnical company like Philips could be quite useful for them. Therefore, *Verwalter*⁴ (Governors) were appointed to ensure that German interests were taken into account by the Philips company. The names of these *Verwalter* were Bormann and Merkel. The Germans required a high production of transmitter and receiver equipment for their own army, and soon irritation arose because the company did not meet the delivery obligations. In 1942 Dr. L. Nolte was sent to Eindhoven to supervise the production. The regime then became noticeably stricter. On December 6, 1942, the allied airforces bombed a number of Philips factories to prevent further contribution to the German army's needs. In 1943 Dr. H. Rohrer, a former AEG director, was sent to Eindhoven by the Germans. Rohrer took steps to reorganise the production process, and due to the weakened position of the directorate, he was able to implement these measures in a short time. In 1944 he was joined by Rzehulka, a Telefunken engineer. The company's behaviour in the period of German occupation showed tension between seemingly adapting to German requirements while at the same time sabotaging all their efforts to profit from the company's capacities. The same sort of tension was reflected in the Nat.Lab.'s position during the 1940-1944 period.

In the 1940-1944 period the lab population grew from 108 to 151 scientists.⁵ At the same time, the research programme was reduced (some topics had to be dropped because of the danger of being put to military use by the Germans). The reason for the large intake of new scientists, despite the reduction in the size of the research programme, was in many cases linked to their personal situation. The company, and for scientists in particular the Nat.Lab., served as a refuge for people who were in danger of being arrested by the Germans, and possibly deported to Germany. Thus, several scientists, some of whom were later to become directors, like Casimir and Vink, started working at the Nat.Lab. Vink later revealed that the Germans had made a list of *Anstifter* and *Mitläufer* (instigators and followers) at Leyden University. Somehow within Philips it was known that Van Arkel, his professor in Leyden, was thought to be an *Anstifter* and Vink a *Mitläufer*. Both Van Arkel and Vink were invited to join the Nat.Lab. to escape from possible arrest by the Germans⁶. Likewise Casimir was invited by Holst, through his brothers-in-law Köhler and Verwey, who already worked at the Nat.Lab, to join the company⁷.

Because of the war a number of changes took place at the Nat.Lab. Yet there was also a lot of work that continued almost as if there was not a war going on.⁸ Reports had to be written in German for the *Verwalter*, but other than that, for many employees, there was not much to keep them from doing their normal work. Often the scientists wrote fake reports that did not give a realistic impression of what was really going on. Verff even stimulated this.⁹ One time, he and Holst had created a wonderful and complex 'fake' plan to prove to the Germans that the research that they had commissioned would totally disrupt the Nat.Lab.'s organisation and would not therefore be feasible.¹⁰

There are nice stories that show that sometimes the Germans took those reports seriously, such as in the case of the Stirling engine, where the Germans were made to believe that a new type of fuel was being developed. Sometimes the scientists even found their written nonsense seriously quoted in German scientific literature much later on.¹¹

Of course certain research could not be continued, at least not overtly. In a survey conducted by J.M. Verff, written about a year after the liberation¹², an impression is given of the total research programme continued during the occupation. All research in the following field was stopped:

- obstacle detecting,
- infrared viewing,
- high frequency hardening,
- mirror optics,
- television.

With some issues, such as the infrared viewer, it is obvious what the potential military use by the Germans could be; with other subjects the potential applications were less evident.

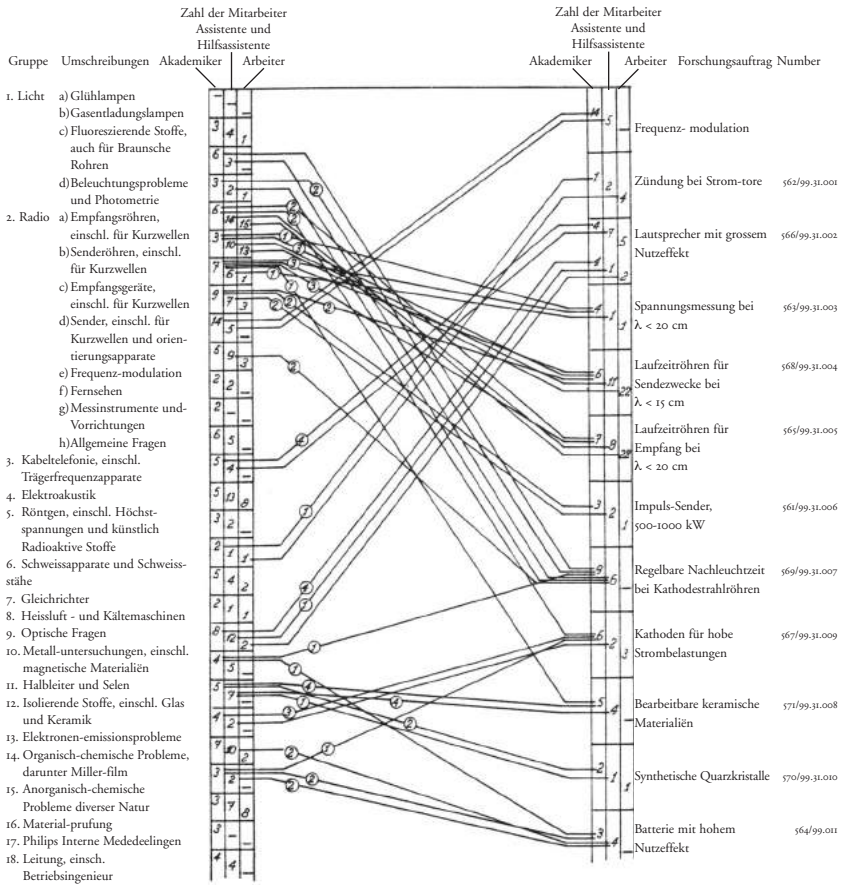


Figure 14. The Verff scheme that was invented to make the Germans believe that the organisational changes they demanded would totally disrupt the Nat.Lab. organisation. The left-hand side shows the existing division in groups, the right-hand side shows the situation as desired by the Germans. The lines show the complicated transfer process that would be needed (from Garratt 1976, Vol. 1, 310).

Research was secretly continued in the fields of:

- hot gas engines,
- cellophane film,
- facsimile transmission,
- magnetic materials,
- the cyclotron.

A lot of research continued as normal, such as in the fields of:

- radio,
- electro-acoustics,
- measurement equipment,
- telephone devices,
- welding rods,
- selenium rectifiers.

What was commissioned by the Germans but not accepted by the lab management or sabotaged by the scientists, were studies in the fields of:

- frequency modulation,
- photo cell amplification,
- throat microphones,
- ultra short wave receivers,
- tubes for accelerating ions,
- service aerials,
- aerial amplification.

Only by the end of the occupation period had some progress in the last-mentioned category of fields finally been made, but the liberation came just in time to prevent preliminary reports falling into German hands.

In the more fundamental fields of research that were not directly related to concrete products – such as colloid chemistry research being done by Dr. J.H. de Boer the circumstances were, in a way, even more favourable than before the occupation. Garratt is probably right in suggesting that this research, the practical value of which was very unclear, would not have been possible in peace time. Perhaps the occupation also gave people the opportunity to freely consider the possible values of applying the emerging field of solid-state physics to the Nat.Lab. research. The colloquia as a resource of new scientific thinking could also be continued, but of course without the input of the famous foreign scientists who had visited the Nat.Lab. before WWII. In those days Dutch scientists of good repute, such as Kronig, Bremmer and Casimir, gave presentations.¹³ Soon after the war the *Philips Technisch Tijdschrift* (*Philips Technical Review*) started being published again, and from the articles that came out, it becomes evident that a lot of new knowledge had been gained during the years of German occupation.

The city of Eindhoven was liberated on September 18, 1944. At that time the northern part of the country was still occupied and would remain so until May of the next year. In fact, the winter of 1944/45, known as the ‘hunger winter’, was one of the most difficult periods of the war in that part of the Netherlands. For Philips and for the Nat.Lab., the period of recovery started in September 1944. Shortly after liberation Holst gave a presentation for the Nat.Lab. employees to show them that the company

was still very much alive and that the challenge facing the Nat.Lab. was to help the company to recover from the war.¹⁴ It was especially difficult to attract young new scientists, as the universities with physics, chemistry and engineering faculties were all in the – still occupied – northern part of the country. Therefore, a request was made to the government – still seated in London – to have a temporary academy set up in Eindhoven that could educate new scientists and engineers for the Nat.Lab. The government gave permission to carry out this plan, and a committee consisting of Van der Pol, Bakker, Casimir, Slooff and Hamaker¹⁵ was set up to make the necessary arrangements. All the administration work was done in a restaurant, known as *Het Rozeknopje* (the Rosebud), where Casimir registered the students. In a number of buildings in the Eindhoven region, lectures were given to students by a staff of about seventy scientists.¹⁶ The Tijdelijke Academie (Temporary Academy) was continued when the northern part of the country was liberated and the government returned to The Hague, but only for a few more months. By the end of 1945, the other universities had resumed their programmes, and so the need for the Tijdelijke Academie ended, even before it had the chance to develop into something substantial.

The following years would bring some important changes to the Nat.Lab. The 1944-1946 period can be seen as a period of transition. There are a number of reasons for this.

In the first place there was Holst's retirement in June 1946. By then, he had been in charge of the Nat.Lab. for over thirty years. His retirement did not immediately precipitate a big change in terms of his role at directors' meetings. For some years he stayed on as an advisor. That made the 1946-1954 period a time of transition in terms of his role. In 1947 he advised the Philips directorate on the possible future role of the Nat.Lab. According to him, the main areas in which the Nat.Lab. could contribute to the recovery and further extension of the company were: improvement of light sources, application of frequency modulation (FM) in radio equipment, new types of electron tubes, television, the opto-mechanical registration of images and sound, the hot gas engine, and the new field of semiconductors. A year later, Holst suggested focusing on new products that could be manufactured in the already existing factories. He expected that the turnover in lighting and radio would decrease in the future and that the development of television would go at a slower pace than had originally been expected. Using existing factories would give the company the chance to easily compensate possible reductions in the existing activities so that new ones could be implemented quickly.¹⁷ More concretely, Holst thought of radar equipment, measurement equipment, and products for telecommunication. These aspects were also connected to research at the Nat.Lab.

Although for the Nat.Lab. it was of course quite a change to be without the person who had been in charge for so many years, what was probably the most important thing to influence the Nat.Lab.'s position in the company was the effect that the formalisation of the company's structure had in the post-war years. In 1946 it was decided that a Board of Management should be established. Until then, the principle of having only one person to lead the company had been abided by (for many years Anton Philips had been that person). The first Board consisted of a presidium (in which Frans Otten was the chairman, Frits Philips the vice-chairman, and Loupart and Van Walsem were the presidium members) and five members (De Vries, Dijksterhuis, Tromp, Van den Berg and Guépin).¹⁸

A further formalisation of the company's structure was the definition of eight Product Divisions.¹⁹ All the factories were subsumed under the new Product Divisions of Lighting, Electron Tubes, Apparatuses, Telecommunications, X-ray and Medical Equipment, Electro-acoustics, Products for Industrial Applications, Glass and Ceramic Products, and Related Businesses.²⁰ In 1948 another Product Division was started up, the Pharmaceutical and Chemical Products ('Duphar') divisions and in 1952 the Product Division for Industrial Components and Materials took over the ceramics activities of the Glass and Ceramic Products Division.²¹ The formalisation of the company's structure occurred in an atmosphere of 'economical patriotism' (this term is used by Blanken²²), and there was an awareness of the need to revise the political and social structure of Dutch society during the post-WWII years. The Philips company wished to have an important role in this restructuring.

In the past, discussions about new products had taken place in the company's directorate meetings, in which the factories and the Nat.Lab. also participated. Now these discussions gradually moved to the PD directorate meetings. The Nat.Lab. now had to discuss matters with each of the PDs, all of which had their own specific areas of technical expertise. We will see how this change was to influence the contacts between the Nat.Lab. and the factories and the factory laboratories.

The general feeling in the USA that basic sciences could play an important part in the post-war development of industry also influenced the Nat.Lab. In the USA, Vannevar Bush's famous 1945 report 'Science – The Endless Frontier' had promoted 'basic' or 'pure' research (i.e. research aimed at understanding phenomena, without perspectives on concrete products) as the factor that WWII had shown to be the driving force behind technological development. Although almost none of his concrete recommendations were realised, the rhetoric of the report certainly had an impact on the basic science expectations both of politicians and industrialists.²³ Basic science, according to the Bush report, would almost certain-

ly lead to industrial applications. As we will see in the description of the next period of the Nat.Lab.'s history, this philosophy was seen as a reason for making basic science a core task. Basic research yielded an opportunity for the Nat.Lab. to fulfil a role that could be different from the role of the PD development laboratories. In Nat.Lab. management debates there was a continuous careful scanning of new scientific developments to see if new basic knowledge could be relevant to the Nat.Lab. research programme.

When Holst retired, he decided to split up his responsibilities to form three new areas. Roughly speaking, these fields were physics, chemistry and (electrical and mechanical) engineering. For each of these three areas, he had found scientists to take over the field from him, and they were Casimir, Verwey and Rinia. Of these three, Casimir became the most influential successor. The careful scanning of the scientific developments, mentioned earlier, fitted well into his particular background. Casimir was born in The Hague in 1909. He had studied physics in Leyden and in Copenhagen (with Niels Bohr). He had been involved in the development of quantum mechanics and was well acquainted with the important scientists in that field such as Bohr, Pauli and Ehrenfest. After having finished his dissertation, Casimir became Ehrenfest's assistant at Leyden University. He also worked in Berlin and Zurich for some time. His own most important contribution to quantum mechanics was the identification of the existence of a force of interaction between two plates that conducted perfectly, for which he set up an equation in 1948, which was named the Casimir effect, after him.²⁴ In 1942 he joined the Philips Natuurkundig Laboratorium. Casimir was the first person to represent the lab on the company's Board of Management. Of course no one person could be said to enshrine the overall characteristics of a period, but the changed circumstances and its consequences for the functioning of the Nat.Lab. within the company suited the person of Casimir well, in much the same way that Holst with his combination of being practical and interested in getting to know the phenomena underlying products was the right man at the right time.

PART II

**An Autonomous Lab alongside Autonomous Product Divisions
(1946-1972)**

5. A Research Organisation alongside Autonomous Product Divisions

In the *Intermezzo*, the changes in the Philips company as a backdrop to the functioning of the Nat.Lab. were discussed. In Part II, we will see how the Nat.Lab., as a professional organisation responded to these changes. In other words, we will consider how the Nat.Lab. adapted its goals, use of means, culture and structure as well as its relationships with the other organisations within the company (the Board of Management and the PDs) to this changed context. First of all, though, we will consider the wider context of the economic, social and scientific environment of the Philips company and its Nat.Lab. during the 1950s and 1960s.

5.1 The Economic, Social and Scientific Context in the 1950s and 1960s

By about 1950 the damage caused by WWII had to a large extent been restored. From then on, the Netherlands enjoyed a period of economic growth that was to last from 1951 to 1973.¹ This coincided with favourable economic developments worldwide. World trade increased after WWII, and entrepreneurs increased investments. In the Netherlands the government played an active part by adopting a controlled income policy and issuing a number of industrialisation memoranda, the first of which came out in 1949 and involved an investment in the Dutch industry of NGL 5.7 billion.² In the first post-WWII years, consumers were prepared to do without large salary increases. From 1950 onwards, however, consumption increased rapidly, which can be seen from, for example, the increase in energy use for household purposes. From 1954 onwards welfare allowances were included in the government's income policy: not only inflation but also labour productivity increases were included in the calculation of salaries.

Philips' growth rate, which had temporarily decreased, started to rise again. In the 1950s the number of employees doubled to approximately 211,000 people, 75,000 of whom worked in the Netherlands. During the next decade this number further increased to 359,000 people in 1970. The company's financial turnover grew by 30% in 1948. This growth then decreased, dropping to about 12-15% per year in the 1960s. The contribu-

tion that television made to the company's turnover increased from 6% in 1950 to about 30% in 1960. Exports also increased substantially (for instance doubling between 1957 and 1960). In 1947 the net profit was 4.2% of the turnover. In the 1950s it was about 6%. In the mid-1960s it started to decline (by 1970 it was only 3%), and this heralded the coming of a new, more difficult period.

The company's product range extended further in the 1950s and 1960s. The Product Divisions (PDs) brought out many new products. In a 1970 brochure entitled 'Facts about Philips', the 1950s and 1960s were characterised as a period of 'unprecedented expansion in the Philips product range'. Before WWII this expansion was mainly in the area of components and consumer products, but after WWII professional products were to become increasingly important. Activities in the professional telecommunications sector were extended, and activities in the defence sector were initiated (e.g. with the taking over of the Holland Signaal Fabrieken) and in the 1960s, computer activities started. In 1962 a new Product Division was established in this connection. The Annual Reports for the years 1947-1972 show that in nearly all areas of business there were substantial increases in turnover.

For each of the countries in which Philips was active, a National Organisation (NO) was responsible for deciding what products would be sold in that particular country. The NOs, like the PDs, came about as a result of the process of formalising the company structure in the first years after WWII.

The report 'Science, the Endless Frontier'³ by Vannevar Bush (see Intermezzo I) was based on the expectation that basic research would almost 'automatically' lead to important technological progression. This expectation was justified by pointing to the progress that had been made in WWII against diseases. The progress was said to be based on new scientific knowledge. 'Basic' research was characterised as 'being performed without thought of practical ends, leading to general knowledge and understanding of nature and its laws'. For the Bell company the report was a reason for enhancing the role of research in the field of solid-state physics, and this decision was certainly a factor in the invention of the transistor in the Bell Labs.⁴ It was not only in the Bell Labs that efforts in 'basic' science grew. The total amount of money spent on basic research in the USA more than tripled in the 1955-1966 period. The total amount of R&D expenditure nearly doubled. From 1966, the growth stopped for about a decade, and the belief in 'basic' science waned.⁵

The transistor was one of the first practical applications of new physics to emerge in the first half of the twentieth century. Another benefit to be reaped from this development was the maser, followed by the laser. These

inventions seemed to confirm the view that technology had to do with the application of 'basic' scientific research, because both had been developed on the basis of the outcomes of 'basic' research. The fact that such examples could be used to illustrate the industrial usefulness of 'basic' science was no doubt instrumental in justifying the position of that type of research in industrial labs in the years after WWII.

In the Netherlands two new research organisations were established thus also serving to illustrate the increased interest in this type of research. In 1946 the FOM, i.e. Stichting Fundamenteel Onderzoek der Materie (Foundation for Fundamental Research in Matter), was established, and in 1949 the ZWO, the organisation for Zuiver Wetenschappelijk Onderzoek (Pure Scientific Research), was set up. The idea of 'basic' research influenced discussions in the Nat.Lab. management circles about its own role within the company with its new PD structure.

5.2 A New Task Profile for the Nat.Lab.

After the formalisation of the new PDs in the Philips company, each with its own development lab, the Nat.Lab reconsidered its position with respect to these PDs. in conjunction with the company's Board of Management. The PD development labs were not new. We already came across them in Part I. They had emerged in the 1930s as production-oriented factory labs. Now they were part of the new PDs. Each of the PDs was more and more able to determine its own product policy. Their development labs could operate in accordance with the PD's own ideas, because they were controlled by the PDs themselves. The Nat.Lab.'s budget was allocated directly by the company's Board of Management, and the PDs had no formal say in the Nat.Lab.'s research programme. This made the PDs behave more and more selectively with respect to the product ideas that the Nat.Lab. offered them for their product portfolio. They did not see it as a loss with regard to their own resources when they refused to transfer certain research outputs. This situation had repercussions for the Nat.Lab. In the years 1914-1946 the Nat.Lab. had been a hybrid lab in that it had had different types of laboratory tasks. Part of the research had been aimed at understanding the phenomena underlying the company's existing products (e.g. lamps) in order to improve them. There had also been development research, which had been more directly related to the development of new products. This type of research had provided the company with technologies that had been necessary for realising the desired product diversification. In the case of radio, this type of research had been done in co-operation with the factory labs. Finally, there had been testing and measuring activities, and assistance had been given for production problems in factories. The testing and measuring work had been only carried out in the early years of the Nat.Lab.'s existence (1914-1923). How did this task profile change in the new situation?

In 1947 there was a series of meetings for the company's Board of Management representatives and research representatives. The title of these meetings was: 'Onze laboratoriumplannen' ('Our laboratory plans'). At the first of these meetings, a report produced by Casimir, Rinia and Verwey, the three new directors who had succeeded Holst, was discussed. In this report the authors defended the independent position of research to enable 'fundamental' research to be done. In the document, 'fundamental research' was defined as that which 'aimed at understanding nature'. It was opposed to 'industrial research', which 'in the end aims at making better and new products'. According to the report, a centralised lab alongside of the developmental research within the PDs, would yield an opportunity for broad-based and multidisciplinary expertise resources for the whole company. In the Holst period all the research that took place in the Nat.Lab. somehow related to concrete products. No research occurred that was entirely without envisioned concrete products. In the new era the Nat.Lab. was also to serve as a think-tank for the PDs' long-term product policy by completing research for which no concrete products were yet envisioned, although in the end, of course, new products would emerge from such research in exactly the way the Vannevar Bush report suggested, namely that such research would almost automatically have a high chance of later finding its way into industrial applications. Holst had expressed his agreement with the Vannevar Bush report,⁶ and his opinion was often requested in the first years after his retirement.

This expected Nat.Lab. role was not entirely new, because the company directors (Loupart, Otten and Van Walsum), who were in charge after Anton Philips withdrew from the company's directorate, had already expressed the hope that the Nat.Lab. would fulfil such a role, but in many cases Holst had been rather hesitant in that respect.⁷ In the meetings it was agreed that the organisation of the lab should be disciplinary because a PD-wise division of research would be 'fatal' for its proper functioning.⁸ Probably the fear was that in that case research would be too much centred on short-term PD interests and not enough on long-term-oriented research.⁹

The Board of Management supported this 'fundamental' research idea, but the Board also wanted research to be related to the company's interests. Thus a special CTB, (Comité voor Technisch Beleid; Committee for Technical Policy) was set up, which would serve as a platform for discussing the choice of research topics from the point of view of the 'connection between research-development-manufacturing'. The CTB met at least twelve times in 1947 and 1948.¹⁰ The meetings convened for discussing various research fields' potential for the company. The Board of Management emphasised the need for such a committee by stating that 'in the past years at least 10 million guilders have been spent on radio receiver research without any noticeable changes'. There was a difference in atti-

tude between Holst and Casimir with respect to the relation between research and PD interests. Holst stated that according to him research should only deal with topics for which an industrial turnover of at least a million guilders could be expected. Casimir refused to take that as a restriction, because he expected it would be a barrier to truly innovative research. This illustrates how in those years both Holst and Casimir tried to influence company policy and determine the Nat.Lab.'s role.¹¹

In the 1946-1972 period the debate on the proper role of the Nat.Lab. in relationship to PD development labs continued. This debate largely took place during Corporate¹² Research Conferences (CRCs), a series of meetings for research directors held every other year.¹³ Holst chaired the first of these meetings, in 1948, and was still present at the 1952 CRC.¹⁴ This shows that Holst's involvement continued after his retirement. From 1965 on a more frequent series of directors' meetings, known as Research Directors Conferences¹⁵ (RDCs), was introduced. The CRC and RDC debates show how the ideas about the three types of research tasks for the Nat.Lab. at lab management level changed due to the fact that new PDs had been established, each with its own development labs and its own commercial and technical directors who were able to decide which products would go into production and which would not. What was the perspective on the Nat.Lab.'s task that evolved as a result of the debates in those meetings? To answer this question, the three main types of activities of an industrial research lab that were examined in Chapter 2 (namely long-term oriented 'fundamental' research, development-oriented research, and practical support) will be considered.

Long-term Oriented 'Fundamental' Research

As we saw in Part I, the Nat.Lab. displayed rather 'follower' behaviour with respect to the company's product diversification, not in the least because of Holst's frequent hesitations in the Orco meetings. In the 1946-1972 period the Nat.Lab. started initiating new areas of scientific research that were not directly related to product diversification decided upon by the company, and for which no PD had yet expressed any need. By doing the sort of research that had been promoted in Vannevar Bush's report – research that was entirely focused on gaining new knowledge, and not oriented towards concrete products – for which the Nat.Lab. directorate often used the term 'fundamental', rather than 'basic',¹⁶ the Nat.Lab. would have the unique position of providing the company with entirely new options for product diversification or product improvement.¹⁷

In a number of cases the idea of 'fundamental' (i.e. non-product-oriented and entirely focused on understanding phenomena) research being a spe-

cific Nat.Lab. task was confirmed by the PDs. In 1970 the RGT (Radio, Gramophone and Television) PD sent a report to the Nat.Lab. to express its desires in terms of 'fundamental' research (with later possible applications in the field of electronic parts for RGT equipment).¹⁸ Likewise in 1968 the PIT (Products for Industrial Applications) PD explicitly uttered a need for 'fundamental research' to be done in the Nat.Lab.¹⁹ A third example is to be found in the Elcoma PD, which in a meeting in 1968 urged the Nat.Lab. not to focus too much on short-term development needs, but rather to do research that would ultimately yield new computer memories that would be profitable by, for example, 1975.²⁰

Several examples can be given to illustrate focuses on 'fundamental' research that were not directly related to any existing PD product, as a specific Nat.Lab. long-term-oriented goal. A first example is the material SiC (silicon carbide). SiC was a semiconducting material that was found to be suitable for high-power applications at high temperatures (up to 2000 °C). No PD was, however, interested in this property, because the conditions for which SiC would be an almost unique solution – for example, for electronics systems in rockets – were not found in the application areas in which Philips was active. Research into SiC in the lab also showed that coloured light-emitting diodes (LEDs) could be made with it: red, green and blue, so that in fact the whole colour triangle could be produced. That could be interesting for colour television. But the production of pure SiC crystals was problematic. The chemist W. Knippenberg did a lot of experiments on this. At a certain moment he was able to produce pure crystals, but then the next problem emerged: the fact that SiC grew in more than one crystal type (the problem of the polytypes,²¹ thus causing a variation in bandwidth within the material). Knippenberg became interested in this phenomenon and wanted to find out if it was a thermodynamic phenomenon or a matter of kinetic growth conditions. It appeared to be the latter. Further study into the influence of impurities on crystal growth yielded something totally unexpected: with 1% lanthanum in the gas phase, the crystal growth changed dramatically, going from plates to whiskers.²² And whisker growth would make the material interesting for fibre-enforced materials. The research generated sufficient material for five doctoral theses and about a hundred articles.²³ The focus of the research was on understanding the properties and phenomena surrounding this material. Although no PD was seriously interested,²⁴ the research continued as long as it yielded enough scientific output.

A second example is to be found in the laser research that started at this period, long before any concrete applications within the company were envisioned. A similar long incubation time had held up the use of stereophony and facsimile. In both these cases the lab was involved very early on, but it would be many years before research output would lead to

a new business in the company. With stereophony the Nat.Lab. contribution was characterised as the more 'fundamental' research involving high fidelity and sound quality 'in general' rather than specific devices. For facsimile it was stated: 'the difficulty now is in finding the customer'.²⁵ This reflects what the attitude of the Nat.Lab. was towards the PDs: the Nat.Lab. did 'fundamental' research with the ultimate aim of supplying new (long-term) product options to the PDs that no PD had asked for.

The long-term interest of the Nat.Lab., and connected to that the search for new research fields, did not automatically mean that every possible new research field was entered. Several options for new research lines were discussed at CRCs and RDCs, and rejected. Some examples of this are tissue research in biochemistry/biophysics, plasma physics, nuclear physics (other than instrumentation issues), and cosmology (other than instrumentation issues).²⁶ The last two examples show that Casimir sometimes found ways of remaining informed about a research field even though its relevance to Philips was not (yet) clear. In such cases, the Nat.Lab. could develop experimentation and measurement equipment. Limited research efforts and costs were counterbalanced by small profits. Casimir took the same approach when it came to the area of field theories in quantum mechanics.²⁷ Most of the work on instrumentation for nuclear research was done for the Instituut voor Kern Onderzoek (IKO, the Dutch for Institute for Nuclear Research) in Amsterdam. In 1949 this institute started operating a (synchro-)cyclotron, built by Philips, and Philips researchers frequently worked with this institute.²⁸ Until about 1958 the cyclotron produced radionuclides for Philips-Duphar. This co-operation would last until 1980.²⁹

The interest in exploring physics in totally new fields in some cases went together with a lack of interest in research into better-known phenomena. Thus, the Nat.Lab. was not very interested in doing research into gas discharges, because it was felt that this phenomenon was well understood and no longer suited for 'fundamental' research.³⁰ The fact that for the company gas discharge lamps remained an important business was not an issue. During the 1954 CRC it had already been observed that little research was being done into lamps in the Nat.Lab.³¹ There was a sharp decline in the publications in that area: less than half the number of articles in the *Philips Technical Review* Volumes 11-20 (1949-1958) compared with those to be published in Volumes 1-10 (1936-1948). Most articles dealt with photometrical issues in general, while some dealt with electrical circuits for gas discharge lamps. Some effort had been put into electroluminescence, but the expectation that this would result in a new type of lamp no longer existed in 1954. Lamps were seen as an area where the Nat.Lab. did not have a very important role to play. Most lighting research

was transferred to the new research lab in Aachen (later in this chapter we will discuss the emergence of the foreign Philips research labs). This did not reduce the total strength of the programme, but the Nat.Lab.'s involvement was very much reduced.

Lack of opportunity for 'fundamental' research was also a reason why the Nat.Lab. did little for the Huishoudelijke Apparaten (Domestic Appliances) PD.³²

On several occasions Casimir expressed his concern about the extent to which the Nat.Lab. was able to realise the ambition of doing 'fundamental' research. At the 1960 CRC he explicitly stated that he was worried about the fact that the Nat.Lab. had not been a forerunner in most areas of 'fundamental' research, and in 1962 he also mentioned some specific areas where this had happened: travelling wave tubes, carcinotrons, ferrite memories, super-conducting devices, masers, lasers, digital telephone, data systems.³³ The lab had certainly always made useful contributions, but never a real breakthrough. In a presentation given for researchers in June 1962, he mentioned some undesirable categories of research in the lab and then went on to give details about two such types: the 'advanced classroom experiment', which only repeats what has been found in literature, and the 'model-railroading', which only reproduces a smaller and more primitive analogon of an existing technical realisation.³⁴ One of the reasons for being concerned about the lack of originality of the research had to do with the company's patent position, for which the Nat.Lab., of course, felt a special responsibility. In two management meetings in 1965, this patent position was even referred to as being 'alarming'. Until the end of his time as leader of the CRCs, Casimir came back to the issue of the need to have sufficient original research output. In 1970 we still find a remark made by him to the effect that there was too much short-term research and a lack of inspiration from new academic disciplines.³⁵

The remark about Casimir's concerns should not, however, leave us with the impression that the outcome of 'fundamental' research was poor. It was in particular materials research that yielded some important new scientific insights.

Research into the field of ferrites had started before WWII and was continued after WWII. This research contributed to the knowledge of solid-state chemistry and defect chemistry. The aim of that research was to get to know the relationship between the position of atoms in a crystal and the resulting properties of the material. Also, the preparation of crystals with desired properties was part of this research. F.A. Kröger and H.J. Vink in particular contributed substantially to this field with their thermodynamic studies. They developed a theory that explained the relationship between the concentrations of different imperfections in a crystal

structure and the concentration of foreign ions in the crystal, as well as the composition of the gas phase with which the crystal is in balance while it is being made. In the field of phosphors, too, several new discoveries were made by, among others, E.J.W. Verwey, F.A. Kröger, W. de Groot, A. Bril, W.L. Wanmaker, and G. Blasse. Knowledge on the distances in the phosphor crystal could be used to estimate the efficiency of the luminescence. Several of the (green and blue) phosphors that were developed on the basis of these insights were later used in TL tubes. Metals formed another category of materials in which research was done. J.D. Fast studied wear and tear on iron and steel after welding processes. Fast became known, not in the least, for his publications in which he explained atomic physics and thermodynamics to a wide audience. The work on the capacity for storing hydrogen in LaNi_5 led to a patent, which much later, and quite unexpectedly, was to become the basis for rechargeable metal hydride batteries. Finally, in the metals field, the work of A.R. Miedema and his models for alloy behaviour should be mentioned. These examples of materials research show that this type of research certainly yielded new insights. It is difficult to assess what the overall impact of this type of research on the development of new products has been. The phosphors and rechargeable batteries illustrate that in some cases, there certainly was an impact.

Development-oriented Research

In the previous period, the Nat.Lab. had done development-oriented research to enable the company to realise the desired product diversification. As we saw, the Nat.Lab. did this by gaining a better understanding of the phenomena underlying the functioning of the products. This type of research continued in the 1946-1972 period. With respect to the division of the development work between the Nat.Lab. and the PDs, the Nat.Lab. directorate displayed a changing attitude compared with the previous period. The Nat.Lab. directorate still saw development-oriented research as a task for the Nat.Lab., and this was also expected by the company's Board of Management.³⁶ But the kind of co-operation that we saw in radio development was no longer seen as appropriate. It was rather the case that the Nat.Lab. expected the PD laboratories to take over the development of new products at an early stage.³⁷ When the basic design problems had been solved by gaining a better understanding of the underlying phenomena, the Nat.Lab. would transfer the research output to the PD so that the PD lab would be able to transform the prototype into a manufacturable product. Doing development work in co-operation with a PD was once rejected by Casimir, the argument being that 'that only gives trouble'.³⁸ The transfer of research output, however, in practice appeared to be difficult. Often, the Nat.Lab. management was not confident of the PD lab's ability to fulfil that task. In some cases that suspicion may have

been justifiable.³⁹ But one may question whether that was the real problem. A more probable cause for the problematic transfer of research output was the doubt on the part of the PD about the commercial feasibility of the Nat.Lab.'s product idea. The Nat.Lab. sometimes interpreted this as stubborn unwillingness.⁴⁰ In any case the result was that in a number of cases the Nat.Lab. did even more development work on a product than previously when it worked in co-operation with a factory lab. In the management meetings complaints were then heard to the effect that these (extra) development efforts kept the scientists from doing their real work ('fundamental' research work).

In the CRC and RDC meetings examples of this attitude towards the development work can be seen. In 1948 the research being done into television was discussed in a special meeting. By then it was clear that television was a major business for the company, that would even be bigger than the radio business. Not much effort was yet being put into colour television. According to Rinia, one of Holst's successors, this was a matter of manpower. It was the 'unanimous opinion' of the participants of the discussion meeting that pickup tubes⁴¹ were a 'subject of primary importance in the Philips company'.⁴² Rinia made a plea for a 'fundamental' approach towards television development problems instead of combining existing ideas. The idea was that the Nat.Lab. should make a 'fundamental' contribution to the development work, and the PD should take over the work at an early stage of development.

An area where it was difficult for the Nat.Lab. to find its 'fundamental' contribution to the PDs' development work was in 'systems' research. Since Philips had decided to produce whole radio sets rather than only radio tubes, the company had started to expand from being purely a device-producing company to being a device- and system-producing company. The same happened in the X-ray area: at first only tubes were produced, but later on complete X-ray equipment sets were produced as well. Accordingly, in the mid-1950s the Nat.Lab. shifted its research focus from individual X-ray tubes (Metalix, Rotalix, Oralix and Symmetrix) to X-ray systems.⁴³ Another area where a shift towards complete systems was made was in telephony. In the previous period (Part I) work in this field had already been done in the Nat.Lab. (e.g. the ultrashort wave connection between Eindhoven and Nijmegen). Originally, this too had been a device area: research was done into transmitter tubes for carrier wave telephony. Before WWII this had led to research into various modulation methods: amplitude and frequency modulation (which were also relevant to the areas of radio and later also television), and after WWII the range was extended to include phase modulation, delta modulation, and pulse code modulation. Later on the research extended further into the realm of cir-

circuits for telephony and telephone exchanges. Thus, the whole system of telephony became part of the research programme. The modulation methods also brought the lab into the area of signals and signal processing. This area was related to various products: not only telephony, but also radio, television and the recording of sound and pictures. The general character of signal processing (it was not related to a specific product, but could be applied to a variety of products) somehow typified the Nat.Lab.'s 'fundamental' contribution to product development work. Later, it became closely tied to the IC area. One of the early examples of Nat.Lab. work in that area is the 'bucket-brigade memory', an analogue shift register for television signal processing, produced in MOS technology (see the case study on LOCOS in Chapter 6).⁴⁴

For the Nat.Lab. the intriguing 'fundamental' question was whether something like a general 'systems science' existed. In the 1950s a philosophical debate began on the existence of a General Systems Theory, a theory about the nature of systems that could be applied to any system, natural or artificial.⁴⁵ According to this theory, a system was more than the sum of its parts. The existence of such a 'fundamental' type of theory would constitute an interesting typical Nat.Lab. task. Certain people in the Nat.Lab. had promoted this idea, and so it even became a separate topic of discussion at the 1956 CRC. Although he recognised the relevance of research on complete systems, Casimir denied the existence of such a 'systems science'. On several occasions he joked about this, for example by stating that a toothbrush, a glass and toothpaste should also be seen as a system.⁴⁶ But the systems theory would remain influential in international philosophical debates for several decades to come, and it undeniably also had practical implications for the development of technologies (in particular in the area of cybernetics). Although Casimir was well aware of the limitations of the systems theory, he underestimated the place it would come to occupy in the world of engineering.

Practical Support for PDs

The Nat.Lab. management also changed its views on the research assistance required for practical and production problems in PDs. They tended to regard this as a task that was no longer appropriate for the Nat.Lab., but as with development-oriented research, this task did in practice continue. In general, PDs found that the Nat.Lab. was prepared to help with practical problems that their own labs were unable to solve.⁴⁷ The main reason for assisting the PDs in such cases was because of the good informal contacts that existed at the workflow level. It was not surprising that these contacts were good. In the 1946-1972 period there was a conscious policy to transfer people from the Nat.Lab. to the PDs, and also the knowledge possessed by these people was adopted (this will be further dis-

cussed in section 5.5). Often the people who had been transferred kept in touch with their former colleagues in the Nat.Lab. and thus had easy access to knowledge, in particular about more sophisticated analysis techniques that could be helpful in the event of production problems.

The Evolution of the Nat.Lab. Research Programme

Let us now see how the Nat.Lab. research programme developed in the 1946-1972 period under the influence of these ideas about the goals of the Nat.Lab.'s activities, in terms of the three specified types of research.⁴⁸ In doing so, we must realise that it is not possible to indicate a direct relationship between the changes in the research areas as sketched in the tables below and in the debates about 'fundamental' and development-oriented research as sketched above. 'Fundamental' research was supposed to take place in all areas. This included mechanical research.⁴⁹

Table 1 presents the research programme as it was in 1946.⁵⁰ There were three main areas: materials, tubes and electric systems (radio, television, radar and telephone). It was from 1947/8 onwards that the topics of magnetism, semiconducting materials, medical and biological research, and television increased most sharply. From 1950 onwards the research effort put into control and automation also increased substantially and, to a lesser extent, the topics of luminescence and computers. The areas where efforts decreased were radio, telephone, Stirling cold gas machinery, and photochemistry (particularly where the Philips-Miller system was concerned). The fields of growth in research partially correspond to the main fields of PD turnover growth in the years 1947-1954. Research into magnetic and semiconducting materials, control and automation and television particularly found applications in the PD Elcoma, Telecommunications, ELA and PIT areas, and apart from ELA these were the fastest growing fields. The increase in biological research was not accompanied by an increase in activity on the part of the PD Pharmaceutical and Chemical Products. In this research an analogy between nature and communication and computer technologies was sought. It was not the intention to support the PD.

The result of the changes in the research efforts can be seen in Table 2.

This table shows what the research programme looked like in 1954.⁵¹ By then materials research had become a dominant area in the research programme. In this area there was plenty of opportunity for 'fundamental' research. The research into devices, which had previously mainly centred on tubes, had grown due to the increased attention being paid to transistors.

Table 1. The research programme in 1946

Main research field	Research topics	No. of scientists
1. 'Materials'	Magnetic materials	2
	Dielectric materials	1
	Luminescent materials	5
	Semiconductors	5
	Metals and glass	6
	Measuring methods & instruments	2
		total: 21 (16%)
2. 'Devices'	Vacuum tubes	19 (14%)
3. 'Systems'	Radio	8
	Television	8
	Radar	2
	Telephony	11
		total: 29 (22%)
4. Supporting research and services	(various)	20
	Applied optics	3
	Mechanics, control and automation	2
	Statistics and applied mathematics	4
	total: 29 (22%)	
5. Mechanics		5 (4%)
6. Medical and biological work		1 (1%)
7. Atomic physics and nuclear power		3 (2%)
8. Gas discharges		4 (3%)
9. Miscellaneous subjects	Photochemistry	14
	Welding rods	1
	Liquid air machine (Stirling)	6
		total: 21 (16%)

Table 2. The research programme in 1954

Main research field	Research topics	No. of scientists
1. 'Materials'	Magnetic materials	11
	Dielectric materials	4
	Luminescent materials	9
	Semiconductors	20
	Metals	5
	Glass	5
	Measuring methods & instruments	5
		total: 59 (27%)
2. 'Devices'	Basic elements (transistors)	25
	Vacuum tubes	12
		total: 37 (17%)
3. 'Systems'	Radio	6
	Television	13
	Microwaves	(PM)
	Telephony	15
	Computers	(PM)
		total: 34 (16%)
4. Supporting research and services	Colloid chemistry	(PM)
	Analytical methods	6
	Chemical preparations	4
	Metals technology	4
	Bonding & brazing of metals, ceramics, glass	(PM)
	Testing of materials	4
	Applied optics	3
	HF measuring devices	(PM)
	Electronic measuring methods for physical quantities	4
	Mechanics, automation	7
	Statistics & applied mathematics	8
	total: 40 (19%)	
5. Mechanics	Fundamental & applied acoustics	6
	Ultrasonics	(PM)
	Servomechanisms	(PM)
	total: 6 (3%)	
6. Medical and biological work		9 (4%)
7. Atomic physics and nuclear power		7 (3%)

Main research field	Research topics	No. of scientists
8. Gas discharges		5 (2%)
9. Miscellaneous subjects	Photochemistry	4
	Plastics	4
	Welding rods	2
	Liquid air machine (Stirling)	8
		total: 18 (8%)

In subsequent years there was an increase in low-temperature physics and technology and in measuring and control. The PIT (Products for Industrial Applications) PD, which profited most from the outcomes of that research, was one of the fastest growing PDs in those years.⁵² Apart from those changes the programme remained fairly stable. Table 3 shows what the situation was like in 1968.

The largest part of the research programme was divided into the areas of materials, devices and (electric) systems.⁵³ When we compare the relative efforts in the materials, devices and systems domains at the beginning and at the end of the 1946-1972 period.⁵⁴ We see that the effort made in materials was really quite constant. Two important areas in this period were the semiconducting materials and magnetic materials. For some years quite some effort was put into superconductivity and superfluidity. The effort in devices grew in the late 1960s, mainly due to the emergence of semiconducting devices (ICs). Research into light sources and tubes (other than television tubes) diminished in the Nat.Lab. Laser technology was another emerging field. The relative effort put into systems research diminished somewhat in the late 1960s. At that time an emerging field was that of the computer. Instrumentation for 'fundamental' science fields, such as nuclear physics and astrophysics, also became well represented in the research programme. The systems domain saw its main growth in the next period (1972-1994). In terms of disciplines, it was in particular solid-state physics, semiconductor physics and chemistry, and mathematics that were to grow in importance.

The more 'fundamental' areas like solid-state and semiconductor physics received ample attention, too. This corresponded with the fact that the management identified this type of research as typical Nat.Lab. work. 'Fundamental' fields for which applications were still far away, such as lasers, superconductivity and superfluidity also grew substantially. By working on instrumentation, the Nat.Lab. was also able to follow the

Table 3. The research programme in 1968

Main research field	Research topics	No. of scientists
1. 'Materials'	Theory and general problems	15
	Magnetism, metals and low temperature physics	42
	Semiconductors	19
	Dielectrics, piezoelectrics, ferroelectrics (PM)	
	Optical phenomena	7
	Miscellaneous chemical subjects	12
		total: 95 (25%)
2. 'Devices'	Tubes	33
	Solid state devices	53
	Other devices	(PM)
	Device applications	8
		total: 94 (25%)
3. 'Systems'	General problems; filters	(PM)
	Radio, acoustics, recording	22
	Television	13
	Computers	10
	Telecommunications and radar	7
	Measuring and control; automotive	20
	Infrared systems	7
		total: 79 (21%)
4. Supporting research and technology	Mathematics	10
	Chemistry and physics	24
	Technology	6
	Optics and photography	6
	Miscellaneous	
		total: 46 (12%)
5. Mechanics	Electro-mechanics	5
	Stirling machines	13
	Household appliances and misc. mechanics	14
6. Medical and biological work	(incl. perception)	16 (4%)
7. Atomic physics and nuclear power		11 (3%)

developments in other 'fundamental' fields such as nuclear physics and astrophysics. All of these fields indeed fitted with the term 'fundamental' because none of them at that time was related to concrete products, but only to phenomena that were by far not fully understood yet. The laser was not an industrial product until specific applications had been invented for it. For Philips the field of optical recording and optical communication were to become areas in which lasers were exploited. But that would not happen until the late 1970s.

New, Foreign Labs for Development Support Tasks

Apart from the company structure formalisation after WWII, the Nat.Lab. was confronted with another change, namely the emergence of a number of foreign labs within the Philips company. The company's Board of Management wanted to have labs in specific countries in order to cater for opportunities for research support for the company's industrial activities in those countries.

The first lab was set up in 1944 when Prof. O.S. Duffendack was appointed its director at the North American Philips Corporation (NAPC) branch.⁵⁵ This lab in Irvington, in the state of New York, aimed at supporting the activities of the NAPC, which had been founded two years before. Marketing the Metalix X-ray tube was one of the NAPC's activities. Duffendack demanded that he would be appointed for at least five years and that he would be given sufficient resources to build up a serious research programme. In 1964 the lab moved to Briarcliff Manor.⁵⁶

The second lab to be set up outside the Netherlands was the Mullard Research Laboratories in Salford, UK (MRL; later the name was changed to the Philips Research Laboratories, PRL), founded in 1946.⁵⁷ The name was a strategic choice: rather than calling it the Philips Lab, the British name Mullard was kept to make it sound more British. This was done because in the wartime, which was the time when the preparations for taking over this lab were made, the British government was suspicious about working with Philips. They were afraid that information would leak to (German) occupied areas via Philips' contacts with neutral areas.⁵⁸ As the government started reducing the number of government-funded research labs after the war, vast numbers of scientists became available for the Mullard Lab. Dr. C.F. Bareford was appointed its first director.

When these two labs had been established, a memorandum was issued to clarify the role of the foreign labs in relation to the Nat.Lab. It was stated that the Nat.Lab. should serve as the centre for the research activities and that it should be the only lab where more 'fundamental' research should be carried out.⁵⁹ The other labs should focus on 'applied' research, related to the various business opportunities in the specific countries where those labs were situated. The foreign labs were also expected to

obtain contracts from the national governments of their respective countries (at a meeting in 1952 it was mentioned that the lab in Salford for instance was at that time funded for up to 75% by these sorts of contracts).⁶⁰ In order to achieve that goal, the choice of the managing director was considered to be important: he should be a person with such a good reputation that the national government would consider him to be a serious partner. Consequently, the labs were given a strong national orientation, and they were expected to work in close co-operation with their respective National Organisations (NOs) with the Philips company. Holst had expressed an opinion that the various labs should work 'not for Philips Paris or Philips London or Philips Eindhoven, but rather for Philips as one body',⁶¹ but in practice this did not work. Whenever the Nat.Lab. ran into capacity problems, the other labs could take over certain projects. Already from the beginning it was clear that there would be problems with respect to the role of the foreign labs. In the summer of 1950, even though in September 1945 the co-ordination of activities had been established in the Rye report, Duffendack complained that Philips had not been very clear in its policy towards the NAPC: was it a 'daughter company' or an 'independent establishment'.⁶²

Already in 1946 the idea of having a Philips lab in France had been considered.⁶³ It was not until September 4, 1950, though, that the French Laboratoires d'Electronique et de Physique Appliquée (LEP) was formally founded, although in 1948/9 the first lab activities had already started in Paris.⁶⁴ The lab was a Société Anonyme with as its only shareholders the companies for which it acted as a research laboratory: La Radiotechnique, S.A., Hyperelec, S.A., T.R.T., S.A. and the Compagnie Française Philips and its affiliates Philips Industrie and Philips Eclairage et Radio. Dr. G.-A. Boutry was appointed as its managing director. Boutry had studied at the Conservatoire des Arts et Metiers. As a consequence, the scientific status of his background was not the same as that of someone who had studied at the more respected Grand Ecole.⁶⁵ As has already been mentioned before, this status was seen as relevant to acquiring contracts from governments, so the situation was not ideal. Yet the lab functioned well under his leadership and flourished. In 1965 the lab was moved from Menilmontant to larger premises in Limeil-Brevanne.

In Germany Philips founded two labs. The lab in Aachen started up in 1955 as an annex of the Nat.Lab. It was expected that after WWII, it would be difficult to attract enough Dutch scientists. To attract German scientists an annex was founded in Germany, because at that time it was not yet wise to let German scientists move to the Netherlands.⁶⁶ Aachen was close enough to Eindhoven to enable easy contacts. Dr. Pannenberg was appointed as its first managing director. From May 1955 on he made all new Aachen scientists work in Eindhoven for some months. Later it became evident that it was not really a problem to attract scientists to the Nat.Lab., but the lab was kept on.⁶⁷

The Hamburg laboratory was established in 1957 with Dr. F. Borgnis as its first managing director, and like the labs in the USA, UK and France, it had a more nationally oriented bias. Borgnis was a good scientist, but he probably lacked the managerial capabilities that were needed to build up the lab, because the technical director of the Philips company in Germany, Dr. Mootz, asked Pannenberg to take care of the Hamburg lab for some time. Mootz found Dr. Jobst, former director of the Valvo tube factory in Hamburg, who was prepared to take care of recruitment for both labs, which for Pannenberg, inexperienced in the German situation, was a great help.⁶⁸ The Hamburg lab was established under the Müller company name. We came across this German company in the X-ray tubes case study given in the previous period (see Chapter 4). The names of the two German labs were similar: Philips Zentrallaboratorium Aachen (PZA) and Philips Zentrallaboratorium Hamburg (PZH).

In 1963 a lab was founded in Brussels, Belgium: the MBLE Research Lab. MBLE stands for Manufacture Belge de Lampes Electriques, and it was a company that Philips had bought in 1924.⁶⁹ When Philips took over the lab from the Belgian government and added it to the MBLE company to become the MBLE lab, it was still a small computing centre (with only ten scientists). After that it expanded, but it always remained the smallest Philips laboratory. Dr. V. Belevitch was its first managing director. Its disciplinary range was limited to applied mathematics.

One more lab was founded in 1979, in Sunnyvale, USA, with Dr. E. Kooi as its managing director. In the description of the 1972-1994 period, this lab and its role in IC technology will be discussed further.

The national orientation of the various labs resulted in each lab having certain areas of specialisation.⁷⁰ The English lab in the 1946-1972 period was active in transistor and IC applications, microwave communication, military devices for infrared night viewing, masers, magnetic film computer storage and automotive electronics. At the LEP the areas of specialisation were photoelectricity, television optics, vacuum physics, neutron production (related to the French atomic bomb), and computer peripheral equipment. The Hamburg lab pursued research that stemmed from its Müller background (X-ray equipment), and the Aachen lab took over a number of lighting research activities from the Nat.Lab. and worked on electric motors for domestic appliances. The lab in Brussels was active in applied mathematics for circuit theory, space research, the automatic layout of printed circuit boards and computer software. The lab in Sunnyvale was dedicated to IC.

To get some idea of the relative size of the labs, the following numbers for 1964/5 can be used: the costs of the Nat.Lab. then were NLG 60 million (2,130 people), the Mullard lab in Salfords cost NLG 14 million (700 people), the Irvington lab in the USA NLG 6.8 million,⁷¹ the LEP in France NGL 13.6 million (320 people), the German labs put together

NLG 13.7 million (Aachen 330 people, and Hamburg 310 people), and the Brussels lab NLG 0.9 million.

In order to establish periodic communication between the lab directors, the CRC and RDC meetings were initiated (mentioned before in this section). In addition, contact was stimulated by exchanging scientists between the labs. The minutes of these meetings show that the main effect of the meetings was to inform the directors about what went on in each other's labs. The meetings did not serve to arrive at agreements on certain divisions of tasks. Each lab stuck to its own interests, and the directors were autonomous when it came to deciding on the research programme for their particular lab. This was in accordance with the national task outlines, which formed the basis of their existence. At some CRC meetings, however, concern was expressed about this. Already in the 1952 CRC comments were made that it would be difficult to steer the labs from the Netherlands in cases where this would be desirable.⁷² M. Lopez Cardozo, director of the Corporate Research Bureau, who attended the CRC meetings from 1955 on, was assigned the task of labs co-ordinator.⁷³ In 1955 he complained about the 'steering groups', which did not 'steer' but only seemed to serve as a sort of 'colloquia'. Steering groups had been installed for those topics that were researched at more than one lab, and they were manned by the directors and group leaders involved in the respective topics. The exchange of personnel and reports, mutual visits and conferences needed further stimulation, and the fact that this was mentioned in the CRCs would imply that up until then the co-ordination via these ways had not resulted in the effects that the management had had in mind. During the 1962 CRC it was decided that a bi-monthly exchange of progress reports should be started up, and at the next CRC Casimir mentioned in his introduction to the meeting that he had noticed an improvement in the relationship between the labs. Only the exchange of personnel remained problematic, and it was stated that J.H. van Santen, who had already been head of the Bureau for some years, should make arrangements to improve this.⁷⁴

In 1966 the Corporate Research Bureau explicitly expressed the desire to establish some sort of co-ordination between the lab programmes. According to the Bureau, large research areas should be divided over at most two labs.⁷⁵ At that time the recommendation did not yield any concrete effects. It was not until 1972 that an international research co-ordinator was appointed, G.W. Rathenau, who had become managing director of the Nat.Lab. in 1967.

To summarise, in terms of laboratory tasks, the Nat.Lab. was intended to be the only lab where non-product-related research was done. As indicated, this was the Nat.Lab.'s own main ambition in the 1946-1972 period.

Development-oriented research work was carried out at all the labs, and each lab could decide on its own specialities, depending on the national situation in which the particular lab operated. Thus there was tension between the original national task of the labs and the desire to co-ordinate activities in cases where this would be useful.

Production-oriented Research at the CFT

In 1968 the start of the Centre for Manufacturing Techniques (Dutch abbr.: CFT) was reported in a CRC meeting. This organisation had been initiated by the Board of Management. According to Van Houten⁷⁶ it was primarily Tromp who had come up with the idea; Pannenberg also played a role in the decision by pushing Tromp, because he realised that the continuous decentralisation could result in a difficult situation for smaller PDs which would not be able to finance a mechanisation centre of their own.⁷⁷ The CFT was a central company organisation for mechanisation issues, similar to the research labs. Unlike the research labs, though, the CFT's budget was not allocated directly by the Board of Management. The CFT had to acquire the main part of its budget from the PDs. The CFT's role was to support the PDs when production problems cropped up and to develop new manufacturing and automation techniques. In order to fulfil this role the CFT did research, but it was research that was always focused on production techniques. Thus, the CFT became a new company factor that influenced the task profile that the Nat.Lab. had defined for itself. Conducting small research studies to support PDs when it came to solving practical production problems was retained as a part of the Nat.Lab. task profile, mainly because of the good contacts at the work-floor level. Ultimately, it was the CFT that for the PDs became a more 'natural' resource for those sorts of research needs. At first, this may have caused some competition between the CFT and the Nat.Lab., because the Nat.Lab. still kept a small group working on production methods, but the relationship between the CFT and the Nat.Lab. seems to have been fairly good, in particular with P.J.M. Botden and his group, which had been moved from the Nat.Lab. to the CFT. Some people were detached from the Nat.Lab. to the CFT for some time.⁷⁸ Most of the CFT people came from the PDs, and so they had a more sound grounding in production than the Nat.Lab. people usually had. Apart from the informal contacts there were no structural contacts with the CFT.

To Summarise

In the goals that were recognised by the lab management, the onus was on taking a responsibility for the company's long-term product policy. For this a substantial effort in 'fundamental' research – for which there was

not yet a concrete application – was seen as being essential. Development-oriented research continued to be seen as a Nat.Lab. task, in that the Nat.Lab. was to gain new knowledge about the phenomena underlying the products, but as far as the Nat.Lab. was concerned, product ideas should be transferred to the PDs for further elaboration after the basic problems had been solved. The research support network for solving production problems formally was not given as a core task for the Nat.Lab., but in practice it was the good contacts at the workflow level that caused this work to continue. The new foreign labs were meant to be primarily involved in development-oriented research, and the CFT became a new resource of knowledge for the PDs in cases of production problems.

5.3 *Further Increase of Means*

In the 1946-1972 period the Board of Management gave the Nat.Lab. a stable proportional share in the company's means growth. The budget for the research laboratories was allocated directly by the company's Board of Management. This shows that the Board of Management believed in the useful role of the Nat.Lab. in addition to that of the PD development labs. For the period discussed in this Part, the research budget was stable and fixed at about 1% of the company's turnover. The result was that the amount reserved for the Nat.Lab. and the foreign labs (including the costs for obtaining patents and licences) grew from NLG 5 million in 1947 to NLG 130 million in 1965. The research costs for the Nat.Lab. alone amounted to about 40% of the entire budget; the foreign labs had about 36%; the remainder were costs for obtaining patents and licences.⁷⁹

There was no project-wise budgeting, because Casimir was against that. He feared that it would form an obstacle to the research freedom that was necessary for doing 'fundamental' research.⁸⁰

It was the increasing means that allowed the population of the whole research organisation (Nat.Lab. plus foreign labs) to grow even more vigorously than in the previous period (1923-1946). From 1966 onwards the number of employees remained stable until 1975. At the end of Part II the reason for that change will be discussed. The growth in the lab population was due to an increase in the number of scientists, assistants, and technical and administrative personnel. The number of scientists grew from 157 in 1945 to 388 in 1965; the number of assistants grew from 170 in 1945 to 991 in 1965; the number of technical and administrative personnel grew from 240 in 1945 to 722 in 1965. The numbers show that the ratio of scientists to other employees went from 1:2.6 to 1:4.4. In the years 1965-1970 a number of assistants left, and an equal number of scientists came, which altered the ratio to 1:3.5.⁸¹ This may give the impression that the 'research potential'⁸² decreased after 1945. We have to bear in mind, though, that

the educational background of the assistants changed in this period. Until the early 1960s most assistants were drawn from the 'hbs', a higher secondary education type of school. In 1965 the 'hts', a college institution for higher vocational education within the Dutch educational system, was reorganised, and the study was disrupted by the increased orientation towards academic scientific education.⁸³ From then on, more assistants with this new 'hts' educational background entered the lab. Those assistants had a higher level of scientific and technical competence. To allow such assistants to do the ordinary measurement work that had traditionally been done by the assistants with a hbs background would be to under-rate their capabilities. Besides that, the emergence of the computer had reduced the need for simple measurement work done by people. The role of the assistants therefore changed in this period, and they became involved in setting up experiments rather than carrying out measurements. Several assistants decided to continue their education at a university, in order to gain the scientist status. Some of them became university professors.⁸⁴

The size of the facilities also increased in this period. In 1944 the total floor area of the Nat.Lab. in Strijp (a district of the city of Eindhoven) was 20,000 m². By 1952 it had increased to 25,620 m², and in 1955 a new wing came into use, which further increased the workfloor area to 27,920 m². In 1963 the official move to new facilities situated in the village of Waalre (near the southern border of Eindhoven) started.⁸⁵ The engineer S.S. Wadman co-ordinated the first phase of the move in 1963, and he tried to take into account the requests of the scientists in terms of room sizes and occupation. The move took place in phases, so that for a certain period there were two locations. This caused some inconvenience, like gaining access to the library.⁸⁶ A mini-bus service between Strijp and Waalre ferried people from one location to the other.⁸⁷ The rapid growth in the lab personnel apparently demanded very ambitious planning of the new site: in 1970 a lab population of 3,000 people was expected and therefore quite a large number of buildings was planned.⁸⁸ Already by 1965, it had become evident that the growth would be less than expected, and so just under half of the planned buildings were removed from the plans.⁸⁹ The 1963 move was seen as a good occasion for holding a symposium on the research activities. This symposium was held on September 26 and 27 of the same year.⁹⁰ A number of research studies were presented, which together gave an impression of the total research programme continuing at that time. It was not only the lab management that stimulated the organisation of this symposium, but also the Philips Board of Management.⁹¹

To sum up, the means allocated to the Nat.Lab. to achieve the lab's goals and objectives kept pace with the company's growth. The internal (non-

project-wise) budgeting of the lab fitted in with the Nat.Lab.'s vision of its role as the 'fundamental' research lab.

5.4 *Culture and Structure for a 'Fundamental' Research Lab in an Industrial Company*

In Part I we identified two tensions in the culture and structure of an industrial research laboratory as a professional organisation. There is tension between an academic and an industrial orientation, and there is tension between freedom for the scientists and control over the scientists. In Part I, we saw how Holst struggled with these tensions. Now we will consider the way in which these tensions were dealt with in the 1946-1972 period.

An Academic Atmosphere in an Industrial Lab

For Holst, the main reason why he had invited famous scientists to present their new theories in the Nat.Lab. had been to give his lab a certain academic status (see Part I). Often the theories that were presented did not directly relate to the work going on in the Nat.Lab. That did not really matter because the main purpose was to attract good scientists. Casimir displayed a similar interest in the latest developments in the natural sciences, and not just for the sake of their academic status. By the time he started working there, the fame of the Nat.Lab. had already been established. In the CRCs Casimir always asked the question of whether a certain new theory or field could be absorbed into the Nat.Lab.'s research programme. Thus, a careful scanning of the latest scientific developments under his leadership became a continuous item at the top of the CRC's agendas.

Because of this orientation to scientific developments, some new disciplines entered into the research programme. In 1958 a need was expressed to incorporate the new information sciences into the research programme.⁹² The number of publications in that area was still low at that time. The most striking Nat.Lab. activity was the development of what became known as the PETER computer, followed by the PASCAL computer⁹³ in 1954. PETER was the Philips Experimentele Tweekalige Electronische Rekenmachine (Philips Experimental Binary Electronic Calculator). Although the name PASCAL was in the first place the acronym for the Philips Automatic Sequence CALculator, while also referring to the famous 17th-century scientist Blaise Pascal, the name was soon to become an acronym for: Philips Akelig Snelle CALculator (the Philips Horrendously Fast Calculator). The PASCAL was a binary parallel machine that was used by the laboratory itself for doing scientific calculations. Some characteristics of the PASCAL were: its 42-bit word length, its 660 Hertz

clock frequency, its 2016 44-bit magnet ring memories and its 16,384 44-bit drum memories. Punch cards were used as input and output medium. For this machine 1200 tubes, 10,000 transistors and 15,000 diodes were used. A third computer was called the STEVIN computer, after the Dutch mathematician Simon Stevin.⁹⁴ In the 1960s there was an enormous output increase in the area of information sciences. For information technology there was a separate group, with Zonneveld as its group leader. The group belonged to the field of 'supportive research and technology'. In the same field there was a new mathematics group, and Bouwkamp was the group leader. From Bouwkamp's publications we can see that the mathematical problems he selected were sometimes very loosely related to the rest of the research programme, which was sometimes very loose. That was something that fitted in well with the idea of exploring the potential of the discipline as a more 'fundamental' type of research.

With respect to the academic orientation, contacts with universities about the content of the scientific work were seldom mentioned in management (CRC and RDC) debates. In the field of transistors and ICs, the universities were not seen as interesting partners. The Nat.Lab. maintained that the universities lagged behind their industrial counterparts development-wise. The main role of the universities at that time was to educate new scientists. The vast majority of the incoming scientists in the 1946-1972 period (about 300) came from the Technische Hogeschool Delft (Delft Polytechnic),⁹⁵ the oldest of the Dutch polytechnics. The second oldest one was the Technische Hogeschool Eindhoven (Eindhoven Polytechnic) that was founded in 1956. This second polytechnic was located in the vicinity of Philips, although the founders were keen to prevent this institute from becoming a 'Philips-Polytechnic'.⁹⁶ In the 1946-1972 period about 100 scientists went from that polytechnic to the Nat.Lab. Other universities that supplied the Nat.Lab. workforce were the University of Amsterdam (about 60 scientists), the University of Utrecht (about 60 scientists), the University of Leyden (about 50 scientists), the Free University of Amsterdam (about 25 scientists) and the University of Groningen (about 20 scientists).⁹⁷ Philips had contacts with the universities, not in the last place via the 'buitengewone' (extraordinary) or 'bijzondere' (special) professors that worked both with the Nat.Lab. and with a university or polytechnic (in most cases with one of the polytechnics). There were also scientists who left the Nat.Lab. to become ordinary professors. The professors served as a sort of bridge between industry and university. On the one hand, they brought industrial know-how to the university, while on the other hand their position allowed them to follow the academic developments. Perhaps most importantly, this put them to contact with potential new Nat.Lab. scientists. Often contacts were maintained with other university professors as well, although in the 1970s these had to be kept silent

because the labour unions might oppose this ‘hidden’ talking about possible future colleagues.⁹⁸ When such contact led to serious interest in a student who was near to finishing his or her study, the professor concerned would be visited by a representative of the company’s personnel department and one of the lab’s directors. Depending on the judgement of the professor, the student would be invited for a meeting at the Nat.Lab, which could lead to an appointment.

The importance of ‘fundamental research’ as a particular Nat.Lab. activity influenced the Nat.Lab. culture (at the scientists’ level). Not only the management, but also the scientists themselves had an interest in the position of that type of research. This concern was expressed by them at the Directors-CoCo meetings. These meetings had been initiated by Holst in 1945 to reinforce contacts between the lab management and the workers.⁹⁹ CoCo was an abbreviation for Contact Committee. This committee consisted of elected representatives from among the scientists. In the 1960s Directors-CoCo meetings were held about eight times per year. Later the frequency was reduced to about two or three times per year. Both the scientists and the other employees were represented in what was known as the Kern (‘Nucleus’). The Kern served as a sort of workers’ council.¹⁰⁰ It met about every other month. The representatives were elected for a period of three years.

In the Directors-CoCo meetings the issue of ‘fundamental’ research was raised several times by the CoCo members. Concern was expressed by the researchers that the lab management seemed to have more appreciation for ‘applied’ research than for ‘fundamental’ research.¹⁰¹ For Casimir this was reason enough to give special attention to the issue in his presentation at the Directors-CoCo meeting on June 14, 1962. He ensured those present that the management did recognise the importance of ‘fundamental’ research. It is not clear what caused the scientists to believe that ‘applied’ research would have better recognition with the management than ‘fundamental’ research. In considerations at management level, there was constant concern about the position of ‘fundamental’ research. Anyway, the examples given show that not only at the directors’ level, but also among the scientists there was a strong feeling that ‘fundamental research’ deserved to be continued without being in any way cut back. Even without evident reasons the scientists were already eager to defend ‘fundamental’ research against what they perceived as the threats of increasing resources being put into applied research.

Freedom for Scientists to do Proper ‘Fundamental’ Research

In the eyes of the management, the orientation of ‘fundamental’ research for long-term innovations required a type of researcher who was able to

exploit a high degree of freedom. Researchers who worked in the lab during those years still remember that when they started their work in the Nat.Lab. they were only told what topic they were to work on, not what sort of research they were expected to do. Casimir's perspective on the Nat.Lab. as a professional organisation was that, as professionals, scientists only need to be given the necessary framework for their research work, but no strict guidance. Only researchers who were not able to make sophisticated use of this freedom needed to be given exact indications about what to do. Such researchers did exist. In 1962 Pannenburg complained that there were too many of them, and that there was a lack of more independent researchers.¹⁰² This did not change Casimir's conviction that an industrial research lab should be based on the ability of good scientists to determine, by themselves, what research is relevant and what is not. He maintained that only with such researchers could high-quality 'fundamental' research be made to contribute to the company. In that respect several of the rules that he had presented as 'Holst's rules' (see section 4.4) in fact reflected more his own research management philosophy than Holst's.

Growing Hierarchy in the Structure of the Lab

The existence of the Directors-CoCo and Kern meetings shows that there was a need to establish contacts between the lab management and work-floor-level employees. It was felt that these contacts needed to be formalised. In the first post-WWII years, a structural hierarchy had developed. This had created distances that did not exist when the lab workforce was still small.¹⁰³ In this hierarchy we find the managing directors (Casimir, Verwey and Rinia who served after Holst retired), directors (H. Bienfait being the first one), group leaders, scientists, assistants and technicians. The point of having a layer of three (later four) managing directors was for each to take care of about one-third (later one-fourth) of the entire research programme. In 1968 there were four managing directors: Rathenau (physics), Vink (chemistry), De Haan (applied physics) and Teer (electronic systems). Most of the materials research was conducted under Rathenau and Vink, most of the devices research took place under De Haan, and most of the systems research was done under Teer's directorship. Directors had to direct a number of groups in certain research fields. Group leaders were responsible for the daily supervision of the work of the scientists and assistants in their group and were responsible for their specific scientific field.

Apart from the position of 'fundamental' research in the Nat.Lab., most of the time it was practical matters in the contacts between the lab directorate and the employees that were dealt with. In the first Directors-CoCo

meeting some practical consequences of the recent war were discussed, in particular people's lack of enthusiasm for their work. The possible causes of this that were mentioned were: material problems (e.g. the housing shortage in Eindhoven and the lack of transportation means) and the lack of research facilities. The Nat.Lab. management tried to solve all sorts of such practical problems. The first Directors-CoCo meeting played a role in making an inventory of these problems. Likewise in the 1960s the Kern meeting agendas mainly reflected practical problems. People talked about issues such as where to put bikes and motorbikes (the plate 'directors only' that was affixed to the bike shed that was nearest to the main building (called WB) illustrates the hierarchical relations of that time), the canteen facilities, noting down working hours, introducing a five-day working week (in 1960), the cleanliness of the corridors and the washing of lab coats. There was also a discussion about the lunch walk around the pond on the campus, which Vink was willing to permit as long as it did not lengthen the lunchtime.¹⁰⁴ Another practical issue was the proposed connection of the (nowadays named) Holst Avenue (the road that leads to the new lab premises in Waalre) to the nearby E3 motorway. Verwey had opposed this connection because he expected it to create unrest.¹⁰⁵ In 1963 a survey was carried out among employees into their living and working conditions: family composition, living circumstances, study facilities at home, financial elbowroom, secondary working conditions, debts, and working conditions at the laboratory. The results were presented and discussed in a Directors-CoCo meeting held on November 11, 1963. The main outcome was that the financial elbowroom was less than would be expected on the basis of people's gross income. Also accommodation was often insufficient. The results yielded material for discussions about housing and holiday regulations even until spring of the next year.

To summarise, the Nat.Lab. culture was academically oriented in that there was a constant search for new scientific fields or important external progress in existing fields that might be relevant for the Nat.Lab. to take up. Scientists had a lot of freedom to determine the content of their research work. Among the scientists the relevance of the fundamental research carried out was a matter of serious concern. Thus, there was a relationship between fundamental research as a particular Nat.Lab. goal and the culture of the lab. The structure of the lab became more formalised, with a hierarchy of managing directors, directors, group leaders, scientists, assistants and technicians. When it came to discussing practical issues, Directors-CoCo and Kern meetings were used.

5.5 *Tensions in the Relationships with PDs*

As we saw in section 5.2, the Nat.Lab. had defined 'fundamental' research as its particular task and had continued to do development-oriented research mainly as far as basic problems were concerned. From the Nat.Lab.'s angle the development labs in the PDs should then be responsible for taking over the further development of devices and systems. Support for solving production problems was offered because of the good contacts at the workflow level, but this type of work was not regarded as a typical Nat.Lab. task. This may seem to provide a clear profile for the Nat.Lab. in its relationship to the PDs, but in practice the contacts with the PDs were not completely unproblematic. On the one hand, the PDs felt that they did not do much to influence the Nat.Lab.'s research programme, while on the other hand the Nat.Lab. was confronted with the selective attitude of the PDs towards the research output that was offered to them for transfer. This selective attitude caused irritation within the Nat.Lab. when it was revealed that the PD had been previously told about the research topic in question but had never indicated that the research output would not be absorbed. From the point of view of the PD, that was understandable. The PD had not paid for that specific research topic, and continuation cost them nothing. And perhaps, after all, something useful would emerge. But the refusal to recognise the Nat.Lab.'s output was seen as a lack of awareness on the part of the PDs to recognise the long-term potential of the research output.

This relationship between the Nat.Lab. and the PDs can thus be characterised as lacking mutual commitment. The Nat.Lab. did not feel obliged to listen to the PDs requests, and the PDs did not feel an obligation to make use of the research output that was offered to them when they saw no opportunities for a healthy business. There were regular contacts between the Nat.Lab. and the PDs, but these had mainly to do with exchanging information. The Nat.Lab. told the PDs what they were doing and vice versa, but no clear commitments were made.

The frustrations that were felt on both sides can be nicely illustrated by the story of the index tube.¹⁰⁶ This tube was developed by the Nat.Lab. as an alternative to the shadow mask tube for television displays. The engineer P.M. van den Avoort and his group had worked on this tube for several years.¹⁰⁷ The principle was quite different from that of the conventional shadow mask tube, and it yielded quite sharp images for colour television. A well-prepared demonstration was given to the PDs Elcoma and RGT (Video) in 1971, and both the technical and the commercial directors were present. The technical director was impressed, but the commercial director did not like the small size of the image. The argument, that in Japan television sets had small-size images did not convince him and the index tube was rejected. In a memorandum written after the presentation, the PD intimated that the index tube was not a good alternative to

the shadow mask tube for them. Apart from the concern about the small sizes – the PD had evidence to show that the Japanese market was already moving towards larger sizes¹⁰⁸ – the PD was not eager to have two different parallel techniques for one product.¹⁰⁹ At that time, however, there was already a group working on this tube in the PDs television pre-development lab.¹¹⁰ This group had quite a large sum of money available, and of course in the Nat.Lab. this had created the impression that the PD was seriously interested in the index tube. For several years research into the index tube had been continued without any signs of protest from the PD, even though almost from the beginning it was known that it would not be suitable for larger image sizes. In the end, when work in the PD on the index tube was abolished by the PD directorate, both the Nat.Lab. and the PD were frustrated about all the effort that had been wasted.

There were different ways in which the Nat.Lab. tried to achieve its goal of influencing the company's long-term product strategy and development. In the first place, meetings were held with the company's Board of Management. Secondly, there were what became known as Quo Vadis meetings where the Nat.Lab. met both the Board of Management and the PD representatives. Thirdly, there were directors' contact meetings in which research directors met PD directors to discuss the activities within the Nat.Lab. and within the PD. Fourthly, the Nat.Lab. organised a series of Corporate¹¹¹ Research Exhibitions to show to the PDs what research output was to be expected in the not too distant future. In the fifth place, the transfer of Nat.Lab. personnel to the PDs amounted to a means of transferring all the knowledge embodied in those persons. Each of these five methods will now be described.

A Nat.Lab. Representative in the Company's Board of Management

At the beginning of this chapter we saw how a series of meetings with the Board of Management (known as 'Onze laboratoriumplannen') was used by the Nat.Lab. to make clear what it saw as its particular task within the company. In 1953 this series of meetings between the Board of Management and the Nat.Lab. managing directors ended. In 1957 structural contact was resumed by appointing Casimir to the position of research representative within the Board of Management.¹¹² Casimir thus became a discussion partner in the company's strategic debates at the highest level. In his autobiography he described this appointment as an important factor underlying his decision to stay with Philips in spite of the fact that the famous physicist Pauli teased him by calling him 'Herr Direktor' and accused him of being no longer involved in serious physics.¹¹³ In the 1957-1963 period Casimir was both managing director of the Nat.Lab. and a member of the Board of Management. In 1963 Eduard Pannenberg suc-

ceeded him as managing director of the Nat.Lab. In 1968 Pannenberg, an engineer from the Delft polytechnic and a managing director of the Nat.Lab., was also appointed to the Board of Management. When he was appointed to the Board of Management, research had remained part of Casimir's portfolio. In 1972, when Casimir retired, Pannenberg took over his role as representative for the research organisation.

The Quo Vadis Meetings

In the 1950s and early 1960s, Nat.Lab. and PD representatives met in what were known as the Quo Vadis meetings. These were initiated by the Board of Management to provide the platform for strategic discussions on new or developing product fields. Examples of such fields were: television, radar, semiconductors, magnetic materials, batteries, electronic medical equipment, and domestic appliances. Usually, the Patent Department was also involved in these meetings.

At those meetings, the Nat.Lab. received ample opportunity to talk about ongoing research activities.¹¹⁴ The focus of the discussions was always the desirable development of the PD's activities. The Nat.Lab.'s contribution varied from PD to PD. In the case of the Quo Vadis meetings with the PD Lighting, the Nat.Lab. reported in 1953 that not much was to be expected from the research, not only in the short term, but also in the long term.¹¹⁵ But usually the Nat.Lab. did put forward suggestions for future PD activities. Sometimes this contact with the Nat.Lab. was only one of the many contacts that the PD had with the research labs. The PD Icoma (later to be merged into Elcoma) reported in 1955 during a Quo Vadis meeting on the contacts with Bayer, BASF, ICI, Shell, and CIBA, concerning research into chemical issues.¹¹⁶ In some cases a PD explicitly uttered doubts about the long-term policy role that the Nat.Lab. claimed to have for the PDs. In 1963 the PD PIT (Producten voor Industriële Toepassingen, i.e. Products for Industrial Applications) challenged the role of the Nat.Lab. in systems research during a Quo Vadis meeting, because the development of whole systems was seen as only the task of the PDs, and not as the task of the Nat.Lab.¹¹⁷ Casimir and Rinia were forced to defend the notion that there was indeed a task for the Nat.Lab. when it came to doing systems research.

Thus, tensions between the Nat.Lab. and the PDs became apparent in the Quo Vadis meetings. Influencing the PDs' long-term product policy which was a role that the Nat.Lab. claimed to have appeared to be difficult to put into practice.

The Directors' Contact Meetings

To give an impression of how the directors' contact meetings functioned, five PDs will be dealt with in some detail: two consumer PDs (Radio,

Gramophone and Television, and Domestic Appliances), two professional PDs (X-ray and Medical Equipment, and Products for Industrial Applications), and the device PD, Elcoma. In the case of the remaining PDs, a shorter survey will be presented.¹¹⁸

With the RGT PD good contacts in specific areas were reported on the one hand in the directors' contact meetings, like with the index tube¹¹⁹ and with ICs-on-ribbon. On the other hand, though, the Nat.Lab. described the PD as a 'leak' in one of those meetings, through which information on new product ideas sometimes reached other parties too early.¹²⁰ That remark certainly did not reveal a great deal of trust. In the Minutes most contacts between the Nat.Lab. and the PD were termed 'informal'. In some cases the PD absorbed research output for further development, but in other cases it rejected the research output that was offered for transfer. In October 1970 the PD rejected a technical solution to a 'tiny vision', which had been developed at the English Mullard lab, as well as a Nat.Lab. proposal for a single-sideband system. In August 1972 RGT expressed interest in absorbing the Nat.Lab. know-how on injection logic, displays and memories, but not when it came to transferring the product idea of a pocket calculator. But the Nat.Lab. displayed the same sort of selective attitude toward the PD. In 1970 the PD showed interest in quadraphony, but the Nat.Lab. responded by showing an explicit lack of interest.¹²¹ There were also examples of a good co-operation. In 1971 there was, for example, a shared Nat.Lab.-RGT project on loudspeakers, and in the same year RGT people were added to the Nat.Lab. group that was working on the Video Long Play (VLP) project.¹²²

In general, the amount of time and money spent on research in the Huishoudelijke Apparaten (Domestic Appliances) PD was much lower than for e.g. the RGT PD. Here, too, we find certain Nat.Lab. proposals being accepted and others being rejected. In August 1968 the PD accepted the application for PTC resistors as developed the Nat.Lab.¹²³ In December of that same year, the use of energy paper for an electrical toothbrush with a water-driven motor was also reported to have been transferred from the Nat.Lab. to the PD. But in October 1970 the PD rejected the transfer of an IR radiating mirror from the lab in Aachen. In general, the Nat.Lab. only seemed to make small contributions to the PD's product development.

Now we move on to two examples of professional PDs. The X-ray and Medical Equipment PD was one of the few PDs not to be merged or split up during its existence (in 1970 it was only renamed the Medical Systems PD). During the 1960s Nat.Lab. directors met with this PD about once every 4-5 months. The PD used the directors' contact meetings to pass on

specific research requests to the Nat.Lab.¹²⁴ With this PD, too, there were instances of good co-operation. In 1968 a decision was taken to start a steering group consisting of Nat.Lab. and PD representatives that would be responsible for making a system analysis of the total health service system and for presenting proposals for an integral development programme for the PD.¹²⁵ That was an example of where the Nat.Lab. really was asked to fulfil the role of long-term 'think-tank'.

The PIT (Products for Industrial Applications) PD, apart from changing its name to Science & Industry in 1971, remained fairly stable as well. In 1967 Pannenberg complained in a directors' contact meeting about the lack of use made by the PD of a number of Nat.Lab. research outputs which he considered to be worthwhile. In particular, the microwave research carried out by the Nat.Lab. had remained almost totally unutilised by the PD. As Pannenberg stated, the result had been that: 'the company's tube position was weak, the semiconductor position almost non-existent, and the ferrite activity small'.¹²⁶ At the same meeting the PD informed the Nat.Lab. that they were not interested in transferring Peek's digital correlator. In 1969 Van Tol (PIT) explained that the PD often rejected ideas generated by the Nat.Lab. because it had to work economically and had a limited development capacity.¹²⁷ In the field of cyclotrons there was practical co-operation, for example in the areas of cold gas refrigerating machinery, measurement equipment for environmental monitoring, equipment for spectrophotometry and plasma-MIG welding. Unfortunately, this co-operation ended in a rather painful way: in a letter to J.D. Otten (PIT), Casimir complained about the fact that the PD had stopped all their cyclotron activities without consulting the Nat.Lab.¹²⁸ On the other hand, it should be mentioned that the Nat.Lab. in the same year cancelled an order for a cyclotron placed with PIT,¹²⁹ which of course had irritated PIT. There were also problems when it came to working together on electron microscopes.¹³⁰ Clearly, the communication between the Nat.Lab. and the PD was problematic in certain respects.

The PD Elcoma – a result of the merger between the PD Icoma and the PD Electron Tubes in 1965 – had a special position in the company. Its role was to deliver devices for the system PDs (both consumer and professional devices). As a consequence, the Nat.Lab. had to keep in contact with Elcoma, not only for device ideas, but also in conjunction with system PDs and when product items for certain devices were needed. This special position of Elcoma's gave rise to several problems. In 1969 Elcoma and the PD Lighting disagreed on the importance of lasers.¹³¹ Elcoma felt that they had to convince Lighting that more effort needed to be put into the development of this device. In a 1975 memorandum entitled 'Elcoma-Research Problems', J.H. van Santen (Nat.Lab.) presented a whole list of research

outputs that Elcoma had not yet taken over from the Nat.Lab., even though at least one system PD had expressed interest. Some of these outputs were magnetic bubbles, LEDs, the material ZrO_2 (seen as 'essential' for some PIT activities), and a new generation of Plumbicon pickup tubes. We have to be aware that this was a Nat.Lab. complaint, and so the example of the magnetic bubbles should make us wary. These magnetic bubbles would never become a commercial success, and it might well be that Elcoma had indeed recognised the problems at an early stage.¹³² The Nat.Lab. experienced problems in its contacts with Elcoma, the device-PD within the company. In an interview Teer called Elcoma 'perhaps the biggest schizophrenic situation within the company'.¹³³ He was alluding to the double life that Elcoma seemed to live: it was part of the whole company, but at the same time a PD that first took care of its own interests.

Apart from the contacts in which a system PD was also involved, the Nat.Lab. had contacts with Elcoma for the sake of Elcoma itself. These contacts were not without problems either. As with other PDs, there was a selective attitude towards the Nat.Lab.'s research output. In a directors' contact meeting of 1966, Elcoma informed the Nat.Lab. that energy paper would be transferred, but that the PD was not interested in the deposited capacitor.¹³⁴ Just as in the PIT case, there were mutual accusations. In 1966 the Nat.Lab. accused the PD of being responsible for the loss of the company position in ferrites. According to the Nat.Lab. this loss was not the result of the fact that research stopped activities on Ferroxdure, but it was caused rather by lack of production quality and a poor assortment.¹³⁵ For its part, the PD expressed irritation about the fact that Teer, as a Nat.Lab. director, had written in a USA travel report, without consulting the PD, that the sales of ferrite heads in that country was not interesting.¹³⁶ The PD was angry about the fact that the Nat.Lab. had decided to stop research in the field of piezo-electrical materials in Eindhoven and to move this branch to Aachen without even consulting the PD.¹³⁷ All that does not sound too positive, but with this PD, too, cases of good practical co-operation can be cited, for example in the fields of resonators, filters and electrolytic capacitors.¹³⁸ The PD could also ask for specific research support, such as in the fields of ferrite cores and soldering.

Now we come to the remaining PDs. The PD Lighting had a research laboratory of its own, which was often seen as being in competition with the Nat.Lab. The Lighting Lab maintained that it covered almost all research in the field of light sources. In some areas there was co-operation with the Nat.Lab., but not on light bulbs or gas discharge lamps (research on those topics was done in Aachen). We find issues such as: p-n luminescence,¹³⁹ the CO_2 -laser,¹⁴⁰ the use of SiC for fibre-enforced tungsten,¹⁴¹ and batteries¹⁴² on the agenda of the directors' contact meetings in the late 1960s. Finally there were some smaller PDs, like Glass and Ceramic Products

(reorganised in 1952 to become the PD Glass) and Duphar, the Philips PD for Pharmaceutical and Chemical Products. From the early 1970s on, an important issue when it came to the contacts with the PD Glass was the optical fibres issue. With respect to Duphar, the contacts were limited. In 1965 the Nat.Lab. tried to convince the PD that their research into molecular biology was of interest to Duphar,¹⁴³ but no evidence has been found that the PD made any effort to transfer research output in this field. It was only in 1968 that there was a concrete request put in by this PD to the Nat.Lab. and that was for support in the field of isotopes.¹⁴⁴ In a 1970 letter to Vink, Verwey remarked that the group that was to work on Duphar topics (the biology group) had some points in common with Duphar, but most of its research did not relate to Duphar activities.¹⁴⁵

Altogether these directors' contact meetings leave us with mixed feelings. On the one hand, the Nat.Lab. and the PDs were able to find out about each other's activities, and the meetings do show several examples of good communication and co-operation. On the other hand, both from the Nat.Lab. and the PD side, we see points of irritation. Former directors and co-directors who were once involved in the directors' contact meetings recall the informal meetings in which no real transfer decisions were made.¹⁴⁶ The PDs felt unable to influence the Nat.Lab.'s research programme, and the Nat.Lab. felt unable to get their research output transferred to the PD. The fact that there was willingness to communicate on both sides, raised expectations of commitment, and irritation was caused by the fact that the other partner did not fulfil these expectations.

We should, though, be well aware that the reports of these meetings do not present the whole truth. The meetings mainly dealt with the possibilities for research output transfer. What is not mentioned, for example, is the contribution that the research work made to the PDs' patent position. A more complete analysis of the relationship between the Nat.Lab. and the PDs would probably lead to a more positive image than that seen here. The conclusion about lack of commitment therefore mainly refers to the debates on transfer surrounding new product ideas.

The Corporate Research Exhibitions

In 1959 the Nat.Lab. organised an exhibition for the PDs to show exactly what research was expected to yield output for them in the near future. From 1959 onwards such demonstrations were held every other year, and it is in 1967 that we see for the first time the name Corporate¹⁴⁷ Research Exhibition attached these biennial events (from 1973 on they became annual events). The catalogues of the CREs are fairly representative for the research programme as a whole, in terms of the fields that were covered, except for a slight overrepresentation of television and radio research and

an underrepresentation of research into magnetic materials. Not only the research taking place at the Nat.Lab. was presented, but also the research continuing in the foreign labs was demonstrated.

The first 'CRE', in 1959, lasted one day. Nine rooms had been fitted out for demonstrations, which could be visited one after another. In each room there was a series of demonstrations, each lasting for about 5-10 minutes. In 1963 the programme was spread over two days, and there were parallel sessions. By 1965 the event had even spread to two locations, because by that time part of the laboratory had already been moved from Strijp to Waalre. Non-product-oriented research was not excluded from the programme; the topic groups were miniaturisation and microtechnology, various devices, magnetism and superconductivity, digital techniques and systems, measurement equipment, tubes, television, radio and acoustics, transmission, electro-mechanical research, chemistry and technology, and light. In 1967 a symposium on a specific theme was added to the exhibition, namely microwaves.

Transfer of Personnel

Another mechanism for influencing the company which the Nat.Lab. possessed was that of the transfer of personnel to other parts of the company. Holst had already expressed the view that the time spent in the research lab would be very advantageous to people who were transferred to the company's factories.¹⁴⁸ Having been trained to solve difficult problems, such people could fulfil useful tasks in the factory labs. Doing fundamental research in the Nat.Lab. was thus not only something that was advocated by the possible long-term effects for the company's product portfolio, but also by its 'educational' effect.¹⁴⁹ The number of scientists transferred to the PDs was not higher than in the Holst period. Between 1923 and 1948 100 scientists had moved from the Nat.Lab. to the factories, and about 150 had come to the lab.¹⁵⁰ In the years 1946-1955 about 50 had left to go to the PDs, and about 90 had come to the lab.¹⁵¹ This means that the ratio of scientists going to scientists coming had somewhat decreased. Between 1946 and 1972 most of the transferred scientists went to the PD Elcoma. The 1946-1955 outflow consisted of physicists, chemists and electrical engineers in about equal numbers.¹⁵² Fewer scientists went to the PDs Licht (Lighting), Telecommunicatie Industrie (PTI, Philips Telecommunication Industries), Producten voor Industriële Toepassingen (PIT, Products for Industrial Applications), and Röntgen en Medische Apparaten (X-ray and Medical Equipment). Only a few scientists went to other PDs. It was clear that when it came to the transferring of scientists, there was a bias towards the professional PDs. The number of people transferred remained small compared with the number of people who stayed with the Nat.Lab.¹⁵³ The fact that most people preferred to remain in the Nat.Lab.

was something that also emerged from a survey held among lab personnel in 1969. It appeared that almost 50% had never even considered transferring to some other part of the company. Almost 30% saw no realistic possibilities for that. About 5% had tried and failed. When asked to consider the possibility of transfer, about 75% of all scientists expressed the desire to stay with their current group. According to the survey, the preference to stay in the Nat.Lab. was remarkably high compared with percentages in the rest of the company.¹⁵⁴ Apparently, transferring people in order to influence the company was not without problems either.

Assessment of the Relationships with the PDs

In spite of the variety of ways that the Nat.Lab. had at its disposal for exerting an influence on the PDs, the overall impression is that increasing the number of formal meetings certainly did not guarantee better transfer. This impression is confirmed by the report produced by a special committee, called the Efficiency Contact Committee, which in 1963 reported to the Board of Management on the contacts between the research labs and the PDs. The outcomes were not very positive: it was stated that in general a smooth transfer from research to the PDs was certainly not guaranteed. The observation that there were no systematic, organised contacts between the research management and the PD management was, so the committee believed, one of the reasons for this.¹⁵⁵ The existing contacts (described above) were rather informal. In 1968 a critical report about the Nat.Lab.-PD interface once again came out. The report had been produced by a committee led by Dr. J.J. Verschuur and was entitled 'Market Research and Development & Operations Research'. This report was also critical about the exploitation of research. The report opened with two underlined statements: 'we are strong in our research; we may have been offering too many uncorrelated results to our Product Divisions'. The cause of this was seen as lying in the interface problems (again underlined in the text). An appendix that gave information on the distribution of the total research effort over the various market sectors, and in relation to that, over the various PDs, accompanied the report.¹⁵⁶ The numbers have 1967 as their reference year. The breakdown was as follows: 103 graduates and 6 assistants worked on 'fundamental research' (not for any particular PD), 57 graduates and 20 assistants worked on general services (for internal Nat.Lab. use), 95.5 graduates and 33 assistants worked on Elcoma topics, 37 graduates and 20 assistants concentrated on consumer equipment, 281 graduates and 103 assistants devoted their time to professional equipment and systems, and 18 graduates and no assistants focused on lighting. So there was a bias towards professional products. The research effort was also compared with data on the West European market in the indicated sectors, as far as electronics was concerned. The numbers revealed that the

16% research effort devoted to Elcoma issues related to a 24% contribution on the part of these issues to the total market income. The 6% research effort expended on consumer products seemed rather small compared with the 23% of the company's total market income that these products covered. For professional products there was a good match: 48% of all the research was spent on what yielded 48% of the company's market income. Lighting seemed to have been somewhat poorly endowed: 3% of the research for what yielded 6% of the total market revenues. This confirmed the notion that the support that the Nat.Lab. offered to the PDs was relatively more targeted towards the professional PDs than the consumer PDs. In a way this was not surprising because the professional PDs (and Elcoma) more or less had the same outlook as the Nat.Lab. PIT for example produced measurement equipment that had been developed by the Nat.Lab. for its own use. W. Knippenberg, formerly a group leader at the Nat.Lab., revealed how the intense contacts between the analytical chemistry Nat.Lab. group and the PD always enabled him to identify what sort of analytical equipment would be useful to work on.¹⁵⁷ Another topic on which the Nat.Lab. and the PD PIT agreed was that of the cyclotrons. The research for this was done in the branch in Geldrop, in close co-operation with the PD that delivered the cyclotrons to customers in Buenos Aires, Paris, Lyon, Göttingen, Saclay, Amersham, Amsterdam, Groningen, Petten, Langley Field, Hamburg and Zürich. What is illustrative for the close co-operation with PIT is a remark made by G.T. de Kruijff, who from 1956 to 1970 was involved in this co-operation. According to him, people could sometimes hardly tell if they officially belonged to the Nat.Lab. or to the PD.¹⁵⁸ It was quite a culture shock for De Kruijff when, in 1974, he moved to the PD Audio and was confronted there with a quite different attitude towards the Nat.Lab. Once a colleague there told him: 'The Nat.Lab.? I never go there. It is useless.' J. van Nieuwland, who (much later) had undergone a similar change, having moved from the Geldrop cyclotron group to the PD Audio, intimated that Nat.Lab. scientists sometimes joined PD developers when they went to their customers.¹⁵⁹ P. Kramer, who before becoming group leader in optics had worked in Geldrop on cyclotrons, also referred to the good contacts with PIT. According to him, the Nat.Lab. was well able to push certain products onto the market via PIT.¹⁶⁰

While the Nat.Lab. thus had a definite impact on the professional PDs, the consumer PDs brought out some important products that the Nat.Lab. had not been involved in at all. An example of this is the compact cassette. It had been solely developed by the RGT development laboratory¹⁶¹ without any Nat.Lab. intervention, although the system aspects of the product would have made Nat.Lab. involvement plausible. The stereo gramophone was also primarily the result of RGT lab work.¹⁶² Likewise, the PD Light brought out several new types of lamps that the

Nat.Lab. had not been involved in.¹⁶³ Although in the Holst period there were also some examples of products, in whose development the Nat.Lab. had not been involved (probably the best known being the shaving device), in the Casimir period the number of such products became relatively larger.¹⁶⁴ This again illustrates how the relationship between the Nat.Lab. and the factories/PDs had changed.

The relationships which existed with the PDs were often discussed at the CRC and RDC meetings. In those discussions there was more emphasis on the problems than on the good contacts, the possible reason being of course, that good contacts need no debate. In that sense the impression these meetings create is biased. However, because there was a relatively frequent need to discuss problems, this would seem to indicate that they were not problems that could simply be dismissed. A number of problems and causes for those problems were identified at the CRCs and RDCs. There were complaints about the slowness of the transfer of research output to the PDs¹⁶⁵ and about the lack of effort on the part of the PD to take research output seriously.¹⁶⁶ It was felt that the Nat.Lab. had little impact on the PDs' long-term product policy and that production quality could be improved if the Nat.Lab. were more involved as an advisory body.¹⁶⁷ At the same time, the research managers complained about lack of guidance from the PDs in the desired long-term research.¹⁶⁸ In the field of development-oriented research, the Nat.Lab. complained about the lack of information (e.g. about the market) emanating from the PDs.¹⁶⁹ All in all, the debates in the CRCs and RDCs do not show a great deal of self-criticism. It was mostly the PDs that were are blamed for the difficulties in transferring research output. As we saw with the directors' contact meetings, the PDs had their arguments, too. It would be rather one-sided to make it look as if the PDs were entirely responsible for all the tensions in the relationship between the Nat.Lab. and themselves. It would be fairer to see mutual lack of commitment as the main cause of this. It is precisely the reversing of this lack of commitment that would be the target of the actions to be taken in the next period, between 1972 and 1994. This road towards mutual commitment would take about two decades to traverse, which shows how deeply rooted the problems had become in the years 1946-1972.

All these tensions, however, cannot detract from the fact that in the 1946-1972 period there were some substantial successes which meant that the Nat.Lab. made a very important contribution to the company. In the next chapter we will examine two of the most prominent successes, namely the Plumbicon and the LOCOS process. Apart from those two big successes, many new insights were being gained into materials research (see section 5.2) that were to have a more subtle (i.e. less easily traced) influence on the

development of new products. It was not possible to make an overall survey of the Nat.Lab.'s contribution to the company in terms of patents, licence incomes and cross licences. That would have been a useful way of quantifying one aspect of the contribution of the Nat.Lab. to the Philips company. This lack of organised data makes it difficult to assess the effectiveness and efficiency of the approach taken by the Nat.Lab. in the 1946-1972 period, but the Nat.Lab. did certainly continually contribute to the company's patent position.

One may question whether major successes of the sort achieved by the Plumbicon and LOCOS would ever have been achieved without the great freedom given to the researchers and the opportunities they had to study the phenomena underlying the various new products. In that respect the Nat.Lab. approach in the 1946-1972 period must be judged in the context of the circumstances of those years. There were many chances to launch new products onto the market and the company could afford to have a laboratory with great scientific freedom. This freedom may have caused a lot of personal frustration in the contacts maintained between the Nat.Lab. and the PDs, but it certainly resulted in a number of great successes.

6. The Practice of Autonomous Research

In Chapter 5 the general characteristics of the Nat.Lab. as a professional organisation in the 1946-1972 period were described. In this period some important technological developments took place, such as the emergence of television and semiconductor devices, such as transistors and ICs. In this period the Nat.Lab. maintained that its main role for the company was to do research to support the PDs in developing their product policy, and in particular their long-term policy. At this time the economic circumstances were favourable, and there were ample opportunities for developing a long-term policy to explore new markets. The Nat.Lab. envisioned playing an active role in this rather than continuing its follower-behaviour of the previous period (under Holst). To do that, a fair 'fundamental' research deal, aimed at gaining a deeper understanding of the natural phenomena on which perhaps in the future new products could be based, was seen as essential. The lab continued to do research into phenomena underlying existing products in order to support (further) development of such products. The same sort of work had been done in the Holst period. In that sense there was a continuation of the function of the Nat.Lab. as a source of knowledge for the company. With this development-oriented work the Nat.Lab. believed that its task was to solve the more 'fundamental' problems and to develop product ideas, which, at an early stage, could be transferred to the PDs' development labs. Both types of tasks (long-term oriented and development-oriented tasks), a good feel for the latest scientific development and great freedom for researchers became important features of the Nat.Lab.'s culture. At the same time, a hierarchy for controlling the totality of the research programme evolved, in which levels of managing directors, directors, group leaders, scientists, assistants and technicians all had a specific function.

Various mechanisms were used to exert an actual influence on the PDs. In practice, it appeared that there was a lack of mutual commitment that hampered effective contact between the Nat.Lab. and the PDs as far as the discussion of new products was concerned and which created tensions at the management level on both sides (where such debates took place). Regarding the influence that the Nat.Lab. had on the PDs, there was a certain bias towards the professional PDs. One of the mechanisms that was used to influence the PDs was the transferring of people from the

Nat.Lab. This often resulted in good informal contacts on the workflow between former Nat.Lab. employees and their colleagues who had stayed in the Nat.Lab. These contacts worked well when it came to practical (production) problems. The lab management did not see this as a core task of the lab, but it was realised that such contacts were useful for maintaining good contacts at the workflow level, so they were not opposed to a limited amount of time being spent on this.

As in Part I, a number of case studies will be used to illustrate the role of the Nat.Lab. within the company. The case studies selected reflect a balanced view of the effectiveness of the Nat.Lab. in the 1946-1972 period in that both its successes and failures (in terms of commerce) are included. At this time the PDs may have had the feeling that their influence on the research programme was minimal, but besides all the knowledge developed for the company, the Nat.Lab. certainly made some substantial contributions to the company's commercial success. Two of the case studies tell the story of two such 'big hits': the Plumbicon television camera pickup tube, and the LOCOS process for ICs. On the other hand, there are examples of research efforts that did not become business successes, and which resulted in the Nat.Lab. and the PDs blaming each other for the failure. Examples of that are the Stirling engine and the Video Long Play.

6.1 The Stirling Engine

Introduction

The history of Stirling engine research has already inspired several people to write about it. The titles of the publications are not usually neutral: 'Promise and disappointment: the history of the hot-air engine',¹ or 'The Stirling engine: a Fascinating Failure'.² Titles like these appeared even while, in several places in the world, Stirling research was still continuing. Maybe the fact that Philips had, at a certain moment, abandoned this research had had an impact on the general image of this type of research. People no longer seemed to believe any more that the Stirling engine could ever become successful as a clean, cheap, compact and silent energy source, in spite of all the current efforts in that direction. To quote G.T. de Kruijff, former director of Philips' 'Products for Industrial Applications' (Dutch abbr.: PIT) PD: 'The Stirling engine is very promising and will always remain so.'³ And perhaps this pessimism is justified, because the Nat.Lab. certainly gave the Stirling engine a fair chance. Stirling research has been one of the longest lasting of all research lines, and there were years when the Stirling group was one of the largest in the laboratory.

The research carried out on the Stirling engine in the Philips Nat.Lab. is a clear example of research that was kept alive for a long time because of the enthusiasm of the people working on it. That was something that had begun with its 19th-century inventor, Robert Stirling, who was not an engineer, but a preacher. What, one might wonder, motivates a preacher to spend his leisure time on engines and even on submitting a patent request for an engine? Was it a passion for the people who had become victims of exploding steam kettles? Was it that same passion that later motivated the Philips researchers? Or was it a scientific passion for elegant thermodynamic processes? The principle looked beautiful because of its simplicity: by alternately making a gas expand in a hot space and compressing it in a cool space, heat was consequently converted into mechanical power. The main Stirling engine biographer, C.M. Hargreaves, wrote the following words that went around in the lab in about 1965: 'the engine was driven by cranks, the research on it by eccentrics'.⁴ The key figure in the research, R.J. Meijer, revealed how once after a successful demonstration of the engine at H. Rinia's jubilee, Rinia was supposed to announce that the research was to be stopped, but that he had no courage to raise the subject because Meijer was so enthusiastic to tell him about the research.⁵ During the same event F.J. Philips became interested in the Stirling engine, and at his own jubilee, in 1970, he received a lawn mower with a Stirling engine as a present.

The contrast between the passion of the researchers and the disappointing outcome of the Stirling engine at Philips (neither the acquired knowledge nor the product was to become relevant for the company) is what makes this an intriguing case study. It shows how complex the relationship between science, technology and society is in the context of an industrial research laboratory, particularly when a supply-oriented approach is taken.

The Initiation Period

It is not entirely clear upon whose request the Stirling research was started in the Nat.Lab. Some say it was the Apparatus Factory that put in a request for a new energy source. Others say it was Dijksterhuis in the Radio Factory, who put in such a request to Rinia. How the idea of the Stirling engine was discovered by the Philips people is also unclear. Here also there are two stories: either Rinia saw the engine in Utrecht, or Holst saw it in Berlin. The first researcher was H. de Brey, appointed in 1937 (or 1938) by Oosterhuis. Whatever the correct story may be, in both cases the Stirling engine derived from contact with a factory, and immediately a market for it was envisioned. That fits in well with our overall impression of the 1923-1946 period: in the diversification of the Nat.Lab. research, and factory activities often went hand in hand.

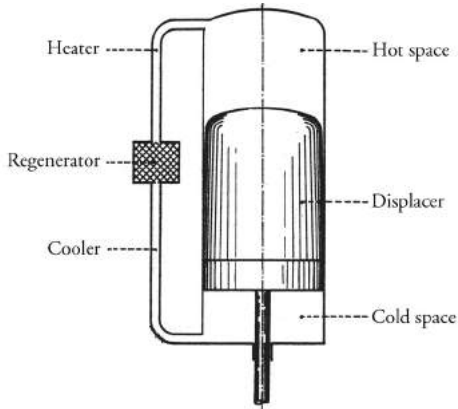
The market envisioned for the Stirling engine will be described below. The hot air engine – the name Stirling engine was not used at that time – seemed to be an appropriate solution in the search for a silent, compact energy source for radio transmitters and receivers in developing countries. With this device a wide range of fuels could be used as input. Even though the efficiency of the engine was rather poor (about 0.2%⁶), it was evident that there was a large potential for substantially improving it by using new knowledge on heat transfer and gas flows. De Brey thus was able to realise improvements in quite a short time by replacing the cavity that originally functioned as a regenerator – the part that Stirling had invented for enabling the gas to temporarily store its heat when passing from the hot to the cold space – with wound chromium wire.

In 1939 the first patent was applied for, and in 1942 it was granted (it was No. 51975). The first actual engine was built in 1938 by Leblans, and it operated at a rotational speed of 1500 rpm; it produced a power of 16 watt.⁷ During its development there had been co-operation with the outside world: together with the Shell lab in Delft, a special paraffin burner was devised.⁸ In 1939 F.L. van Weenen joined the group and somewhat later Du Pré, Van Heeckeren, Clay, Stigter, Mettievier Meyer and Schultz also joined. By 1940 the group had developed an engine that ran at 4.000 rpm and yielded 1.5 hp.⁹ The efficiency was 29%, which was quite an improvement compared with the original 0.2% with which the group started.

World War II does not seem to have had a great impact on the Stirling research. That impression is confirmed by the fact that soon after the war ended, articles on the hot air engine were being published in the *Philips Technical Review*. Even as early as February 1945, newspaper articles appeared with titles such as: ‘Revolution in the field of engines?’ During the war, in 1942, what was to become known as the double-acting engine was invented by Van Weenen. A patent was applied for in 1943.¹⁰ It became important much later.

The only concern in this period was to divert the attention of the Germans. A story is told that shows how the researchers were successful in doing that: the Germans believed that they were working on a new type of fuel, and by the end of the war they took with them a cylinder of this new fuel, only to find out that it just contained ordinary air.¹¹ In wartime the famous ‘bungalow set’ was also developed. It was fuelled by kerosene and was to be used as an emergency generator in 1953 during the great flood disaster for the south-west part of the country.

After the war military interest for the Stirling engine emerged in the USA. A group of ten Nat.Lab. researchers moved to the USA to do research into the possibilities of using the engine in submarines.¹² There



Principle of the displacer-piston system. Moving this piston up and down causes the gas to be transferred back and forth between the hot and cold spaces, via heater, regenerator and cooler.

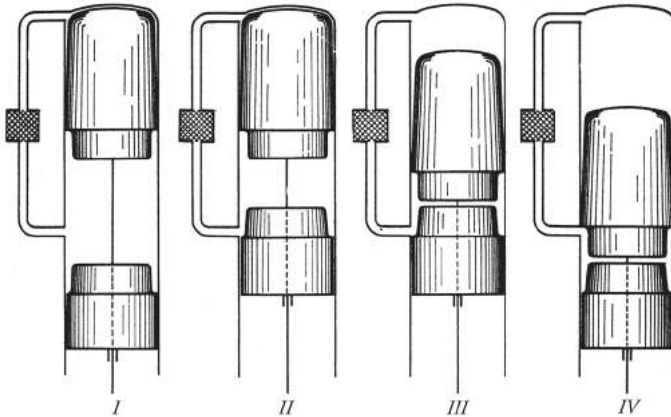
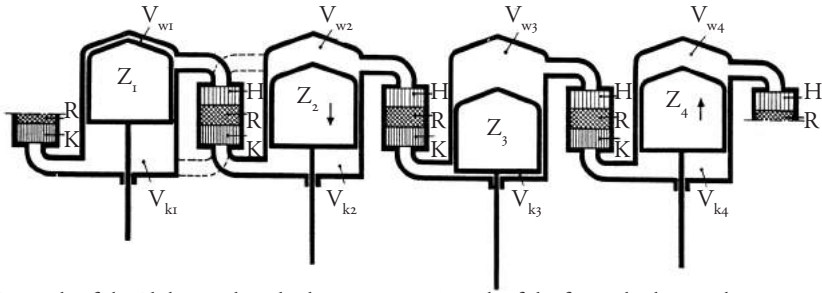


Figure 15. The principle of the hot-air engine (from *Philips Technical Review* Vol. 20, p. 247).

In the cylinder there are two pistons, one of which is called the displacer piston. The air in the cold space (the lower part of the cylinder) is compressed by the power piston (from I to II). This requires work on the part of this piston. The air is then transferred to the hot space (the upper part of the cylinder) by the displacer piston, and via the regenerator where stored heat is absorbed (from II to III). The air then expands to its original volume, thus performing work (from III to IV). Due to the fact that the pressure is lower in the cold space, the work that was needed there is less than that performed in the hot space. The air is then returned to the cold space, again via the regenerator, where heat is stored (from IV to I). A burner and a cooler maintain the differences in temperature between the hot and the cold space. The energy this requires is equal to the difference in spent and gained work. Thus, energy is converted into work.



Principle of the Philips multi-cylinder air engine. In each of the four cylinders is a hot space at the top, V_{w1} to V_{w4} , and a cold space at the bottom V_{k1} to V_{k4} . The hot space of each cylinder is connected via a heater H, regenerator R and cooler K, with the cold space of the next cylinder. The pistons Z_1 - Z_4 of the successive cylinders move with a suitably chosen phase difference (in the case shown with four cylinders this is 90°).

Figure 16. The double-acting engine (from *Philips Technical Review* Vol. 9, p. 131). In each of the cylinders pistons Z_1 - Z_4 serve a dual purpose: both for its own cylinder and for the next one. Thus, a very compact multi-cylinder engine is constructed.

were several research topics represented in the group: thermodynamics, regeneration, heat transfer, gas flow resistance, aerodynamics, control technology, heaters and lubrication, design/construction/balancing, materials science, sintering, hard soldering, and welding.¹³ For the Eindhoven group this was quite a heavy loss; after they left, there was a shortage of personnel.¹⁴ In 1948 the group returned, because the prospects for success were too bleak (there were considerable problems with the lubrication and the heavy and costly wobble plate).¹⁵ Officially, their activities were coordinated by the Philips lab in Irvington. In 1949 Stirling activities were taken up again, but then in the area of cooling¹⁶. In the Netherlands an agreement was made with the N.V. Werkspoor that was to result in a joint venture called N.V. Thermomotor. In 1947 an agreement was made with the N.V. Samenwerkende Motorenfabrieken (Co-operative Engine Factories, Inc.), which was active in the field of shipbuilding. The work done with this Kroon group suddenly ended after one of the engines exploded, and a worker was killed. As in Irvington, the work on the refrigerating machine was initiated in Eindhoven, too, and it was J.W.L. Köhler who was put in charge of that. Den Haan became group leader of the engine group. In 1953 attention was to shift from the engine towards the refrigerating applications.

In 1946 a commercial machine group was set up by the company. The new group was to produce engines in the city of Dordrecht. The idea of forming this group probably originated during the Orco meetings, some of

which were dedicated to Stirling engine activities. The people attending those meetings not only considered the original idea of radio transmitter and receiver application, but also a 100 W generator for lighting, a 1000 W generator for household appliances, an engine for small boats and a small backup engine.¹⁷ A special committee was established, which consisted of representatives of the company's directorate (F.J. Philips, Loupart and Van Walsem), the Nat.Lab. (Rinia, Horowitz, Van Weenen, Du Pré and Lambeek), the sales department (Gelderblom), the patent department (IJzer) and Werkspoor Inc. (De Fouw).¹⁸ In a Personnel Survey we find records of the following numbers of workers: 43 scientists and 20 technicians in the Nat.Lab. and 18 people in the engine lab in Dordrecht (plus 12 who worked on 'licence development').¹⁹ Such direct contact between the company's directors and the Nat.Lab. was not peculiar to the 1923-1946 period, as we saw in Part I. But this case study shows us that things will change when we move into the next period, 1946-1972. The commercial ambitions regarding the Stirling engine resulted, in 1952, in the emergence of a 'founding stage' PD. In 1947 the idea of such a PD had already been raised with the company's directorate.²⁰ It would, however, maintain this name until it was abolished in 1953. When, in 1954, the refrigerating machine was taken over by the PIT (Products for Industrial Applications) PD, the engine was not taken over. The reason for that may well have been that the originally envisioned market (radios in developing countries) no longer existed. Meanwhile, the transistor had replaced radio valves, which used much less energy so that a battery was sufficient to provide all the energy required. Even suggestions for various other possible applications, such as the ones that were known to the Werkspoor and Kroon groups, did not apparently convince the PD of the usefulness of taking over the engine from the Nat.Lab. Indeed, this fits in well with our overall picture of the 1946-1972 period: PDs were selective when it came to transferring product ideas from the Nat.Lab. Already in 1948, doubts about the commercial feasibility of the hot air engine had been expressed by the company's department of CV&P (Commerciële Voorcalculatie en Planning, i.e. Commercial Preliminary Calculation and Planning), that even refused to perform market research as long as there was no operational prototype and the price, efficiency and characteristics were not known (and – a nice remark was made – as long as Mr. IJzer in all seriousness proposed delivering a bicycle pump with the engine to enable it to be started up²¹). In 1949 a report was produced by Den Haan, Kwant, Van der Leeden and Van Weenen, and in that report it emerged that they mutually disagreed about the commercial feasibility of the engine.²² That was one reason for the Board of Management to announce that all Stirling activities within the Nat.Lab. should be ended. The final annual report of the 'founding stage' PD mentioned that the development group had moved from Dordrecht to Eindhoven, which had yielded an annual budget reduction of

NGL 73,000.²³ The main unsolved problems were the lubrication of the double-acting engine, the burners/heaters, regulation of pressure and the amount of fuel, as well as operational safety and cost.

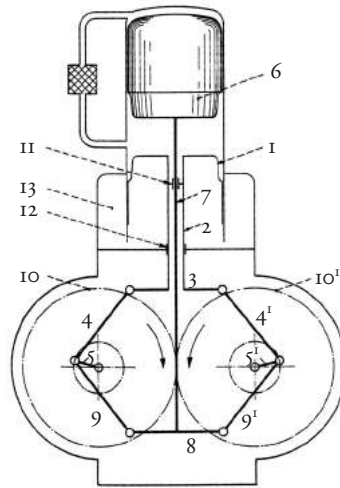
The Stirling Engine as a Car Engine

In 1954 Meijer was permitted to continue the Stirling engine research on his own, with just some technical assistance (in the introduction it was mentioned that this was because Rinia could not find the courage to tell the enthusiastic Meijer that the research had to be stopped). In 1960 Meijer successfully defended his doctoral thesis on what was to become known as the rhombic drive, which he had invented.²⁴ This rhombic drive brought considerable improvements for the engine because it allowed for perfect balancing of a one-cylinder engine and for reduction of the size of the engine.

The meetings, which were usual two-weekly, were continued, and according to Hargreaves, the group had already grown again to 20 people in 1955, but Meijer was the only scientist. In 1956 a second scientist, Fokker, joined the engine group for half of his working time. The other half he spent on the refrigerating machine. In 1960 Meijer was made the official leader, by which time there were 30 people in the group, including 3 scientists. In 1964 the group consisted of 50 people, and in 1969 there were 70, 16 of whom were scientists!

The reason for the dramatic growth in the size of the group was the establishment of certain contracts with external parties. In 1958 after many months of negotiations, a contract was drawn up with General Motors in the USA. The year before that Köhler and Meijer had gone to GM to demonstrate the engine.²⁵ GM's primary interest was not in a car engine. Köhler and Meyer had even explicitly advised against that.²⁶ The contract was drawn up with GM's Allison division that was engaged in developing space technologies and satellites. The Stirling engine was seen as a possible source of energy in satellites. The US Air Force had given Allison the assignment to work on this. For the developers this constituted quite a challenge, since the engine would have to function for a long time without maintenance. In 1966 GM gave up the idea,²⁷ but it would not be long before the application for car engines would come to be recognised as an option, even though Rinia had once predicted that this would probably be the least suitable field of application for the Stirling engine.²⁸

In 1965 contact was made with MAN-MWM, a German firm that constructed coaches and trucks.²⁹ The year before that, the contract with Werkspoor had ended. By then quite an amount of money had already been spent on Stirling engine research (probably as much as NGL



Schematic diagram of rhombic drive mechanism.

- 1 = power piston.
- 6 = displacer pistons.
- 5-5' = cranks in two shafts rotating in opposite senses and coupled by gears 10-10'.
- 4-4' = con-rods pivoted from ends of yoke 3 fixed to the hollow power-piston rod 2.
- 9-9' = con-rods pivoted from ends of yoke 8 fixed to displacer-piston rod, which runs through the hollow power-piston rod.
- 11 and 12 = gas-tight stuffing-boxes.
- 13 = buffer space containing gas at high buffer pressure.

Figure 17. The rhombic drive mechanism by R.J. Meijer (from *Philips Technical Review* Vol. 20, p. 251).

The rods 3, 4, 4', 8, 9 and 9' make a rhombic figure that is connected to the moving displacer piston 6 (rod 3 is fixed, rod 8 moves up and down with the displacer piston). The high pressure in the engine and buffer space (13) is sealed off from the rest of the engine by 11 and 12.

6,500,000 in the 1946-1964 period and NLG 1,490,000 in the 1954-1964 period for research, and another NLG 7,720,000 for the 'founding stage' PD).³⁰ In 1968 another important contract was drawn up, namely with United Stirling in Sweden. Here, too, traction applications were the primary interest: for city buses and mine vehicles. A feasibility study had shown that the latter idea was possible but would require a radiator that would need to be three times as large as usual.³¹ The contract with United Stirling would continue until 1979, when Philips stopped all its Stirling activities. For both MAN-MWM and United Stirling the contract provided the opportunity to send engineers to Eindhoven to learn about the Stirling technology. Thus, in 1967, two MAN-MWM engineers stayed at the Nat.Lab. for some time, and in 1969 another seven arrived from Unit-

ed Stirling. Meanwhile, all the Stirling knowledge was of course being carefully protected by patents. Through Dr. Duffendack, who was in charge of the Philips lab in Irvington in the USA, there were some contacts with other external parties, such as the American Gas Association and the Steward Warner company. In 1971 there was the 3C-action, in which three companies (Cummings, Continental Motors and Caterpillar) were involved.³²

As we saw before, the Stirling activities did not lead to any commercial activity within the Philips company (the 'foundation stage' PD was in the end abolished). Even though the range of products within the company was quite wide, the engine did not fit well into this range. The consequence was that the Stirling group had no direct relationships with other parts of the company.³³ Within the lab there were several contacts, but probably only because the group sought services from others. For example, the group made use of the PASCAL computer in the lab.³⁴ To quote R.J. Meijer: we could go to anyone, but nobody interfered with us.³⁵ In terms of discipline areas, there were hardly any relationships between the Stirling group and all the other research groups. Hargreaves even called the group a 'thermodynamic island within the lab'.³⁶ The results, though, were not kept internal in the Nat.Lab. but were presented to other Philips organisations at the Corporate Research Exhibition on a number of occasions (in 1963, 1965 and 1969).

In 1970 another effort was made to start a commercial activity. That was when the Product Group Stirling Engines (Dutch abbr. PSM) was initiated.³⁷ This venture did not continue for very long either. Pannenburg had stimulated this effort, but Meijer had been against it from the beginning. His argument was that the group did not yet have a marketable product available. In the 'Kern' there was considerable concern about the fact that around 45 people were moved to the new Group.³⁸ There was certainly good reason for this concern because a report by Fry Consultants had shown that for traction application, a careful study into the opportunities should first be made.³⁹ By 1971 the group had already been dissolved. In co-operation with the personnel department (Dr. Simon Valkenburg) the staff was moved back to other groups. Some of them returned to the Stirling group in the Nat.Lab.⁴⁰ A range of research topics was represented in the Stirling group. In a Progress Report we find the following topics: control technology, temperature regulation, sealing, the wobble plate, burners, heat transport and storage, heat pipes, and electrical and mechanical services.⁴¹ Most of these topics had no clear relationship to the rest of the research programme.

Curiously the end of this period links up with the new environmental legislation being created in the USA. The new Clean Air Act imposed much

stricter conditions onto the composition of exhaust gases being emitted from car engines. Although nowadays we might expect this to be favourable for the Stirling engine, General Motors responded by giving priority to improving the internal combustion engines to make them comply with the new legislation. They expected this type of engine to still be the dominant technology at least in the near future.⁴² The same legislation was to provide the motive for another American car company, Ford, to approach Philips in a search for co-operation in a new Stirling engine phase: the environmentally friendly engine.

The Stirling Engine as an Environmentally Friendly Car Engine

The idea that the Stirling engine might be an alternative to the internal combustion engine from an environmental point of view had in 1971, before the co-operation with Ford, already led to a grant application being submitted to the Dutch government for the development of a city bus engine. The timing for such an application was not bad: in the early 1970s a general awareness of environmental issues was emerging. In October 1971 Philips published the news that the government had granted it NLG 5 million over a period of three years to co-operate with the DAF company to develop an engine for a city bus.⁴³

The activities in the 1972-1979 period were, however, dominated by the co-operation with Ford.⁴⁴ Both the co-operation with DAF and Ford led to extensive discussion in the Corporate Research Conference in 1972. G.W. Rathenau introduced this discussion by pointing out the positive characteristics of the engine: its external combustion, low noise, and the thermodynamic efficiency of the closed cycle. The important parameters for further development were: weight, volume and price per hp, overall efficiency and maintenance. Rathenau stated that the market was small, but that there were good opportunities for usage in city buses, because the cost constraints would in such cases be less tight. In the debate that followed, the whole idea was considered uncertain. The same sort of conclusion was arrived at in the Research Directors' Conference of June 1973. Kauer, who was in charge of the Aachen laboratory at that time, mentioned the absence of production technologies and of an appropriate marketing channel as reasons for the lack of success up until then. He did, however, see the combination of heat storage systems and the Stirling engine as a possibly interesting option, because in that way a 'zero emission vehicle' could be obtained.

In 1973 the group was moved from Rathenau's 'Physics' main group to De Haan's 'Applied Physics and Mechanics' main group. Meijer had four groups by then: 'Stirling engines' managed by Van Beukering and Spigt (16 scientists), 'Mechanical Research' led by Muiderman (5 scientists),

‘Stirling Refrigerators’ supervised by Prast (5 scientists) and ‘Electrical and Mechanical Engineering’ under Rietdijk (9 scientists).⁴⁵

In 1976 a demonstration was held for Ford with a Ford Turino with an inbuilt Stirling engine and with the DAF city bus, which was flown over to the USA just to participate in this demonstration. The demonstration was a success, and the requirements, in terms of emissions, were met.⁴⁶ It had cost, however, a lot of effort and much inventivity. Probably no engineer would dare claim that the expectations had really been fully met. The engine was still quite heavy, the efficiency did not surpass 25%, and its reliability was not very high. The demonstration, though, marked a milestone in the Philips-Ford co-operation after which considerations concerning the next phase were embarked on. Growing problems among American car companies to a large extent determined these considerations. Apparently, a feeling of uncertainty quickly rose, because the Stirling group in the Nat.Lab. had been reorganised not long after the considerations had commenced. This reorganisation was substantial enough to cause a debate in the Kern on August 24, 1976. A number of people were transferred to Spigt’s energy group, and a number of workshop people were moved to the mechanical department (‘administratively’ as it was put in the minutes of this Kern debate). Meijer became an advisor for Ford in the USA.

One important stimulus encouraging co-operation with Ford was the grant that Ford had received from the American Department of Energy (DOE): \$110 million for a period of eight years, to which Ford itself added another \$50 million.

The activities were not sustained until the end of the eight-year period. In 1978 it was announced in the Philips Board of Management that Ford had cancelled the Stirling contract and stopped all activities. The reason for this was not explained. There are no indications that there had been any specific problems with the project. It seems plausible to believe that Ford just did not want to take further financial risks, not even with partial government support, given the unfavourable economic climate of that time. Hargreaves ends his biography with the remark that in particular it was the expected price of the engine that was the main obstacle to success: in 1978 the Stirling engine was still not able to compete with the internal combustion engine. In an interview⁴⁷ Meijer also mentioned production costs as a reason for delaying mass production of the Stirling engine.

The dissolution of the Stirling group fits with our overall impression of the 1972-1994 period (see Part III) because it was in this period, more than before, that the interests of the Product Divisions came to be used as a criterion for continuing or ending projects. The Stirling engine group was not the only group that did not fire the interest of any PD and that

was dissolved at that time: the biology group was dissolved, too. In the Kern discussion the main motive for dissolving (or ‘reducing’ as it was called, because some energy research was continued) the Stirling group was given as being the general research policy in which focusing on spearheads was regarded to be important.⁴⁸ Again, Valkenburg was involved in dealing with the personnel aspects. Rietdijk had already announced on December 6, 1978 that there would be a substantial reorganisation and that his preference would be to finish this within three months in order to restore everyone’s peace of mind as soon as possible. It certainly did not go as fast as that.

Meijer, who had already moved to the USA in connection with his advisory work for Ford, decided to stay there permanently and to start his own company, which was called Stirling Thermal Motors.⁴⁹ Through Pannenborg he obtained the necessary rights to use the Stirling knowledge. Together with Benjamin Ziph, who had come from the Philips lab in Briarcliff Manor, he was soon able to acquire some contracts. He kept in touch with F.J. Philips in the Netherlands, who remained active in the promotion of the Stirling technology. The refrigeration application side was continued in an independent way under the name of Stirling Cryogenics & Refrigeration, and it is still located at the Philips plant grounds in Acht, near Eindhoven. Even today the Stirling engine is still sometimes mentioned in the newspapers from time to time, but otherwise all the promises remain unfulfilled.

In the case of the Stirling engine, a new research line emerged from a concrete request made by a factory. That perfectly illustrates the ‘follower’ behaviour of the Nat.Lab. in terms of the company’s product portfolio, which was typical for the 1923-1946 period. As happened often during that period, an effort was made by the company’s directorate to derive from that a new industrial activity. The Stirling story then takes us into the next period, 1946-1972, in which we find that autonomous PDs deal with the research output in a more selective way. In this, it was the PD PIT that decided to take over the refrigeration machine, but not the engine, from the laboratory’s Stirling group. Stirling activity ended in the next period (1972-1994), in which – as we shall see in Part III – there was a stronger tendency to concentrate on those issues for which there was evident interest among one or more of the PDs.

The case study of the Stirling engine also shows how important the role of individual researchers (like R.J. Meijer) was. We also learned about the roles of individuals in the case studies given in Part I, and this thread will run through the lab’s history characterising it as a professional organisation. Both Holst and Casimir, each in their own way, struggled to find a balance between allowing strong, individualistic scientists to have their freedom and controlling their activities in order to establish research programme coherence.

The Stirling group was certainly productive when it came to writing Reports and Technical Notes. In the 1946-1954 period we find in the Registers over 40 titles on Stirling technology, most of which have to do with the engine; in the 1954-1967 period there are over 60 titles and in the 1960-1967 period, 57 titles. In the years after that about 30-35 new titles appeared annually. With several projects that had been commercial failures, the claim was made that they had nevertheless yielded good spin-offs in the sense of providing new knowledge that could later be used in other projects. Thus – as we shall see later on – the VLP project ‘failed’, but at least it resulted in knowledge that was used to develop the CD. Similarly, the Mega project in ICs ‘failed’, but Philips today still uses the output generated in terms of knowledge in IC-technology projects. In the case of the Stirling engine, there were also some (albeit very few) examples of knowledge published in Reports and Technical Notes which later on were used to solve, for example, practical problems with corroding wire connections in a Swiss telephone exchange system. The fact that the group was fairly isolated in a disciplinary sense had certainly not been a stimulus to the transfer of knowledge to other parts of the lab or to parties outside the lab.

6.2 *The Plumbicon*

Introduction

This case study takes us into the field of television research. Both for the company as a whole and for the Nat.Lab., television was an important field. By choosing the Plumbicon to focus on, we limit ourselves to the broadcasting side. The Plumbicon is a pickup tube for television cameras. It was one of the great successes of the research programme in the 1946-1972 period. In that sense it resembles the next case study of this period: LOCOS (see section 6.3). In the IC world all companies were at a certain point ‘condemned’ to use the Philips LOCOS technology. Likewise in the television world, no company could get away from using the Plumbicon for professional television broadcasting.

The analogy goes even further: as LOCOS was inseparably connected to the name E. Kooi, so the Plumbicon was connected to the names E.F. de Haan and P.J.M. Schampers. Finally, a third point of analogy is that both LOCOS and the Plumbicon were first developed in the Nat.Lab. and only at a certain stage were transferred to the PDs. As we have seen, this was rather typical for the 1946-1972 period. We shall also see how selective the response of the PDs was, and that again was characteristic for this period: the ‘standard’ Plumbicon was transferred, but the Plumbicon with electrostatic focusing was rejected.

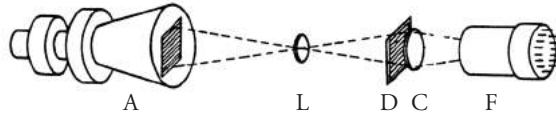
The Plumbicon was part of the device domain, though there were rela-

tionships with the domain of materials research, and with the systems domain. Quite different types of know-how were needed to make the Plumbicon the success it became: not only solid-state physics (traditional 'fundamental' research), but also a knowledge of vacuum technology and the know-how to produce stable layers of lead oxide were required. Therefore, we not only meet a theoretically oriented physicist like L. Heijne, but also a man who gathered insight in the processes by careful experimentation, like P.P.M. Schampers. In the contacts with the PDs, the complexity of the situation is again revealed: apart from Elcoma, the device PD, also ELA, a systems PD for the whole camera, was involved. The case study confirms what we saw before, which is that it was this structure of the company (with a separate PD for devices) that made contacts difficult.

All these ingredients make the Plumbicon case a fascinating one. In the description – as with the Stirling engine – a preliminary phase which preceded the actual research in the Nat.Lab. will be sketched. The research that led to the invention of the Plumbicon will then be described. Finally, the step from making single Plumbicons to producing in larger numbers in the factory will be discussed.

The Preliminary Phase of Transforming Optical Images into Television Signals: From Nipkov to Vidicon

The first successful attempt to transform an optical image into an alternating electrical current that could be transmitted as a television signal was embodied in the Nipkov system. With this system the light of a lamp was focused by means of a lens. In front of the lens there was a rotating disc with a series of holes in it that caused a light spot to scan a picture. The reflecting light was caught by a photocell, which then transformed the alternation of light and dark spots into an alternating electrical current. On the display side the opposite process took place: the alternating current was used to make a light bulb produce more or less light. With the help of a similar rotating disc, the change of light intensity was projected line by line so that the original picture was reproduced on the screen. Based on this technology John Logie Baird in the UK and Charles Francis Jenkins in the USA produced the first efforts to commercialise television. This resulted in a 'television boom' in the USA only (in the years from 1928 to 1932).⁵⁰ At the beginning of the television era, the Nat.Lab. also worked on the Nipkov technology. Druyvestein studied the selenium photocell and a special neon lamp, which was seen as a possible light source for the display side. Even when the Nipkov technology had been replaced by electronic technologies, Rinia was still able to achieve competitive results with this 'primitive' system. However, it had its limitations: the photocells were not very sensitive, and they did not respond swiftly enough to follow all the fast changes in the picture. In addition, the



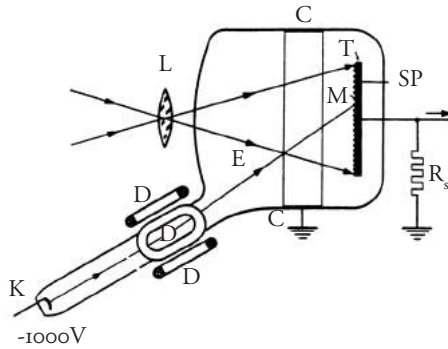
Principle of the flying-spot scanner. The electron beam of a special cathode-ray tube A (the scanning tube) describes a frame on the screen, which is projected onto the flat transparent object D by the lens L. The condenser lens C collects the light passing through and throws it onto the photo-cathode of the multiplier tube F.

Figure 18. The flying-spot scanner (from *Philips Technical Review* Vol. 15, p. 221).

mechanical disc placed great limitations on the number of lines per image and on the number of images per second that could be realised. Both were important: too few lines per image would mean that the picture would be too rough and therefore not recognisable, and too few pictures per second would mean that the image would appear discontinuous and jerky. In principle, using a light spot on an oscilloscope screen solved the problem of the mechanical slowness.

With the use of this ‘flying spot’ one was able to broadcast films.⁵¹ The ‘intermediate film’ method, whereby a ‘live’ broadcast could be made, involved first filming the event and then immediately processing the film and passing the – still wet – film through the television ‘flying spot’ camera. Even then, though, it was clear that this was awkward and not suitable as a final solution for outside ‘live’ broadcasting and for the commercial use of television.

The invention of the iconoscope created by RCA’s Vladimir Zworykin in 1933 in the USA represented an important step. This invention made the whole process of transforming an optical picture into an electrical signal of an electronic nature. An image was projected onto a photo-emissive target screen in which the pattern of light and dark areas would be transformed into areas of large and small positive electrostatic charges. Then this pattern would be scanned by an electron beam and the changing extent to which the beam would have to compensate for locally missing electrons would determine the level of the current, and so an alternating current would be the output. The advantage of this system was that the effect of the light was saved up during the whole period between two successive times when the target was hit by the scanning electron beam. This made the iconoscope tube much more sensitive than the Nipkov system. In the USA, the implementation of electronic TV broadcasts was at first hampered by fights over patents, but after 1933 it really took off.⁵²



Iconoscope. L a lens projecting the scene on the mosaic M of the target T. SP signal plate, R_s load resistor, C collector, K cathode, D deflection coils, E scanning beam. The (electrostatic) focusing is not shown.

Figure 19. The iconoscope television pickup tube (from *Philips Technical Review* Vol. 13, p. 121).

This tube is used in a television camera to convert a light image into an electrical television signal. Light is focused by a lens L and hits target T on which mosaic M of photo-sensitive material has been deposited. The more light hits the target, the more electrons will be emitted from the material, and the higher the positive charge is that will be created (because of the negative electrons leaving the neutral target). Thus, the light pattern is converted into a pattern of small and large charges on the target. This pattern is read by a scanning beam E of electrons that is emitted from cathode K and drawn to the target by collector C. Deflection coils focus the scanning beam. The higher the positive charge created on a spot of the target, the higher the number of electrons will be in the scanning beam that is used to replace the electrons emitted from the target, and the fewer the electrons left to be transferred to signal plate SP and read as a television signal.

At first, Holst did not seem to recognise the potential of this tube.⁵³ There were reasons for that: the speed at which the scanning beam hit the target was so high that the process of filling the electron gaps with the beam electrons was very uncontrolled. Electrons often fell back into wrong positions on the target. The alternative was to have a tube in which the scanning beam was slowed down by means of an opposite voltage, so that it would just touch the target, and the remainder of the beam would go back in the direction of the electron gun (the 'return beam' as it was called). This was done in the orthicon, which was developed by RCA and presented in 1937. The disadvantage of this system was that the slow scanning beam was less well focused. As a consequence, the orthicon was less suitable for television standards where there were a great number of lines per image. In the USA and the UK fewer lines per image were used, so the orthicon became the 'standard' type of tube in those countries, while in Europe the icono-

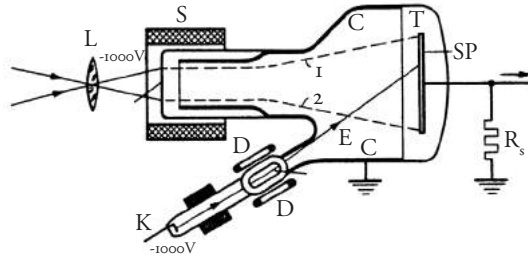


Image iconoscope. P photocathode, S coil of the magnetic electron lens. 1 and 2 paths of photoelectrons, Foc focusing coil. Other letters have the same meaning as in fig. 4.

Figure 20. The image iconoscope (from *Philips Technical Review* Vol. 13, p. 124).

Here the process of converting the light image into an electrical image is split up into two parts: first the light hits photo cathode P, from which electrons are then emitted that move to target T to create the pattern of charges, as in the iconoscope.

scope became the 'standard'.⁵⁴ Both the orthicon and the iconoscope were improved by inserting a photocathode in order to split up the conversion of the optical image into an electrical signal into two steps. The improved types were called image iconoscopes (patented in 1934) and image orthicons (patented in 1946). A second improvement of these tubes over what had gone before lay in the way in which the light from the image and the scanning beam could come from opposite directions by making the target screen transparent. This made the tubes a lot more compact.

Meanwhile, the importance of colour television increased. For a colour television three tubes per camera are needed. Because the amount of light had to be split up into three parts, one for each of the tubes, light intensity became an even more important criterion than it had been with black-and-white television. Generally, it was recognised that only the image orthicon was good enough for colour television cameras, but this was a quite complex and voluminous tube.

In 1950 RCA brought out a different tube that was not based on photo-emission, but rather on photoconductivity. In that tube the pattern of negative charges on the target was produced not by letting electrons be ejected from the target by the light that fell on the target, but by letting the electrons move through the screen that had become locally conductive in the places where light fell on it. This 'vidicon' had two main problems: when no light fell on the target, there was still a small current (that was called a 'dark current') that caused a loss of contrast between light and dark. Also, the photoconductive effect was too slow to follow quick

changes in light intensity. The RCA scientists were convinced that these problems were intrinsic to the effect of light conductivity, and so they decided not to look further for other materials.⁵⁵ In the Nat.Lab. the research group expected to be able to find materials that would not necessarily have these problems. The idea was that there might be materials that could be put into the target so that p and n regions would be produced, which formed a diode and would stop the dark current. This idea would ultimately lead to the invention of the Plumbicon.

The Research that Led to the Invention of the Plumbicon

In the period described above, the Nat.Lab. had done some work on all the types of tubes discussed so far. Until WWII this continued in spite of the fact that – probably mainly because of Holst's doubts about the feasibility of television⁵⁶ – there was not much confidence in the possibility of making television a commercially viable product. The technical feasibility of television was already known at that time.⁵⁷ In 1938, during a 'Television meeting' with representatives from the company and the lab management staff, it was decided that the company should become more active in television research.⁵⁸ In the early post-war years iconoscopes and image iconoscopes had been produced in the lab.⁵⁹ A production rate of one image iconoscope and two normal iconoscopes per day was thought to be feasible. The image orthicon was not studied very intensively: it was considered to be too expensive and too complex; high light sensitivity was considered to be only of interest to military applications.⁶⁰ The orthicon was only considered to be sufficient for 500 lines per image, while the image iconoscope could deal adequately with 750 lines per image. There were also ideas to develop a '?-scope'⁶¹ that would have a lower redistribution of electrons in the scanning process. A copy of a French tube, the eriscope, was purchased for inspection.

In 1950 Bruining, the group leader, wrote a memorandum on pickup tubes, in which he indicated the strategic decisions that he thought were necessary and the options he listed were these:⁶²

- the image iconoscope: suitable for studio use, but not for outdoor use,
- the CPS emitron (a British tube similar to the orthicon): the same,
- the image orthicon: less suitable for studio use, but good for outdoor,
- the vidicon: insufficient for studio, good for outdoor use and for industrial applications.

He recommended working on improving the image iconoscope in the short-term and meanwhile developing a new version of the vidicon as a long-term solution. This vidicon should be available in two versions: one with a fast scanning beam and the other with a slow scanning beam. This would imply that the vidicon was seen as a possible solution to the line system dilemma that had split up the market into a USA/UK part and a

European continent part. Another reason why Bruining could argue that people would be well advised to work on the vidicon was because of the knowledge of photoconductive layers that was available in the Nat.Lab. (as a result of 'fundamental' research). The material that was seen as most suitable for the new vidicon was cadmium sulphide. At that time lead oxide was already being used, but it is not clear if this was for vidicons or other tube types. From the minutes of a Contact meeting on pickup tubes, dated November 27, 1950, we get the impression that lead oxide was used for orthicons. The minutes of a meeting held on January 3, 1951, reveal that cadmium sulphide was still being used for vidicons. In an interview with E.F. de Haan, 1953 was mentioned as the first year in which lead oxide was used for the new vidicon tube.⁶³ It is conceivable that lead oxide became an option because it was already being used in other tubes as a blocking layer on top of the photo-emissive selenium layer.⁶⁴ According to De Haan,⁶⁵ who had succeeded Bruining as the group leader of the pickup tube research group, the choice of lead oxide was instigated by the fact that the isolating properties of lead oxide were known (important for reducing the dark current) as well as for the fact that its yellow colour – which in the colour spectrum is just between red and blue – would be advantageous for the colour sensitivity of the tube.

Thus, several research avenues were pursued in the lab for a number of years. Work on the image iconoscope and on the new vidicon continued. Meanwhile, purchasing orthicons was continued as an option to 'fall back on'. The work on the '?-scope' also continued, and in 1955 the '?' was replaced by 'scenio'. This tube, the 'scenioscope', which was invented by Schagen, was of the image iconoscope type, but with a glass target, which improved the quality of the image for lower light intensities when compared with the image iconoscope.⁶⁶ Just like the image iconoscope and the original RCA version of the vidicon (with antimony sulphide as the photoconductive material), the scenioscope initially went into production,⁶⁷ but after a short while the name of the tube no longer appears in the Electron tubes PD documents.

Work on the vidicon involved several different disciplines: on the one hand, knowledge of solid-state physics was required, while on the other hand the knowledge and skills to produce good layers in the glass tubes was important. In the latter area it is particularly the name of P.P.M. Schampers that must be mentioned. For several years he experimented on finding ways to prepare the lead oxide layers in a reproducible way with good properties for the Plumbicon. This involved very clean vacuum conditions during the deposition of the layers and during the melting together and sealing of the glass parts to form the Plumbicon tube. Another problem was the steering of the electron beams with the focusing coils.⁶⁸ Finally, the researchers had to take into account the conditions laid down by the IG ELA to make the tube fit into their cameras.⁶⁹

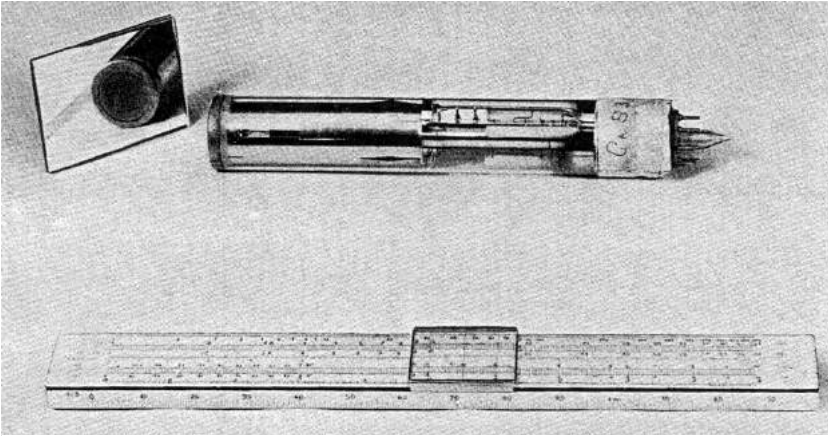
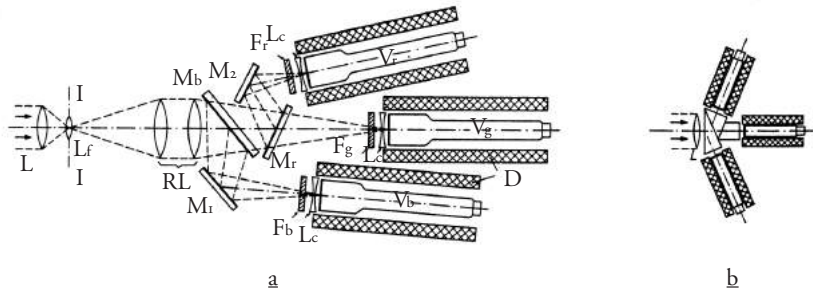


Figure 21. The Plumbicon (from *Philips Technical Review* Vol. 25, p. 134).

In 1958 pilot production started at the Nat.Lab. (in Waalre) under Schampers's leadership; simultaneously, a group in Geldrop (led by Van den Boomgaard) started producing lead oxide vidicons. One problem was the lifetime of the tubes. This varied greatly from tube to tube, but often did not exceed 50 hours, even when the working voltage was kept as low as 15 volts.⁷⁰ Partly influenced by some visiting American television experts,⁷¹ the PD ELA became convinced of the importance of this new tube, which by then had received the name 'Plumbicon',⁷² a name put forward by the Nat.Lab. The PD Medical Equipment had also become interested in the vidicon for X-ray imaging purposes.⁷³ As it was taking so long for the tube to be considered ripe for production, the Board of Management started wondering if perhaps it would not have been better to continue working on the image orthicon, and at a certain point they even urged Casimir to stop working on the Plumbicon.⁷⁴

The Transfer to the Elcoma PD for Production

In 1960 the PD Elcoma started the pilot production of the Plumbicon, and Schampers did a lot of work to improve the stability and quality of the lead oxide layers. He wrote very exact instructions for the production process in the hope that this would improve the reproducibility of the process.⁷⁵ As it turned out, the PD did not want to copy the Nat.Lab. process, and so it started its own experiments and, in so doing, blamed the Nat.Lab. for having already worked on the Plumbicon for eight years without achieving any evident success.⁷⁶ The Nat.Lab. experiments con-



Practical arrangement of the colour separation systems in:

a) image-orthicon cameras,

b) the 'Plumbicon' colour camera, the components being illustrated here *on the same* scale as those in (a).

In (a), L is the camera lens. To allow lenses with a large relative aperture and normal focal lengths to be used, an optical relay system. RL is included. A focal length here of about 150 mm gives sufficient clearance for the colour separation system. Lf field lens for the concentration of the light beam in the optical relay system. The curvature of the image produced by Lf is compensated by the three correction lenses. Lc, Fr, Fg en Fb are colour-correction filters to limit more sharply the wavelength ranges separated by the colour-selective mirrors. D deflection and focusing coil systems. The other letters are as in fig. 2.

Figure 22. Arrangement of three pick-up tubes in one colour television camera (from *Philips Technical Review* Vol. 28, p. 338).

Mirrors M are colour-selective mirrors for splitting up the light into blue, red and green components; F are colour-correction filters, and L are lenses.

tinued because production at the PD was not a success either. Often the nature of these experiments was trial-and-error. Broerse told the story of the Mercks company having delivered bags of lead acetate. This material was then sampled and used for making a number of Plumbicons: if the results turned out to be good enough, the bags would be kept, and if not they would be returned to Mercks.⁷⁷ It never became clear why some bags were all right, and others were not. Between the Nat.Lab. and the PD there was sometimes even an atmosphere of competition. Again it was Broerse who had a story to tell about that: the director of the PD had at a certain moment asked for lead oxide with the code name 'SJ', because Schampers had found that to work well. Broerse refused to supply it saying: 'you are not able to make good Plumbicons even with this good lead oxide'. His superior, De Haan, later admitted to appreciating this as a joke, but he told Broerse to nevertheless give them the lead oxide.

In 1961 Schampers had success in solving the problems of the speckles that appeared in the Plumbicon images. Very careful experimenting had shown that the speckles, which were caused by small areas in the lead oxide layers with a different electrical resistance, could not be avoided in the process-

ing. The invention that solved this problem was bombardment in an oxygen discharge after the layer had been processed. In this way one made use of the electrical resistance differences in the layers, and the spots that caused the speckles were annihilated preferentially. It appeared that not only the speckle problem was solved but also the lifetime of the tubes increased substantially. By then several Plumbicons were being produced a day. A strict process recipe was laid down, and no more experimenting was allowed.⁷⁸ The variety of tested procedures was later used to write a patent application in which the 'real' production process was buried in 90 pages between a long list of other, unsuccessful processes.⁷⁹ Thus, the real process was published and protected at the same time. By 1962 the production process was well enough under control to stop the production of image orthicons (up until then the only alternative for use in colour television).

At that time it seemed possible to deliver enough Plumbicons per year (ELA ordered about 1200 per year in 1963;⁸⁰ it is not clear how the estimated number of 300 per year⁸¹ for 1962 could be regarded as sufficient). The PD ELA strongly opposed the sales of Plumbicons to third parties, because they wanted to be the only suppliers of cameras with Plumbicons. The Board of Management apparently shared this view.⁸² In 1963 the PD Electron Tubes asked for a budget of NLG 333,000 to set up the production of Plumbicons. This was granted, and an additional sum of NLG 300,000 was asked for because of the expected increased demand (8,000 Plumbicons per year).⁸³ In 1965 another NLG 6,250,000 was invested in a new factory hall.⁸⁴ The production estimates had grown to 3,000 Plumbicons by 1966/7, 7,600 by 1967/8, 10,000 by 1968/9 and 10,000 by 1969/70. The expected profit margin was 20% of the turnover. At that time the market was still divided in its response. The British BBC decided to replace their vidicons with Plumbicons, but the German Südwest Rundfunk company still hesitated. The Fernseh GmbH found that the Plumbicon was not good enough for studio productions.⁸⁵ The production (in a building called RAF4) had problems, and so the Nat.Lab. was called in to assist. In this connection the already ongoing research into extended red sensitivity Plumbicons was temporarily postponed.⁸⁶ Research into the new type of Plumbicon was resumed in 1965.⁸⁷ By then production had been brought under control, which was just in time because the expected demand for 1968 had increased to 12,900.⁸⁸ In 1966 the embargo against delivery to third parties was lifted. Afterwards, the embargo was regretted because the loss of income it led to was estimated to be about NLG 280,000.⁸⁹ Meanwhile, the lifetime of the tubes had improved to such a degree that demand had decreased, and production had to be reduced.⁹⁰

Later Research

The improvement of red sensitivity was one of the research areas in which the Nat.Lab. supported further Plumbicon development. By then Broerse had become the group leader. Later Broerse moved to the PD Light (as he said⁹¹ not because he knew so much about light, but because the Plumbicon was ‘just glass and vacuum’, and so are lamps). As in previous years, the group had about 7 scientists.⁹² Except for improving the red sensitivity, the electrostatic focussing of the scanning beam became a topic for research, but this was never transferred to the PD.

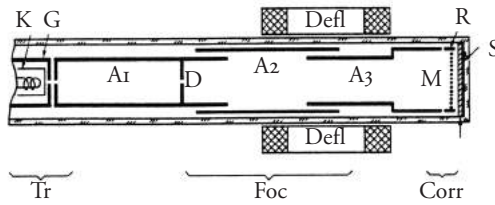


Diagram of the construction of a ‘Plumbicon’ tube with electrostatic focusing. *Tr* triode gun, consisting of the cathode *K*, the control electrode *G* and the anode *A₁*. The focusing lens *Foc* is formed by the electrodes *A₂*, *A₂* and *A₃*, *D* diaphragm. The ring *R* and the mesh screen *M* together with *A₃* form the correction lens *Corr*. *T* photoconducting layer, *S* signal plate. Defl deflection coils.

Figure 23. A Plumbicon with electrostatic focusing (from *Philips Technical Review* Vol. 29, p. 334). Anodes *A₁*-*A₃* take care of the electrostatic focusing. Defl are deflecting coils.

In 1968 the Nat.Lab. brought out a mini-Plumbicon for portable cameras.⁹³ This was a commercial failure, because these kinds of Plumbicons were qualitatively inferior to the normal Plumbicons. For several years research into the control of the electrical field using vacuum deposition of oxide layers was kept going to make production cheaper and to increase the reproducibility of the tubes.⁹⁴ Then there was a period when the Nat.Lab. only advised the factory now and then (1979-1985).⁹⁵ Although the idea of establishing a new Plumbicon tube generation was mentioned in 1978, there was no evidence to suggest that any such serious activity was continuing in the Nat.Lab.⁹⁶ The last Plumbicon activity mentioned in the Corporate Research Programmes is that of the HDTV Plumbicon (in 1985 and 1986). From 1987 on, no more Plumbicon activities are mentioned in the Corporate Research Programmes.

This case study contains several elements that characterise the 1946-1972 period. We saw that there was an emphasis on developing knowledge

about phenomena (the solid-state physics of the phenomenon of photo-conductivity), a push from the Nat.Lab. towards the PDs to accept a new product idea, and a selective attitude on the part of the PDs: the original Plumbicon was transferred, but the Plumbicon with electrostatic focusing was not. The case study also shows that the Nat.Lab.'s idea to develop a new product that 'only' had to be put into production by the PD underestimated the production problems: upscaling production appeared to be one of the most difficult development phases, and the Plumbicon even had to be taken back to the Nat.Lab. for this.⁹⁷ The fact that because of lack of expertise the PD had to give the design back to the Nat.Lab. just proves how low PD involvement had been until then. This contrasts sharply with, for instance, the continuous process of co-operation seen between the Nat.Lab. and the radio development lab as illustrated in Part I.

Finally, we have seen how the Nat.Lab. sometimes followed external developments for a long time without too much effort (e.g. while other companies developed the iconoscope and the orthicon), only to catch up in a very short time as soon as they saw a chance to improve on an existing imperfect, but promising product (in this case: the RCA vidicon). Thus, the Nat.Lab. played a role in positioning Philips as a serious partner in the international television market, which until WWII had been dominated by USA companies (in particular RCA).

6.3 *LOCOS: Local Oxidation of Silicon*

Introduction

As a case study for the 1946-1972 period, LOCOS provides us with another good example of how research aimed at understanding underlying phenomena in the Nat.Lab. could yield very successful outcomes. In the 1980s just about every company that produced ICs was forced to apply this principle. Unfortunately, it has not been possible to establish exactly how much Philips earned from this innovation. The licence incomes on their own must have been considerable. Besides that, there have probably been numerous interesting cross-licences. The story goes that on the occasion of his retirement as managing director of the Patent Department, Pierre Simons said that he had two 'crown' patents: LOCOS and the Plumbicon.⁹⁸ In that respect, there is a strong similarity between LOCOS and the Plumbicon as television pickup tubes that were used in almost every professional camera and which provided Philips with a strong position in the field of television.

As with the previous case studies, we see here how important the role of an individual researcher can be. We cannot imagine LOCOS without

the name Else Kooi. Of course research is always a matter of teamwork, but here again we clearly find one ‘inventor’ who took the lead.

The Prologue: Transistor Technology

Transistors are produced by bringing in (‘doping’) elements (‘impurities’) with an abundance of electrons or with a lack of electrons (‘holes’) into semi-conducting materials (mostly germanium or silicon). This results either in ‘p-type’ material that has an abundance of positive charge carriers, or ‘n-type’ material that has an abundance of negative charge carriers. The transitions between the two types are called ‘junctions’. Regions of n- and p-type material can be separated by insulating layers of silicon oxide. The structure is covered by a protecting layer (the ‘passivation layer’). Transistors in which mobility of both the positive and the negative charge carriers is used are called ‘bipolar transistors’. Integrated Circuits (ICs) are slices of semiconducting material into which large numbers of electronic components (such as transistors) have been built. It was the need to fit large numbers of transistors into a small area that became the main motive for the development of ICs, and therefore it is logical to start the description of our case study with the story of the introduction of the transistor to the Nat.Lab.⁹⁹ This device was a Bell Labs invention with Shockley, Bardeen and Brattain as its spiritual fathers. The history of its invention has already been described many times because it represented one of the most important breakthroughs in the development of electronics.¹⁰⁰ This history is a fascinating one. Originally, the Bell Labs team had tried to make a sort of solid-state analogue of the triode tube. This effort had resulted in an amplifying device, but it could not be controlled because its functioning was not well understood and seemed not to be the same as in the triode. It was not until exploiting solid-state physics more explicitly that the functioning was understood, and the transistor could be mass produced. In 1952 Bell organised a symposium for companies that were interested in transistor technology. The entry fee for the symposium was extremely high. In fact, it was the equivalent of several years of licence payment. Due to a previous agreement with Western Electric, Bell Labs’ mother company, Philips paid the normal US\$25,000 that had to be paid as an advance for future royalties for licensing the transistor technology, but was exempted from paying royalties for transistors that were sold as part of a certain set of applications, such as telephone switching devices.¹⁰¹ The expectation that the transistor was a real breakthrough was, in itself, enough to attract several participants. On returning from a trip to the USA in 1948, Verwey had already told the tube factory people that within a few years they would close their factory because of the coming of the transistor.¹⁰² Such a statement reveals how important the transistor was considered to be. At that time the material used for transistors was ger-

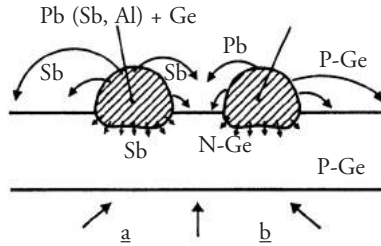


Diagram showing how a transistor structure (P-N-P) is produced beneath a double-doped pellet of lead on a germanium wafer.

a) The lead pellet is melted and lies in a pit of dissolved germanium (temperature 780°C). Antimony (the donor) diffuses into the germanium, partly directly and partly by way of the vapour phase, while aluminium (the acceptor) does not. b) On cooling, aluminium-containing germanium is deposited in the pit (P-type). This is the antimony-doped layer of germanium (N-type), which can act as the base.

Figure 24. The pushed-out-base transistor (from *Philips Technical Review* Vol. 24, p. 236).

Two germanium lead pellets are dropped on a P-type wafer. One contains the acceptor element antimony; the other contains both antimony and the acceptor element aluminium. When heated, the pellets take up germanium from the wafer and release antimony into the wafer. The aluminium does not diffuse into the wafer. The diffused antimony, and also antimony that reaches the wafer via the vapour phase, turns the solid P-type germanium into N-type germanium, which encloses the pellets and becomes the basis of the transistor

manium. Soon silicon was also to be recognised as an option. This semi-conducting material was originally used for diodes in radar systems. The mass production of silicon had been developed in 1941 by Du Pont.¹⁰³ Silicon made it possible to work with higher frequencies, which was very attractive also for radio and television. It was just more difficult to process than germanium. Van Vessel, who was working at the PD Elcoma at the time, told of how he went to the Bell Labs symposium and got the impression that the Nat.Lab. was not yet very interested in working on the point contact transistor that had been presented there.¹⁰⁴ Through his contacts with the RCA company, Van Vessel found out that the layer transistor was a more promising prospect for mass production, and in 1954 the Nijmegen factory started producing layer transistors. Clearly, the PD went its own way and did not wait for the Nat.Lab. to assist them. Officially, the factory fell under the radio tubes group in Eindhoven. There was frequent contact with the Nat.Lab., in particular with Dr. Haaijman and his group, but there was not really an attuning of activities because the lab defined its own programme, and there was not much PD influence (which, as we saw in Chapter 5, was typical for the 1946-1972 period). Van Vessel used to say: If I can understand it, it can no longer be called research. This shows that he was sensitive to a boundary between research

and production.¹⁰⁵ At that time Pannenberg initiated a special contact group in order to enhance the contacts, which consisted of both Nat.Lab. and PD Electron Tubes representatives. The Nat.Lab. saw it as a challenge to utilise the attractive properties of silicon in germanium. In 1957 this resulted in the invention of the 'pushed-out-base' transistor. This invention increased the cut-off frequency from just a few megahertz to about 200 megahertz, thus opening the way to dealing with television signals.

The success of the pushed-out-base (p.o.b.) transistor caused Siemens in Germany to adopt this type of transistor. In the applied research-oriented Siemens & Halske central laboratory in Munich, this kept the Siemens researchers focused on germanium for many years, while their colleagues in the more basic research-oriented Siemens Schuckertwerke laboratory in Erlangen worked on silicon.¹⁰⁶ At Philips, the p.o.b. transistor defeated the scepticism about the Nat.Lab.'s abilities in the field of semiconductors,¹⁰⁷ but at the same time caused the Nat.Lab. people to become even less interested in silicon. Transistor research was done mainly by chemists. Perhaps physicists would have had a less outspoken preference for germanium and would have more easily taken up the option of silicon.¹⁰⁸ Some silicon research was done, as we can see from the 1952 research programme, but this was mainly into diodes, and this research met with several difficulties. Even when silicon had clearly superseded germanium in the USA transistor industry (already in the early 1950s Texas Instruments, by then a leading transistor-producing company in the USA, had opted for silicon), the Nat.Lab. still remained fully dedicated to germanium. In 1956 silicon was considered in a CRC meeting as a possible transistor material for the first time.¹⁰⁹ The debate indicates that an awareness of the potential relevance of silicon had developed in the Nat.Lab. In 1958 Tummers, who had succeeded Haaijman as the group leader, asked a new researcher, E. Kooi, to try to realise a p.o.b. transistor in silicon.¹¹⁰ According to Van Vessem it had been quite difficult to convince the Nat.Lab. of the need to appoint a surface physicist. After two years Kooi had achieved this goal. This silicon transistor was transferred to the factory in Southampton for pilot production.¹¹¹ At the 1958 CRC, Haaijman reported that now most effort was being put into silicon, but that there were still all sorts of problems to be solved. Meanwhile, in the USA companies like Texas Instruments and Fairchild had already taken the next step: it appeared to be possible to make integrated circuits in silicon, thus bringing large numbers of components on one crystal surface.¹¹² The integrated circuit had been invented by Jack Kilby at Texas Instruments in 1958/1959. The 'planar technology' was applied for that purpose. Kooi was instructed by Tummers to do research into surfaces of those types of silicon crystals, using a pragmatic approach. That approach was to make the work of Tummers's group quite different from the work of more 'fundamental' groups, like those of

Sparnaay and Polder, with which there was some contact but from which few results emanated. Dr. F. Meijer, for example, in the Sparnaay group was involved in carrying out certain surface measurements that would have been relevant to the invention of LOCOS. But although his work was related to the surface of semiconductors, no one had informed him about the existence of the work on LOCOS.¹¹³ According to Van Vessem, the Nat.Lab. was biased in its contacts with the USA: for instance, the researchers visited Bell Labs, but not the companies that produced many ICs, like Motorola and Fairchild. In 1962 Haaijman admitted at the CRC that although a lot of work was being done in the Nat.Lab. on 'solid-state circuits', there was a considerable backlog compared to Fairchild.¹¹⁴ The Board of Management was also aware of this backlog. In a letter written by Tromp and Hazeu to the Board of Management we find that, in their opinion, after the magnetic materials nothing spectacular had emerged out of the Nat.Lab.¹¹⁵ Meanwhile, it had become evident to the Nat.Lab. that ICs were becoming important for use in computers. From 1956 on there was a Transistor Computer Contact Group at the Nat.Lab. The PD Computers' director, Jorna, had already asked for solid-state crystal circuits. Jorna did not want the Nat.Lab. to use the thin-film method, which meant that thin layers of semiconducting material were deposited on a substrate¹¹⁶. This method was used by the PD Icoma in Eindhoven, because IBM had chosen this method, and the Philips company directorate had drawn the conclusion that there would be no market for solid-state circuits in the near future.¹¹⁷ Meanwhile, the PD Electron Tubes in Nijmegen worked on integrated circuits with solid-state crystals.¹¹⁸ With the crystals fewer process steps were needed, which was clearly an advantage. The Mullard lab acquired this technique when the GEC Wembley lab was taken over in 1961. It was not until 1966 that the thin-film technique was abandoned.¹¹⁹ In 1964 Klasens remarked at the CRC that no more 'fundamental' research on germanium and silicon was needed and that all efforts should be directed towards applications in devices. Klasens also pointed out that better contact between the chemists and the physicists at the lab and the device specialists was needed, and he mentioned that Bell might be taken as a model. A memorandum written by A.A. Opstelten (from the PD Elcoma, which was the outcome of merging the Icoma and the Electron Tubes PDs) also reveals that the co-operation with the development labs in the PD needed to be improved.¹²⁰ This contact was seen as being particularly crucial for the professional systems (more than for consumer applications). In the Raad van Bouwelementen (Council for Components) the need for good contact between research and development was emphasised, because unlike tubes, ICs were application-determined, which meant that the applicants should have a say in the development as early on as possible.¹²¹ Meanwhile, Philips searched for a company that could be bought up because it would be able to introduce the IC

production techniques. The two companies seen as possibilities, were Signetics and Westinghouse.¹²² In the end Westinghouse was selected, and a contract was drawn up with this company.¹²³

The IC Research Work in the Nat.Lab. that Led to the Invention of LOCOS

While developing planar technology, researchers in the USA¹²⁴ had found out that a covering oxidised layer of silicon (SiO_2) led to stabilisation of the underlying silicon. Passivation, the application of a thin protecting layer on a surface, became a new research topic for Kooi in 1966. In 1967 he would get his Ph.D. for this work in which Tummers had been his supervisor. At that time the oxidising silicon layer was accompanied by some instability, which meant that it could not yet be put into practice. In 1966 Kooi discovered an article by Tombs et al. in which it was suggested that one should not work with silicon oxide, but rather with silicon nitride (Si_3N_4). The article did not, however, indicate how such a layer of silicon nitride should be deposited on the silicon base. At first, Kooi tried out Chemical Vapour Deposition, which was a well-known technique in the lab at that time. The result was as Kooi had feared: the silicon-silicon nitride transition prevented the desired outcome. To solve this problem Kooi tried to insert a silicon oxide layer between the silicon and the silicon nitride. When experimenting with this he did not take a new substrate, but instead a substrate that already had a silicon nitride layer on it. He tried to oxidise the silicon under this layer of silicon nitride, but it appeared that the silicon nitride layer completely prevented the silicon layer from being oxidised. Kooi knew this to be true because on the other side, where there was no silicon nitride-covering layer, the silicon did oxidise. This showed that a layer of silicon nitride could be used as a covering mask when oxidising a silicon layer.¹²⁵ There was also an accompanying phenomenon that would make this even more interesting: one of the properties of silicon was that when oxidised it sank halfway into the surrounding silicon. The result was therefore not to have an extra layer on top of the silicon, but to have rather only half the thickness of the silicon oxide layer added to the thickness of the substrate. That yielded a smoother surface than a deposition process could yield. With integrated circuits the golden rule was always: the smoother the surface, the better. Kooi immediately wanted to have the idea patented, but the patent department first wanted one more problem to be solved, namely the process of etching away the nitride mask after the oxidation process. The nitride appeared to be difficult to etch away. From a 1961 article by Kallander et al., Kooi knew that lead oxide (PbO) served as a catalyst so that silicon oxide could be made at lower temperatures. The result was a sort of lead silicate glass that could easily be etched away.¹²⁶ In March 1966 Kooi reported that this

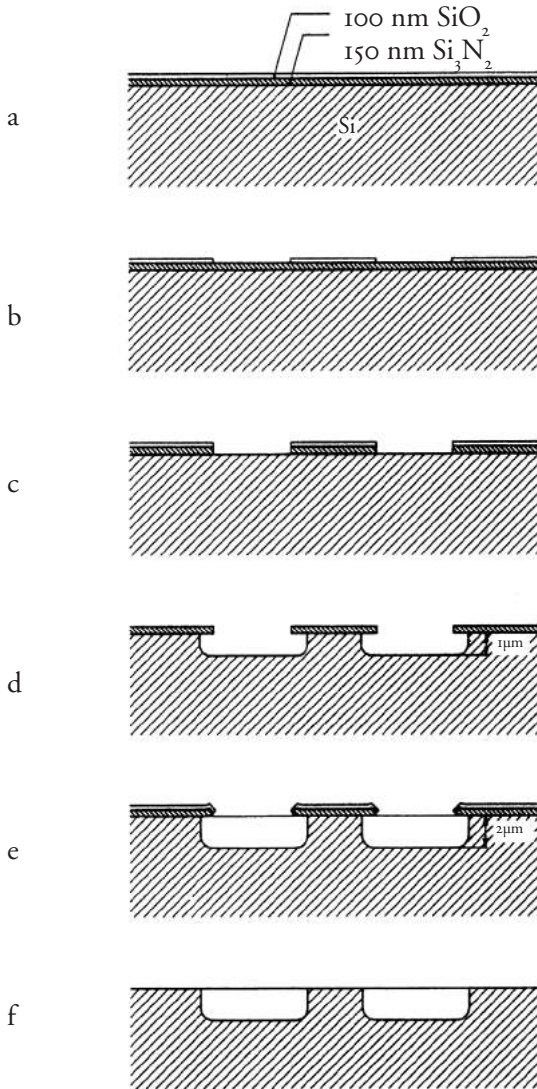


Figure 25. The LOCOS technique (from *Philips Technical Review* Vol. 31, p. 235).

A layer of silicon nitride and a layer of silicon dioxide are deposited on a layer of silicon (a). A pattern of holes is etched in the silicon dioxide using the conventional photo-etching technique (b). The pattern is etched into the silicon nitride with hot phosphoric acid with the silicon dioxide serving as a mask (c). The silicon is etched away at the holes in the pattern to a depth of 1 micrometer (d). The silicon in the holes is oxidised to a depth of about 2 micrometers. The holes are completely filled with the silicon dioxide thus formed. The silicon nitride is only superficially oxidised (e). All silicon nitride is etched away with hot phosphoric acid (f). Thus, a very flat surface is created.

problem had also been solved, and the patent application was submitted.¹²⁷

All this happened in connection with MOS (Metal Oxide Semiconductor) ICs. According to Van Vessem it was quite some time before the Nat.Lab. was prepared not only to work on bipolar ICs for analogue purposes, but also to work on MOS, which was considered to be more suitable for digital techniques.¹²⁸ Bipolar transistors are formed by sandwiching a thin layer of p- or n-type material between two regions of the opposite type of semiconductor. MOS transistors (metal-oxide-silicon) consist of two islands of n- or p-type semiconducting material in a substrate of the opposite type, and covered with a metal layer. At that time the MOS technique was mainly used by small companies such as Intel, and this reminded Van Vessem of the early period of IC technology when small companies such as Texas Instruments, Fairchild and Motorola took the lead by using a method that was different from the method that the bigger companies used, and in that case the small companies had been more successful.¹²⁹ Again this illustrates how minor the PD's influence on the research programme really was. It is also another example of how difficult it was for the Nat.Lab. to free itself from one particular technological trajectory (the other example being the slowness attached to starting the work on silicon because of people's general preference for germanium). Indeed, in the Minutes of the 10th CRC, we find a remark by De Haan to the effect that the number of possible applications for MOS transistors was considered to be small. Kooi also admitted¹³⁰ that the Nat.Lab. very much preferred bipolar ICs, because MOS was expected to be unstable. Nevertheless, it is also a fact that LOCOS developments started with MOS research. The real MOS breakthrough at the Nat.Lab. came as soon as it became clear that there was a quest for ICs for digital computer circuits. In addition, the expectation rose that in the end consumer applications would become digital as well. There are two MOS variants: the p-channel MOS and the n-channel MOS. The difference is in the type of doping: materials that yield positive charge carriers when inserted into the substrate make p-channel MOS (P-MOS), while materials that yield negative charge carriers result in n-channel MOS (N-MOS). At the beginning of the MOS IC era, it was mainly p-channel transistors that were used with aluminium as the metal. With N-MOS an area for separating the channels is needed, unlike with P-MOS. The application of LOCOS then made the oxide layer sink into the substrate to a certain extent, and this automatically functioned as such a separating layer, which then saved one process step for N-MOS. So when N-MOS became more popular in IC technology, the advantages of LOCOS became even more evident than they had been before. The Nijmegen IC factory did not, however, show much interest in adopting LOCOS until they had been convinced that

this process could also be used to combine N-MOS and P-MOS into one IC: C-MOS. This brings our story to the phase of production.

In 1966 certain changes took place in the Nat.Lab.'s directorate. Tummers was appointed director under De Haan and Kooi, together with P. Hart, who was made group leader of his group. The LOCOS research was continued by E. Kooi and his colleague M. Collet, and their main concern was to control the growth processes of the silicon nitride layer on the silicon substrate. In their April-June 1967 Progress Report they wrote that they could not yet establish a clear relationship between growth conditions and surface properties, and in the June-September 1967 Progress Report they wrote that efforts to make the process work under ultraviolet light had failed. The struggle to get rid of uncontrolled peculiarities in the LOCOS structures was to dominate the research in the years to follow. In the 1969 Progress Reports produced by the Kooi/Hart group, one reads about the little hills created on the ridges of the LOCOS structure and the cracks in the corners of the nitride. The term 'bird's beak' for these sorts of artefacts originated from J. Appels, one of Kooi's assistants.¹³¹ What caused the bird's beak was the expansion of the intermediate silicon oxide layer that lifted the nitride layer a bit. Another undesired effect was nicknamed the 'bird's head', and that emerged when the oxide showed a ridge on the border between the nitride and the silicon oxide – silicon nitride double layer masks.

The Production Phase

When LOCOS was invented, the market expectations for ICs were quite high. In the USA the market doubled every six months in terms of numbers of ICs and every year money-wise. More than half of the applications at that time in the USA were still military, but there were clear indications that consumer applications would soon become more important.¹³² For Philips the IC market was problematic from the very beginning. In 1967 Van Vessem reported big losses in Nijmegen because the prices of ICs had fallen.¹³³ Partner Westinghouse withdrew from the IC market,¹³⁴ and with that Philips lost its relationship with the American market. The search for a new partner started.¹³⁵ Soon the name Signetics popped up, but in 1969 a contract was first made with Fairchild, even though the Nat.Lab. had just advised against such a union¹³⁶ (they expected that Fairchild would gain more from them than they would gain from Fairchild and also that Elcoma would profit more from Fairchild than from the Nat.Lab.). Soon, however, the relationship between Philips and Fairchild cooled off after Philips had given an important order to Motorola instead of Fairchild. Fairchild and Motorola were not on good terms because in 1968 some Motorola people (among them director Lester Hogan) had moved to Fairchild.¹³⁷ The search for a new partner gave rise to friction between Elcoma

and the PD RGT. Elcoma preferred a true IC manufacturer, and RGT preferred to have a television specialist for a new partner.¹³⁸ Motorola was considered to be a good compromise because it combined the best of both worlds, but its television group Quasar was sold to Matsushita just a little bit too early. Because no other compromise could be found, Magnavox was bought in 1974, followed by Signetics in 1975: the first-mentioned company was bought because of RGT's preference for it, and the second was seen as a bipolar-oriented partner for Elcoma. The desire to buy a true MOS firm as well could not be fulfilled because of the protectionist policy of the USA government (it had thrown up barriers for foreign companies to operate on the US market). Adding a new research lab to Signetics in Sunnyvale, which – under the guidance of Kooi, until his retirement in 1992 – would be targeted at MOS in particular, solved this problem. It allowed Philips to establish contacts with American companies concerning the MOS technology by using its existing IC base in the USA, Signetics, so that the problems with protectionism were avoided. In the meantime, it seemed that the IC market had passed its worst phase.¹³⁹

The actual transfer of LOCOS to the Nijmegen factory took place around 1970.¹⁴⁰ Until then, the researchers in the Nat.Lab. struggled with various undesired phenomena. It has already been mentioned that the main reason for transfer was the fact that LOCOS could be combined with C-MOS to become LOC MOS. One of the first LOC MOS products was a 256-bit static RAM constructed in a seven-step process with lithographic masks.¹⁴¹ The advantages of LOC MOS were these: less crystal surface was needed, and higher switching speeds were possible.¹⁴² According to Kooi there was a frequent exchange of personnel between the Nat.Lab. and the Nijmegen factory. At the same time, there were signals that LOC MOS was not fully exploiting its potential. In one of Van Vessem's travel reports of 1974, he made the remark that Philips should promote LOC MOS much more strongly (probably he meant on the American market). Making Philips' own partner, Signetics, more active in the selling of LOC MOS applications even seemed problematic.¹⁴³ The production area also caused problems, to such an extent that we find reference to 'catastrophes such as LOC MOS in Nijmegen' (the words of Elcoma).¹⁴⁴ The production process was very sensitive to dust and good housekeeping.¹⁴⁵ At a certain point the PD Video even had to stop producing a certain product because insufficient LOC MOS ICs were being delivered by Elcoma.¹⁴⁶

As we saw, the invention and first application of LOCOS took place in conjunction with MOS techniques. The idea that LOCOS could also be applied to bipolar techniques was thought up in Philips and Fairchild almost simultaneously. This was more difficult than for MOS because silicon oxide only sinks a few microns, while with bipolar ICs rather thick layers are used, which means that the advantage of the effect is relatively small. This is the reason why the first results were disappointing for

Philips. Fairchild had come up with the name of Isoplanar for this technique. In 1971, at a time when the relationship between Philips and Fairchild was problematic anyway, there was a debate in an exchange of letters between the Fairchild managing director, Lester Hogan, and Leclercq (Philips Board of Management). Both wanted to have first rights. According to Hogan¹⁴⁷ Fairchild had come up with the idea inspired by a 1970 article by Jo Appels at a time when Philips did not seem to have realised the importance of the innovation. De Haan and Rathenau informed LeClerq that Philips had certainly recognised the importance of it and had taken good care to give this technique patent protection,¹⁴⁸ even though it was worded in the patent text in a rather implicit way.¹⁴⁹ In the end, both Fairchild and Philips acquired certain rights.

The last step was to combine LOCOS with both the bipolar and the MOS technique to form the BiCMOS ICs. The advantages of the MOS and bipolar techniques were combined to form C-MOS with its low use of energy and the high cut-off frequency and the high switching speed of the bipolar technique. This was particularly the result of efforts made in Sunnyvale. In general, the later Nat.Lab. research was mostly aimed at solving practical production problems, such as the bird's beak and head problems. In that respect Philips was no exception: American companies also usually considered more fundamental research into ICs to be a luxury that they could not afford.¹⁵⁰ Even as late as in 1976 such an artefact was discovered and given the name 'Kooi effect': sometimes white ribbons along the edge of the LOCOS structure could be observed, because – as was discovered – no oxide growth had taken place there. When Kooi retired, he was given a present that alluded quite overtly to the undesired phenomenon: it was a model of a bird with a white ribbon around its neck.

This case study gives an example of a very successful Nat.Lab. invention that had been developed to a large extent before it was transferred to the factory. As in the case of the Plumbicon, here success did not come immediately after the Nat.Lab. had done its development work, because of the difficulty posed by transfer. This process is typical of the 1946-1972 period, when the demands of the PDs did not play a vital part in determining the research programme. We also saw the selective attitude of the PD. Transfer could have taken place long before 1970, because the undesired phenomena that the Nat.Lab. researchers struggled with could have been worked on in co-operation with the PD lab, but the Nat.Lab. was in favour of bi-polar ICs, and Nijmegen was in favour of MOS, and this hampered the transfer of LOCOS. It was not until the PD had identified a MOS application which to its view was useful to them (the combination of LOCOS with C-MOS) that the idea was transferred.¹⁵¹ The whole story of semiconductor activity at Philips illustrates the difficulties that emerge

when different groups in a company do parallel work with mutual distrust. Efficient transfer of knowledge is very problematic in such a case.

This case study is one of the best ones for illustrating the importance of a good patent position. The Nat.Lab.'s work on ferrites yielded the company considerable cost reduction in acquiring Bell's transistor technology. Later on, the LOCOS patents yielded a considerable income for Philips. In the USA it had been difficult to obtain the patents, because Texas Instruments had made similar claims.¹⁵² It was thanks to the fact that the North American Philips Corporation (particularly its lab in Briarcliff Manor) was able to prove that they had received the LOCOS description before TI put in its application that the patent went to Philips. That meant that according to USA patent legislation, the Dutch priority date would hold for the USA, too, even though the Briarcliff Manor people could not understand the Dutch text. This disagreement between Philips and TI resulted in the patent not being awarded until 1976, which in the end was not unfavourable, because the patent thus remained valid until 1993.¹⁵³ Regarding the matter of the combination of LOCOS with the bipolar technique, Philips had to fight with Fairchild for its patent rights. In Japan, too, the company made quite an effort to establish a strong patent position for LOCOS. The great impact that LOCOS had on IC technology has shown that this effort was certainly worth the investment.

6.4 The VLP

Introduction

Perhaps more than any other case study, the history of the VLP (Video Long Play, a record on which video information was stored) shows how difficult it can be to assess the outcomes of a research project. Some stories about the VLP indicate that it was a failure. Pointing to the large research effort and to the fact that the product was hardly sold can support that vision. When we take into account the fact that in many ways the compact disc was a direct follow-up to the VLP, we might realise that we need to alter our initial conclusions about the VLP being a failure. Then we will also recognise that the knowledge on optical recording gained during the course of VLP research formed the basis to research support for CD development. With respect to the CD there is no doubt that this was a successful outcome. The role of the Nat.Lab. differed in the case of the VLP and the CD. That reflects how the role of the Nat.Lab. within the company had also changed over the course of time. In the 1946-1972 period the lab envisioned being a source of inventions that would be offered for transfer to the PDs. This was the case with the VLP: the idea of how to realise optical recording was born in the Nat.Lab., but when the

lab tried to transfer the outcome, that did not really work. Later on, though, when the idea for a new product (the CD) was initiated in a PD and the Nat.Lab. was called in to support by contributing its knowledge, this co-operation led to quite positive results. This kind of situation was more characteristic for the 1972-1994 period.¹⁵⁴ Apparently in this case, acting as a knowledge resource had more effect than the previous role of acting as a source of innovations.

Both the VLP and the CD are products for which development required a great variety of disciplines. In his dissertation on the CD,¹⁵⁵ Jürgen Lang is right when he states that it is hard to imagine that Philips could have made the contribution to the technical development of the CD that it has made if this central research laboratory with its impressive combination of a wide spectrum of disciplines had not existed.¹⁵⁶ The powerful role of the Nat.Lab. as a centre of knowledge did indeed lie in its bringing together under one roof of what other companies usually had scattered over many different labs.

The Pre-history: Small Pictures on a Record and Other Options

The first time we encounter the idea of an optical way of recording images on a record is in a travel report that was compiled by J.H. Wessels when returned from the Salone Internazionale della Tecnica exhibition in Italy in 1957.¹⁵⁷ There a certain Mr. Rubbiani had demonstrated a sort of optical gramophone record made of perspex that had minuscule pictures glued onto it. Wessels quoted the technical details from the patent text: a diameter of 40 cm, 1500 revolutions per minute,¹⁵⁸ 50 pictures per second, 312 lines in the image, scanned with a light source and a photocell. Later on, A. Versnel discovered that the idea was not new, because he had found patents for pictures in records from 1928 (France), 1933 (Germany), 1934 (USA) and 1936 (UK). In the case of all those patents, there was no evidence to prove that someone had tried to realise the idea in practice. From that point of view Rubbiani's demonstration was new, but Wessels was quite clear in his conclusion: he did not expect anything of it. Some years later we again meet the image record: *Het Parool*, a Dutch daily newspaper, carried a report on March 2, 1966, about a demonstration given by CBS in the USA of a metal record with video recordings that could play for about 33 minutes and was expected to cost about 400 dollars.¹⁵⁹ Another American company, RCA, was alarmed by the news of the realisation of this idea by CBS a year later. In 1964 that company had started thinking about the possibility of recording images on records. One of their options was called Photopix, and it consisted of small pictures on a disc, just in the way Rubbiani had done it. These pictures had to be deciphered by a vidicon pickup tube (see section 6.2). It was expected that this would be too expensive, and therefore preference was given to another option, Discpix

with capacitive information storage (by means of electrical charges). One more idea that RCA came up with was Holopix, consisting of small holograms on a disc, but that was rejected as unpractical.¹⁶⁰ All this was going on in the RCA Labs, a laboratory that at that time was looking for a new strategy by which completely developed products could be presented for transfer to RCA product divisions. The lab was endeavouring to re-establish its relationship with these PDs that had strongly diminished since the lab had focused on fundamental research in the post-war years. In that respect, the RCA Labs resembled the Nat.Lab. Probably at RCA, too, this focus was at least partly caused by Vannevar Bush's report 'Science: the Endless Frontier', the report that had also influenced Bell Labs. The first effort to transfer a fully developed product, a sort of television-based facsimile, had not been a success, but the lab saw no reason for abandoning this new strategy simply because of that one failure. Robert Sarnoff, son and successor of the famous David Sarnoff, tried to make the lab return to a more PD-oriented strategy, but this was all in vain in spite of the fact that the PDs complained about the fact that in their view the research lab went too far in developing the product and in trying to make it perfect from a technical point of view. At the same time there were frustrations within the lab because the PDs left much research output untouched, which was of course seen as missed chances.¹⁶¹ The unexpected CBS demonstration with the video record prompted the lab to seek contact with the Electronics Division, and in 1969 this contact resulted in the successor to the Holopix: the Holotape. That was the year in which Philips also decided to start working on optical discs.

The Race to Produce the First Demonstration

Just like in the case of the Stirling engine (section 6.1), the quest for a disc as carrier of optical information came from one of the factories. Generally, Mr. Wols, who was with the PD ELA, is named as the person who went to the Nat.Lab. with the request to start thinking about such a product for educational applications.¹⁶² The concrete desires were: to have a system that would be capable of recording both image and sound on a two-dimensional medium and would provide random-access, but not necessarily with the option to delete the information again, and not on a ribbon-type of medium.¹⁶³ According to H.J.G. Meyer¹⁶⁴, who was at that time director for devices research, the idea was first raised in the systems research department run by Dr. K. Teer and rejected. That might explain why the project was carried out in the devices department, which from the point of view of the nature of the product was not logical. It also fits in with the fact that in 1969 Teer expressed a clear preference for magnetic recording, while Wols had explicitly spoken about optical recording (videodiscs).¹⁶⁵ The first idea was to work with small images on a disc, of the type Wessels had seen in

Italy.¹⁶⁶ S.L. Tan became involved, even though he belonged to the systems department, because in the Nat.Lab. he was an expert in reading the pictures in pickup tubes. Meyer names Klaas Compaan, an assistant in Wols's commercial department, as having come up with this idea. Probably the idea of holograms had also been considered in the Nat.Lab., just like RCA had done, because it is known that in 1970 a certain H.J. Gerritsen (Brown University, Rhodes Island) visited the lab to deliver a presentation about holography for the purpose of recording images.¹⁶⁷

Both Meyer (at that time managing director of the devices department) and Dr. P. Kramer (then group leader for optics; later E.F. de Haan's successor as research co-ordinator) claimed to have played a major part in the project.¹⁶⁸ Meyer said that he had read a set of memoranda by Dr. Hendrik de Lang on track following systems even before P. Kramer had moved from the cyclotron group in Geldrop to the lab in Waalre. According to Meyer, these memoranda contained some brilliant ideas that caused him to convene a meeting with, for instance, Rietdijk who was an expert in mechanics,¹⁶⁹ and Tan (from Teer's systems department). Indeed, there were progress reports and publications produced by De Lang on this issue that had been written just before De Lang left the Nat.Lab.¹⁷⁰ Unfortunately, no minutes from this meeting have been found. There is, though, a memorandum from Compaan dated February 5, 1970,¹⁷¹ copies of which had been sent to Rietdijk and Tan, as well as to some others who were to be involved in the project (Van Beek, Leblans, H.J.G. Meyer and De Vrijer). In the memorandum Compaan wrote about a 30-cm diameter glass disc with a playing time of one hour. Compaan had calculated that 180,000 pictures would have to be stored on the disc in order to obtain 50 images per second. Each image would then be as small as one-third of a square millimetre. Assuming that there was a constant linear speed, the arc speed would go from 2 revolutions per minute for the outer track to 6 revolutions per second for the inner track. A Plumbicon would be used to read the disc. A reward for working hard on this idea was the demonstration of the 'Video Disc', a mechanical system, thought up by Teldec in 1970.¹⁷² At that time the Nat.Lab. wanted to use its own PD-process¹⁷³ (Physical Development, but also: Philips Dippel after its inventor, C.J. Dippel)¹⁷⁴ that had been developed long before for high-resolution photography. The long-term expectations for the 'reproduction of recorded visual information' were investigated by the Lubben group for 'Technology Forecasting'. This group, which had been formed in 1968, had as its aim to create 'a better connection between research and market and society expectations'.¹⁷⁵ In the meantime, Rietdijk worked on magnetic recording on discs¹⁷⁶ and G. Bouwhuis on the optics of the VLP system.

In 1970 these alternatives were abandoned in order to shift towards recording visual information as an optical FM-coded signal and reading the disc with a laser. Writing on the disc was to be done by means of what

Kramer called 'dimples'.¹⁷⁷ The project was carried out under his leadership, and it is quite evident that Kramer, together with Compaan, played a vital part in the development of the VLP. At that time his group consisted of seven scientists, and it was concerned with optics.¹⁷⁸ Because of the variety of issues involved in realising the idea of an optical disc (apart from optics, there was mechanics, control technology, signal processing, laser technology, production techniques for making the discs), people were 'borrowed' from various places to join the VLP group.¹⁷⁹ As early as the 1960s, lasers had been a topic on the research agenda.¹⁸⁰ At that time there was much uncertainty about the feasibility of using lasers for practical applications (only military applications were expected to perhaps be feasible). The laser activities in the lab continued after that but at a low level. In 1970 it was clear that the price of a laser was still much too high to be implemented in a consumer product. In the first working prototype of a VLP system Kramer had used a helium-neon laser that had cost NLG 15,000. With that laser it was possible to record and read a pattern of black and white blocks (a sort of chessboard).¹⁸¹ In particular, K. Bulhuis was put to work to reduce the price of the laser and to make it reasonable for consumer production.¹⁸² At that time the VLP project did not, according to Kramer, have a high status.¹⁸³ Demonstrations given for the PD ELA did not make a great impression, although the PD allowed ELA developers to co-operate, such as the assistant Piet Burgstede. The PD RGT (Radio, Gramophone and Television) also responded sceptically to the demonstrations, mainly because they were more used to mechanical than optical scanning, but also because the Nat.Lab. was apparently not aware of the problems that could occur if the product was to be put into production (in that connection Ottens recalled a naive remark made by Hajo Meyer in which a complete neglect of production problems was revealed¹⁸⁴). In 1971 the project group presented their work to the Board of Management. That made people take the project more seriously, although it was still not important enough to be discussed at the CRCs, not even in 1972, when the first demonstration was given for the press.¹⁸⁵ During that demonstration the project team showed that they were able to record colour pictures on a disc and read them. The main purpose of the demonstration was to show that Philips too was in the video disc market race, not only with a professional product but also with consumer applications. At that time the envisaged applications were: living rooms – entertainment, educational and informative-schools and industry, television studios (as a writer, instead of Ampex), hospitals (archive systems).¹⁸⁶ For strategic reasons De Haan insisted that the demonstration be formally organised by the PD and not by the Nat.Lab.¹⁸⁷ That would arouse the impression that the product was already close to an industrial stage. It was seen as important to create this impression since competition in the video recorder market showed that whoever was first on the market would to a

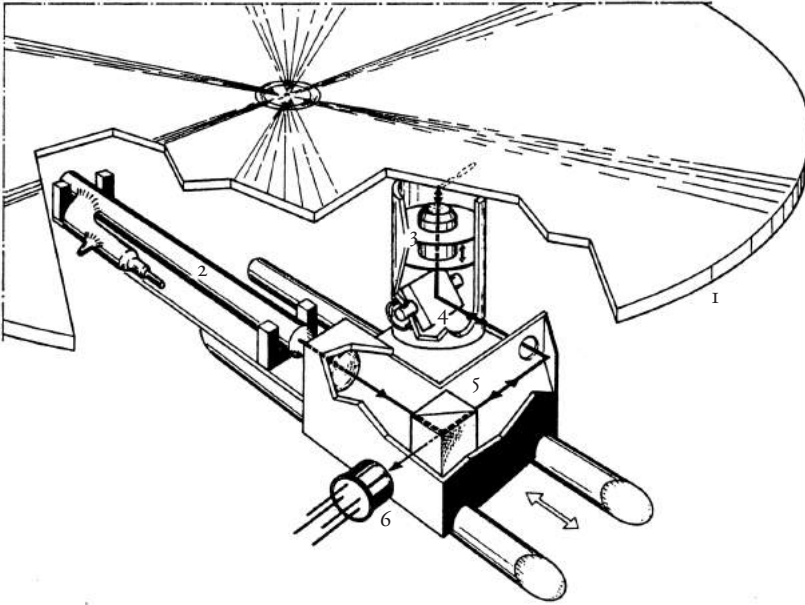


Figure 26. Schematic diagram of a VLP playback unit (from *Philips Technical Review* Vol. 33, p. 180).

Record 1 is scanned from below by light from an He-Ne laser 2. Lens 3 is held focused on the record by a system based on a loudspeaker mechanism. The pivoting mirror 4 ensures that the laser beam remains centred on the track; the mirror is operated by a rotating-coil arrangement. Incident light and reflected laser light are separated by prism 5. Detector 6 converts the reflected light into an electrical signal.

large extent be able to dictate the standards and that it is difficult for followers to change those standards. This was how the race between Philips, RCA, Teldec, and to some extent also MCA and Thomson emerged.

Further Development for the Market Introduction and Transition to the Compact Disc

After the press demonstration of 1972, it was decided that the VLP should go into production. In Strijp a special lab was started and put under the supervision of R. Bom, who had formerly been director of the Main Country Group in North America.¹⁸⁸ A Policy Council was also founded, consisting of Van 't Hoff (PD Audio), De Jong (PD Video), Paling (PD ELA), De Haan (Nat.Lab.), Van Amstel (Polygram), and Solleveld (Polygram). Soon L. Ottens (Audio director) was added to this list. Later on,

we also find the names of H.J.G. Meyer (Nat.Lab.) and Tielens (PD Video).¹⁸⁹ The fact that several PDs were involved illustrates the complexity and the system-oriented character of the VLP. Even more PDs were involved in the project than in the Policy Council, because the PD Lighting was contacted to deliver the lasers, and the PD Glass was expected to take care of the optics.¹⁹⁰ In addition, there were contacts with partners abroad, such as Matsushita, Sony, Zenith and Spectra Physics.¹⁹¹ In 1975 the project was visited by representatives from the American company MCA.¹⁹² After laborious discussions (there was a feeling that MCA was trying to evade some Philips patents)¹⁹³ a contract was signed that led to co-operation,¹⁹⁴ in which it was agreed that MCA would primarily take care of the software (video content).

There were various hints that it was still unclear at this stage what could be seen as the market for the VLP, and the development was more a matter of pushing from the Nat.Lab. to the PDs than of pulling from the PDs. As we have seen, the search was not only for professional applications but also for consumer applications. Whether sound market research was ever done is questionable. M.D. van Hamersveld (CV&P Marketing Research) had proposed allowing the institute Compagnon in Stuttgart to carry out such a research among consumers,¹⁹⁵ but there is no evidence to show that this proposal ever led to serious study. Early in 1974 Polygram hinted that they felt that the project was confusing and that this created a barrier to co-operation.¹⁹⁶ The idea of combining the VLP with an Audio Long Play (ALP) contributed to the fuzziness of the project.¹⁹⁷ Then there was the chicken-and-egg problem of the hardware and software: it was not attractive to develop software while the market penetration for the hardware was still unsure, but on the other hand, this market penetration was not possible without sufficient software offers.¹⁹⁸ At the 1974 CRC meeting, one of the questions asked was whether the consumer's requirements with respect to software were clear. Meyer said that he saw no problems with respect to that.¹⁹⁹ Again, in the case of the VLP, we see an underestimation of the after-research phase. There were also doubts about the affordability of the system.²⁰⁰ In short, it seems that a clear vision on the market situation was lacking. In a meeting organised by the Technical Guidance Committee held in 1974, Kramer stated that in general and particularly with Philips, a vision on the use of the VLP as a new medium was lacking.²⁰¹ Even in 1976, a year before its market introduction, it was reported in an RDC meeting that the Board of Management thought that the VLP perspective was both technically and commercially unclear.²⁰²

Despite all, Philips succeeded in putting the VLP on the market, although much later than expected (originally 1975 had been scheduled²⁰³ and later we find 1977 mentioned as the planned year of introduction to the USA market²⁰⁴). In this case (contrary to many other cases in this period) there seems to have been a smooth transfer to the PD. In 1978 the

market introduction finally took place in the USA. The name LaserVision was chosen. According to the Board of Management, this introduction was favourably received,²⁰⁵ but less than a year later it became evident that sales were below expectations.²⁰⁶ In 1980 RCA also entered the market with their SelectaVision system, and in 1981 JVC introduced their VHD videodiscs. Those other versions soon proved not to be a market success. All the systems suffered from the fact that most software producers developed a preference for videotapes rather than for videodiscs.²⁰⁷

The Nat.Lab. continued to work on the VLP for several years. In 1986 the term 'VLP' disappeared as such from the Research Programme. Until then the lab had worked on various aspects of the system: the optical imaging system, the track following system, the elongation of the playing duration, signal coding, drop-outs with older discs, and the option of using GaAlAs lasers.²⁰⁸ The VLP was also present at CREs until 1983. From 1984 on, the lab only worked on electronic aspects. The reason why the lab continued to work on optical recording was probably because in the meantime a new idea for the use of optical recording had emerged, namely the audio application compact disc.

When the idea of recording video information by optically recording on a VLP was launched, simultaneously the idea was developed to do the same with audio information on an ALP. The Nat.Lab. started working on this in conjunction with the PD Audio's predevelopment lab,²⁰⁹ but it was not clear what the advantages of optical recording would be for that application, apart from the fact that the scanning would not cause the discs to wear out. The notion that it would be possible to put several hours of music on one disc was not seen as an advantage as it was not thought probable that anyone would like to listen to music for hours on end.²¹⁰ It was mainly L. Ottens who recognised the potential of optical recording for audio applications.²¹¹ Meyer told a story (that we could not confirm with any written evidence) of a director of the PD Audio (possibly J. Reineveld) who on one occasion showed a small disc the size of the present CD and said: 'that is what it should look like'.²¹² Apart from the attractive size of what we now know as the CD, Ottens also saw the advantages of digital recording as far as sound quality was concerned. From 1972 on, Toon van Alsem (PD Audio) and Loek Boonstra (Nat.Lab.) were in charge of work on the analogue ALP,²¹³ and in 1978 Joop Sinjou was in charge of the work on the digital recording technology in a special development lab in the PD Audio. Digital signal coding was known about from applications in telecommunications. Because the Nat.Lab. had both this knowledge and the optical recording knowledge that had been gained in the VLP project, they were able to support the PD substantially in developing the CD. So even though the VLP project had been a commercial failure, it had yielded knowledge that had amounted to an important con-

tribution to the realisation of the CD. The supportive work of the Nat.Lab. as a knowledge base resulted in a very strong patent position for the CD.

When the well-known Dutch science journalist Chriet Titulaer wrote an article in 1981 on the video disc in the popular magazine *Natuur en Techniek* (Nature and Technology), he used the title: 'Battle for a million's market', but it became a battle with only losers. The VLP is a good example of a transition from the second to the third period of the Nat.Lab.'s history. On the one hand, the VLP was clearly a matter of pushing by the Nat.Lab. whereby the lab expected to transfer a product that in essence had been completely developed. On the other hand, the work on the VLP was done in co-operation with the PDs. When the transition to the CD was made, the role of the lab changed. The PD took the leading role (thus the CD was subjected to pulling on the part of the PD), and the Nat.Lab. became a knowledge-based supporter.

In the introduction to this case study, another characteristic was mentioned, namely the variety of disciplines that contributed to the work and the strength of the lab as a knowledge source. We have to be aware of the fact that, at first, this bringing together of disciplines within the Nat.Lab. was founded on a very informal basis and organised at the workflow level rather than at the management level. In the next period, 1972-1994, this was to change as we shall see, for instance, in the Berlin optical communication system case study. There the fact that different disciplines were brought together was also of crucial importance, but it was a process that was more consciously directed by the management.

6.5 *Inventions and Innovations*

In the 1946-1972 period the Nat.Lab. saw as its main role within the company the task of serving as a resource for knowledge and inventions, but in practice it tried to develop ideas further to become innovations. 'Fundamental' research would lead to an understanding of phenomena, and this knowledge would lead to the development of new products for the company. Having seen the practice of the research work in the laboratory both at a general level (in Chapter 5) and in more detail (in the case studies in this chapter), one could question whether the specific role of the Nat.Lab. within the company in the 1946-1972 period is well characterised by the claim that the Nat.Lab. as the place where in particular 'fundamental' research was done ('fundamental' meaning: 'aimed at understanding nature' with no explicit reference to concrete products). There is no clear shift in the relationships with universities, as could have been expected if the lab had really shifted its attention to a type of research for which universities would have been a more 'natural' place. There are no indica-

tions of substantial shifts in the publications-patents output ratio, which could have been another indicator that the nature of the research had changed. The case studies, each of which is an example of substantial research efforts in the 1946-1972 period, all show research efforts that have both elements of 'understanding nature', and at the same time there are clear relationships with concrete products. Yet the discussions in the management meetings constantly reflected care for the position of 'fundamental' research in the Nat.Lab.'s research programme. Perhaps the conclusion should be that the function of using the term 'fundamental' was rhetorical more than a guideline for decision-making. In the 1946-1972 period the term 'fundamental' certainly suggested a high scientific status and could be used to claim freedom for researchers. 'Fundamental' research meant to say: research with which the PDs should not interfere. It could function to protect the autonomy of the Nat.Lab. against possible PD influences.²¹⁴

Although the Nat.Lab. saw it as appropriate to transfer product ideas immediately after 'fundamental' problems had been solved, in practice development often took place in the Nat.Lab. as well, because the PDs were seen as being insufficiently willing or capable of taking over the work. The result of this approach was ambiguous. On the one hand the relationship with the PDs was often frustrated, while on the other hand some very successful products emerged from this approach. Those successes, in particular the Plumbicon and LOCOS innovations, counter-balanced a number of (costly) unsuccessful activities (such as the Stirling engine and the VLP ideas). Apart from that, there was research that was focused on existing products. There the Nat.Lab. saw it as its role to gain an understanding of the phenomena underlying those products. That was a perpetuation of the Nat.Lab.'s role as a place where new knowledge could be gained that would be of use to the company. This function had already existed in the previous period, 1923-1946.

This situation changed, however, when resources were reduced in the 1970s and 1980s. In 1965 the first indications of financial problems for the company as a whole were noticed. Immediately the effects for employee numbers were discussed. In the May 1965 RDC meeting it was stated that employee numbers could no longer increase because of the costs. It had particularly been in the first half of the 1960s, research costs had risen sharply. In the November 1965 and May 1966 RDC meetings, this concern was reiterated.²¹⁵ In the next year a 3% personnel reduction for the company as a whole was even contemplated, because of the anticipated financial problems. For research this meant that more people would have to be transferred to 'development, factory automation, commercial planning and operations research'.²¹⁶ In the new situation of reduced resources, the Nat.Lab. could no longer follow the strategy laid down for the 1946-1972

period. If the programme was not made more efficient, the number of 'big hits' would be too small to create a counterbalance for all the unsuccessful efforts. A different strategy had to be found, one that was more closely related to PD interests and which reflected more co-operation in the early phases of development. The struggle to realise this new strategy effectively is what mainly characterised the 1972-1994 period that will be described in Part III.

Intermezzo II

Changing Attitudes towards Science and Technology (1966-1972)

The 1950s and 1960s were periods of economic prosperity. At that time there were ample opportunities for businesses to bring out new products, because the market was able to absorb many new innovations. It was a time when the manufacturers world output quadrupled.¹ In connection with that, energy use also increased: in the USA energy consumption had tripled.² There was a tendency towards a trans-nationalisation of business corporations.³ For Philips, that process had already started long before. The economic surge powered particularly by technological innovations was mainly based on the application of scientific knowledge gained in the Interbellum.⁴ The role of R&D had been strengthened, and the attitude towards science and technology had been positive. By the end of the 1960s, though, there were 'signs of wear and tear'.⁵

In 1968 inflation started to increase. Furthermore, the international monetary system came under pressure due to the US national budget deficit. In 1971 this led to the collapse of the Bretton Woods system, which had provided a stable monetary basis since 1945. In the Netherlands the economic developments differed from those in the rest of Europe. Since 1960 there had been a situation of over-investment. Even though profitability had decreased, industries were not diminishing their investments. Wages increased at a rate that was not in proportion to the position of industries. The share in the world market kept increasing, while profit margins went down. It was to result in a dramatic end to the economic boom period. In 1973 there was the oil crisis with its worldwide impact, and in particular for the vulnerable Dutch economy, that brought with it great adaptation problems.⁶ This had consequences for Philips, too.

It was not only in an economic sense that the world was changing in the late 1960s. There was also a wave of social unrest that started in about 1968. A 'wave of rebellion' swept the globe.⁷ The motives for this rebellion differed in different parts of the world.⁸ Part of the ideology that was being protested against was the indulgence in luxuries that new technological innovations kept offering. There came an awareness that science and technology not only brought blessings, but also provided powerful means for destruction. This feeling soon spread to other segments of the population as well. A plea for the social control of technology was heard. In the USA,

this led to the establishment of the Office for Technology Assessment (OTA) in 1972. This organisation was commissioned to conduct studies into the social effects of new technologies. In other countries similar organisations were gradually set up in later years.⁹ The uncritical acceptance of technology had come to an end.

Within industry there was a further change in thinking. The obviousness of the industrial effects of 'basic' research was challenged. People started to question the assumption that this type of research would always and virtually automatically result in industrial breakthroughs. In the late 1960s certain authors published books in which the importance of the role of the marketplace in innovations was stressed. Authors such as Carter and Williams (1966: *Industry and Technical Progress*), Schmookler (1966: *Invention and economic growth*) and Myers and Marquis (1969: *Successful Industrial Innovation*) became well known in industrial circles and helped to create a different, more critical, attitude towards 'basic' or 'fundamental' research.¹⁰ Alongside with this a change in management emerged within companies that can also be seen at Philips. Companies tended to value more strongly multidisciplinary research that is focused on concrete problems rather than specialised, disciplinary research. Gibbons et al. have come up with the term Mode 2 for this type of research.¹¹ They too discuss the emergence of this new type of industrial research against the background of the changes mentioned above.

One particular issue that had given rise to a change in public attitude towards technology was the concern about the general natural environment. In 1972 the first report emanating from the Club of Rome¹² was published in which the need for setting 'limits to growth' was proclaimed.¹³ There was good reason for concern with respect to the available material and energy resources, as the report pointed out. Even though the problems later appeared to be less alarming than the Club of Rome's report suggested, its message was supported with so many figures and arguments that the impact on policy was substantial. The Club of Rome's report was not the first warning about environmental damage to ever be given. In 1962 Rachel Carson published a book, entitled *Silent Spring*, that dealt with the effects of chemicals, such as insecticides, on the natural environment. This book became a 'classic' in the entire environmental debate. Stimulated by such publications as *Silent Spring* and the report of the Club of Rome, and of course by the 1973 oil crisis, environmental pressure groups started to step up their activities. In the Netherlands a debate was started on nuclear energy and its perceived dangers. A number of environmental organisations were set up in the early 1970s (De Kleine Aarde, Stichting Natuur en Milieu, Vereniging Milieudefensie and Wereldnatuurfonds Nederland, all in 1972, and the Stichting voor Milieu-

educatie in 1974). These organisations not only drew attention to the dangers of nuclear energy, but also to various environmental issues.¹⁴ The specific focus on energy problems also had practical implications for the Nat.Lab. research programme in the early 1970s (e.g. projects were set up on the improved use of energy sources, and on the measurement of pollution). The concerns expressed by the environmental organisations were gradually supported by scientific evidence on the influence that the production and use of technologies was having on the natural environment. In 1973 for example the ongoing depletion of the ozone layer contributed by CFC use was proven. Since 1970 evidence of the global warming effect of CO₂ has been systematically collected and published.

Finally, we can point to the changes seen in the Japanese industrial policy in the late 1960s and early 1970s. Until the mid-1960s, Japanese industries had mainly obtained their licences from USA companies and had produced with little innovation. From 1960 onwards, the Japanese MITI, Ministry of International Trade and Industry, started setting up small programmes to improve the technological capabilities of Japanese companies. In a 1966 study the MITI expressed a desire to look for more dramatic changes.¹⁵ In particular, it was the need to innovate in the computer field that was seen as being important to technological capability in Japan.¹⁶ In the years 1976-1979 a large state-funded research programme helped Fujitsu, Hitachi, Mitsubishi, Nippon Electric and Toshiba to establish sound world positions in the field of Very Large Scale Integration (VLSI) of Integrated Circuits (ICs). In general, the R&D effort was substantially increased: in the 1978-1982 period there was an annual growth in R&D funding of 15%.¹⁷ This made Japan a serious competitor on the world market and caused companies in the USA and Europe to seek co-operation with Japan, such as in 1992 when Toshiba, Siemens and IBM agreed to become involved in a joint effort to develop a 256-megabit IC memory.¹⁸

In reaction to the rapid growth in Japanese R&D efforts and the continuous R&D efforts in the USA, scientific co-operation between European countries grew. European co-operation was not new: in 1957 the European Economic Community (EEC) was established, later renamed the European Community (EC) in the 1980s, and in 1993 it received its current name of European Union (EU). The first-mentioned name change serves to indicate that the co-operation had begun to extend beyond economic agreements. Joint scientific research became part of the whole agreement. In 1982 the pilot phase in the European Strategic Programme for Research and development in Information Technology (ESPRIT) was launched. The Framework Programmes¹⁹ enabled universities to carry out research programmes in co-operation with industry. Philips also took part in several of the projects set up within the bounds of these Framework Programmes, and the Nat.Lab. was often involved in such projects.

In 1988, the European Commission published its 'European vision of research and innovation policies for the 21st-century'. The title of this report reflects the changed attitude towards the role of science in innovation. The name alluded to Vannevar Bush's famous 'Science, the Endless Frontier' report, but this title had been changed into 'Society, the Endless Frontier.'²⁰ Evidently, the emphasis had shifted from science to society as the driving force behind technological innovations. It is this change that we also recognise in the 1972-1994 period when we examine the history of the Nat.Lab.

PART III

The Road towards Mutual Commitment
(1972-1994)

7. Redirecting the Research Organisation for Mutual Commitment

7.1 The Problematic Economic Climate

The economic problems that were described in the second *Intermezzo* had an effect on the whole period which will be dealt with in this third part of the Nat.Lab.'s history. Instability and stagnation characterised the economic situation of the 1970s and 1980s.¹ From 1973 on, the growth in world trade decreased, which also made Dutch exports decrease and inflation and interest rates rise. The first years of economic decline affected the Netherlands less than certain other countries in Europe because the consumption continued to increase. This was all possible because in the 1970s all the economic problems were generally passed on to the government (allowances and minimum wages rose). The economic problems were to hit the Netherlands extra hard from 1979 on when this effect was no longer being felt. The way in which the Dutch unemployment rate developed also differed from other countries. The industrial overinvestment policy of the 1960s had led to a sharp rise in salaries, which in the late 1960s started to cause financial problems for a number of industrial companies. Between 1969 and 1973 the number of unemployed people doubled. In the years between 1980 and 1984 the unemployment rate increased even faster than in other countries because of the increased number of women in the labour force.

These economic problems also had an impact on Philips. Van Zanden and Griffiths wrote of the 'relative success' of the Philips company when it came to dealing with the economic decline.² but the first signs of financial problems became visible at Philips quite early on.³ Its turnover continued to grow rapidly until the mid-1970s, which illustrates that there was continuing consumption until the early 1970s, in spite of the economic decline. The company's profits went up and down.⁴ One of the most troublesome factors for the company in the early 1970s was the monetary instability and the relatively high rate of the Dutch guilder. In the 1970s the Netherlands also became one of the most expensive countries in terms of labour costs. Some PDs had more difficulties than others. Several PDs were faced with a temporary stagnation in turnover increases in the mid-1960s (the exceptions being Lighting, PIT, Pharmaceuticals and Polygram, the phonographic PD; for the Medical Systems PD the temporary stag-

nation came a few years later). In the second half of the 1970s, there was again stagnation in turnover increases for a number of PDs (the exceptions then being the Lighting and PIT PDs). Again, this came early on compared with the overall developments in the Netherlands, with 1979 being the year in which circumstances worsened. During the whole 1972-1994 period special measures had to be taken to increase the efficiency of the Philips company. The climax was no doubt the company-wide Centurion programme initiated by the CEO J. Timmer in 1990. Some business activities were completely abandoned in order to get rid of unprofitable parts of the company. Customer-oriented product development was also greatly stimulated. 'Customer days' were organised in order to raise awareness, in the employees' minds, of the need to focus on customer demands. The role of industrial design was enhanced, which was to improve the quality of the appearance of a great variety of products.

In the 1972-1994 period, scientific and technological developments were taking place particularly in the area of Integrated Circuits (ICs) and digitalisation. As a result of that, computer technology became widespread. Personal Computers (PCs) were introduced to offices in the 1980s and later also at a rapid rate to households. A second important digitalisation field was that of the recording and transmission of data for communication. Optical recording and optical communication were fields in which Philips participated and to which the company made important contributions, not in the least because of the work done in the Nat.Lab. The company was not very successful in the field of computers. In 1962 the first plans to initiate a separate PD for computers were made, and in the next year it was founded.⁵ The PD had very ambitious plans, to such an extent that there were worries about the company's possibilities to make the required investments. These investments were made, and a series of computers called Prooo were developed, but soon it appeared that sales were disappointing. In 1973 Philips, Siemens and the French Compagnie Internationale pour l'Informatique (CII) started a joint venture under the name of Unidata. According to Alfred Chandler, Unidata was 'a last desperate and futile attempt to build a European mainframe base'.⁶ This venture was abandoned by the French in 1975, and Philips decided to stop all its activities in large and medium-sized computer systems. At that time IBM was sovereign in the field of computers, and even a strong American company such as RCA had pulled out of this field in 1971. After Unidata, Siemens turned to the Japanese company Fujitsu to provide it with IBM-compatible mainframes. This was a fairly successful move, and it helped Siemens to survive the computer battle. In the late 1980s Philips was faced with a painful situation when it was forced to give up its ambitious activities in the field of sub-micron SRAM ICs in the Mega project. The factory created for producing Static-RAM ICs was closed down even before

it had had the chance to go into proper production. The PC activities did not result in a good market position, and so they were abandoned, too. A lack of software caused customers to look for alternatives, in spite of the technical advantages of the Philips PC. Here too the IBM competition was too strong for Philips. Rather than producing IBM-clones, as most other companies did, Philips stuck to its own P2000 microcomputer technology, for which different software was needed. It was not only in computers that Philips was faced with a marketing problem. In the 1972-1994 period Philips experienced more product failures because of this. The V2000, a video cassette recorder produced in the early 1980s, was not a commercial success (only JVC's VHS system survived the competition). The same had happened with the Video Long Play system that had been launched in the late 1970s. The sensational success of the CD was at least partly due to the fact that Philips forced its daughter company Polygram to produce a range of CD titles. The introduction of the CD-Interactive again showed that the market certainly did not accept all new products, in particular when software was scarce. The High Definition Television (HDTV) and the Digital Compact Cassette (DCC) of the 1990s were other (costly) examples of market failures. Fierce Japanese competition in all these fields was another factor that contributed to the company's problems.

Despite all this, the company managed to survive these difficulties. Whatever criticism there may have been from the side of the management, the company has until today remained an important partner in the field of electronics. In Part III of the Nat.Lab.'s history, an important question is: how did the Nat.Lab. function in this turbulent period? What contribution to the company's survival was required from and indeed offered by the research organisation? How did the Nat.Lab.'s task profile, its culture and organisational structure change in response to the changes going on in its environment? How was the relationship with the PDs adapted to the new needs of the company? Those are the sort of issues that will be discussed now.

7.2 Towards a PD-oriented Task Profile

In 1972 Pannenberg succeeded Casimir as the research representative within the company's Board of Management. In the same year he gave a presentation to the Nat.Lab. management in which he gave his view on the desirable research developments within Philips.⁷ In his presentation Pannenberg sketched a number of differences between the 1946-1972 period and what he predicted for the coming years. In the first place he had the impression that there was now, more than 'in the time of Langmuir and Coolidge', a lot of scientific knowledge outside industry that needed

to be adapted by industrial research organisations for use within their respective companies. He also maintained that the knowledge that was to be developed in these research organisations was of a less 'basic' character than in the past (he mentioned the development of the transistor to highlight one contrast with the 1972 situation). With respect to the position of the research organisation within the company, he defended the idea of retaining a continued separate and independent research organisation, even though, according to him, this was not the practice in other large systems companies such as Bell AT&T and Siemens, but he insisted on decreasing the attention being given to 'technology push'. For him, the term 'technology push' implied that the initiative for a development was within the research and/or development organisation itself. He pleaded for increasing attention to be paid to the 'market pull', in which the original impetus is a social and/or market need. In order to realise that in practice, he believed that it would be necessary for the research organisation to have more direct contacts with the market. To give an example of how this could function, he mentioned the work being done on measurement equipment for air pollution, something which would have been unthinkable without the intervention of a certain Dr. L.A. Clarenburg, who was a user of this equipment. Finally, Pannenburg pointed out that the financial problems of the company forced the Nat.Lab. to move towards a more selective attitude to research topics. Pannenburg showed that the annual cost per lab scientist had risen tenfold in the 1945-1972 period (from NLG 5,000 to 50,000 per scientist). The company could no longer deal with such cost increases, and so research had to become more cost-aware. Although at the end of his speech Pannenburg hastened to say that he, as Casimir's successor, had no intention of getting rid of the excellent traditions established within the Nat.Lab., he announced new directions for the research programme. Even though the traditions would not simply disappear overnight, according to Pannenburg there would certainly be a gravitation towards a new position for the research organisation within the company. Of course, this had an impact on the content of the research programme.

From 1972 on Pannenburg chaired the Corporate Research Conferences (CRCs), where the research programme was discussed by the Nat.Lab. and foreign affiliated sister labs' managing directors and directors. It is striking to see that the scanning of the most recent scientific and technological developments in the world, which had always had a prominent place on the CRC agendas in Casimir's time, did not reappear on the agenda in Pannenburg's days. That, of course, does not mean that all attention to scientific development had disappeared, but it does indicate a change in the debate priorities. Instead of scanning scientific developments at the beginning of the CRC, the participants were extensively informed about the

state of affairs within the company. This fitted with the ideas that Panenborg had expressed in the speech mentioned above: research should be more oriented towards making scientific developments of the past fruitful for the company than on embarking on research into new scientific fields. The task profile changed from being involved in doing research of a 'fundamental' character towards being keen to make 'fundamental' knowledge fruitful to the PDs.

The new orientation to the needs of the PDs can be illustrated through the agenda of the 1978 CRC, which was entirely structured according to the company's existing PDs: Professional I (Telecommunication and Defence Systems, Professional Data Systems, Nationale Kabel Fabriek), Professional II (Medical Systems and Devices and Science & Industry), Elcoma, Consumer I (Audio, Video, Electro Acoustics) and Consumer II (Light, Small Domestic Appliances, Major Domestic Appliances). It was the first time that the CRC agenda was related to the PDs so explicitly, and from then on that would be the case for nearly all the CRCs.

Two particular areas where past scientific developments could be made fruitful for the company were found to be in the computer and the IC sectors. The first-mentioned topic was discussed at the 1972 CRC. As we saw in Chapter 5, the Nat.Lab. had built a number of computers for use in the lab in the 1960s (PETER, PASCAL and STEVIN). Some projects such as the Themis memory project and the semiconductor memory project had focused on specific parts of a computer. Now research was becoming more concerned with the development of computers and computer equipment as a new Philips products range. A new PD had been set up in the company in 1962. In 1971 the idea to start working on 'small computers' was launched in Teer's systems department,⁸ but it was not immediately realised. In 1977 the idea was launched again, but now in a more concrete form. It was suggested that the company should start thinking about computers that could be used in the home.⁹ Apart from having a central processing unit (CPU), the components should at least comprise a screen, a keyboard and some memory capacity, and the optional apparatus functions should be financial administration, data storage and retrieval, an electronic diary, a calculator, word processing, and the visual representation of data. Game options should also be included. The assignment was given to the scientists Kreuwels and Vos who were instructed to come up with a project proposal. The latter scientist was involved in the Geldrop project centre (later in this chapter, this project centre will be dealt with in more detail). In their report, published in December of that same year, Kreuwels and Vos referred to the 'hobby computer' trend in the USA and argued that the Large Scale Integration (LSI) IC technology would certainly make the home computer technically possible to realise. In the USA

about a hundred companies had already been producing such computers, but until then these small computers had required very sophisticated user knowledge. Here there was an opportunity for research to considerably increase the user friendliness of this apparatus. The Video Long Play (VLP) was considered to be an option as a storage medium. In September 1979 Teer reported that the 'personal computer' project was led by Vos and located in the Geldrop project centre and that it had become evident that a variety of fields of expertise was needed for it (Very Large Scale Integration, ergonomics, computer architecture, software research, development of peripheral equipment, etc.).¹⁰ Contacts were maintained with several PDs: Data Systems (formerly known as 'Computers'), Video and Elcoma. In June 1980 the project team came up with a proposal to start building a computer that would have the following characteristics: a 32-bit processor, 128-256 Kbytes of working memory, 20 Mbytes of background memory of the Winchester type, a digital cassette recorder as an additional storage memory, a real time clock, a keyboard, cursor control, display touch, an A4-size high-resolution display, a printer and a communication bus to connect personal computers with the outside world. In the discussion on this proposal, developing the software was seen as the main problem.¹¹ Meanwhile, the company had become involved in the Unidata joint venture. As was mentioned before, this joint venture did not have a long lifespan. Already at the 1976 CRC it had been necessary to have a discussion to consider the role of research in data processing 'after Unidata'.¹² Bosma, who was the group leader for that area at that time, expressed the opinion that research should focus on 'decomplexing artificial systems' and exploring new areas for application. The Nat.Lab. and the Hamburg lab supported the Data Systems PD in the 'after Unidata' era by working on speech recognition, printers, peripheral equipment, modelling and simulation, computer architecture and parallel processing, systems design, future offices and optical recording. Again, software was mentioned as being a key issue. There were difficulties when it came to making a transition from a hardware-dominated to a software-dominated approach.¹³ Pannenberg later suggested that this had to do with the fact that in software there did not seem to be any 'Maxwell laws', as in physics. This made software an uncomfortable topic for research.¹⁴ At the 1984 CRC meeting, one of the questions raised was whether or not information science was a discipline.¹⁵ The observation that there seemed to be no 'fundamental' laws in the area and the questioning of the 'disciplinary' character of the fields would suggest that the Nat.Lab. was interested in those sorts of 'fundamental' considerations, but that it found that the field was not very suitable for such a 'fundamental' approach.

The Nat.Lab.'s work in the field of ICs can also be used to illustrate a change in the Nat.Lab.'s task profile. As we saw in the previous period, the

Nat.Lab. entered the field of transistors at an early stage, but it was very late to take up IC developments. However, in a relatively short time it was able to catch up and establish a good position in IC technology through the invention of LOCOS and of I²L (Integrated Injection Logic, a technology that allows for an increase in the density of components in bipolar ICs). At the 1974 CRC meeting, it was stated that research into the physics behind the IC technology (the more 'fundamental' type of IC research) was by then no longer being given priority in the way that it was in the 1950s and 1960s. Research had now shifted towards the relationship between IC design and production.¹⁶ New trends in ICs were being identified: upcoming digital techniques, Large Scale Integration (LSI), low energy dissipation, fast circuits, and a need for standardisation. The ICs were not only important for computers, but also in a number of other areas in which signal processing played a part (e.g. optical communication – see the case study on AD/DA-conversion in the next chapter – and optical recording). In particular, Pannenberg made great efforts to stimulate European co-operation as he felt that this was a must if Philips was to survive developments continuing in the USA, and later also in Japan. In 1975 part of the RDC meeting was spent on discussing proposals for European research projects,¹⁷ including in the area of energy. From then on, EC research proposals were to become an almost recurrent topic on the RDC agendas,¹⁸ but since Philips was the only real multinational electronics company in Europe at that time (Thomson and even Siemens were much more nationally oriented), according to Pannenberg it was difficult to get the critical mass required to get a European industrial co-operation started.¹⁹ In 1978 Pannenberg remarked at the CRC that he had tried in vain to start up a European project on Very Large Scale Integration (VLSI). That probably also had to do with the fact that Elcoma was not interested in European work on ICs at that time.²⁰ In the 1980s the field of ICs was given a substantial technological impetus through the Mega project. This joint project undertaken with Siemens was an example of European co-operation. In 1983 an agreement was reached with Siemens, and a proposal was submitted to the European Committee on silicon submicron IC technology.²¹ In the year before, a steering platform had been initiated in which the former Dutch Minister for Science Policy, Van Trier, was involved as well as representatives from twelve telecommunication firms, which defined 15 optional projects.²² The Philips-Siemens proposal for the 1-megabit (1-Mbit) chip was accepted, and the companies started working on a 1-Mbit memory Random Access Memory (RAM) IC. It was agreed that Siemens would work on a Dynamic Random Access Memory (DRAM) while Philips would work on a Static Random Access Memory (SRAM). The PD Elcoma was the formal contract partner for the Mega-project. This PD commissioned the Nat.Lab. to carry out the project for them. This is an example of a new relationship growing between the

Nat.Lab. and the PD: it was not the Nat.Lab. that had tried to transfer research output of its own accord, but rather the PD that had commissioned the Nat.Lab. to carry out a specific research task for them. For Philips the sub-micron ICs were a new area.²³ A lot of extra people were taken on to work on this project. This even caused some concern because some people were worried that the Mega project would push aside other important IC activities.²⁴ It was felt that not only IC technology, but also IC design needed plenty of attention: the Nat.Lab. should also pay attention to 'expressing' oneself 'in silicon', as it was phrased.²⁵ In 1987 Philips was able to present the first ICs to the Dutch Minister of Economic Affairs.²⁶ Behind the scenes there had been a lot of problems (such as tension in the PD and uncertainty about the applications of the ICs; see section 7.5). Because of these problems the Mega project ended abruptly in 1989, and many people had to be laid off again (there was even the extreme situation of new people sometimes being hired in the morning and fired in the afternoon on the same day!²⁷). This was quite a dramatic time for the Nat.Lab. A complete dismantling took place. The new WAX building in which production was to have started was transferred to the PD Components on January 1 of that same year. The building was renamed FAB1, and the MOS R&D activities were concentrated there.²⁸ Siemens's choice to develop dynamic RAMS (DRAMS) appeared to be a better option. In 1985 this company started a co-operation with the Japanese company Toshiba, because it had noticed that times were getting more difficult as the IC market declined. In 1987 Siemens successfully launched a 1-megabit DRAM on the IC market, which by then had picked up again.

Terminating projects was something exceptional in the 1946-1972 period, because there were always opportunities to take up new research topics that were interesting from a 'fundamental' point of view. In the 1972-1994 period, though, the PDs' needs became more important as a criterion for taking up or finishing research topics. Research lines were abandoned if no PD was found to show serious interest in them. Thus, the 1970s saw the end of groups such as the Stirling and biology groups, two groups that had worked on issues for which there was no evident interest in any of the PDs. In addition, we find that in the 1972-1994 period some groups only had a short lifespan, because they were started and ended in this period. In a number of cases their starting up was the result of the social developments described in the second *Intermezzo*. Two examples of such research areas will be discussed here (they were not yet the result of PD-interests, and in that sense they are only typical for the beginning of the 1972-1994 period).

In the first place there was the Lubben group that worked on futurology. This was a somewhat controversial group: Casimir was never very

impressed by its outcomes,²⁹ but Pannenberg expressed admiration for what the group accomplished.³⁰ The group was unusual in that its work was not based on any of the lab's 'hard' disciplines, such as physics, chemistry and electrical engineering, but only on 'softer', social disciplines. This fitted in well with the trend towards a greater social awareness, something that was expected of scientists in the early 1970s. By the early 1980s, however, when the effects of this awareness had waned, the group (by then led by Rademaker) had been dissolved.

Another field that appeared to be 'fashion sensitive' was the energy research field. In 1973 the international oil crisis had aroused a strong awareness of the need to study the way the energy supply could be realised should traditional resources become exhausted. In 1970 Ducot (LEP's managing director) reported on the CdTe solar cell research taking place in his lab. Most of the energy research, though, was concentrated in the Aachen lab. In 1974, one year after the oil crisis, energy scarcity had become a separate issue on the CRC agenda, and a debate was active on the potential offered by solar and nuclear energy. It was in particular in the next CRC that solar energy was identified as a possible future substitute for fossil energy resources. On that occasion the work that was being done at the Aachen lab on energy systems for houses was presented. An experimental house had been built to enable experiments on energy losses, solar heat collectors (in co-operation with the Lighting PD), and on heat pumps to take place. The Nat.Lab. meanwhile worked on components for energy systems, and the LEP did some research on photovoltaic cells. A special group, to be led by Van Zwet, was initiated to provide contacts with the market, as energy systems involved a new line of production for Philips.³¹

Research was also done into environmental air pollution measurement equipment. This work had been started in 1966. As we saw in the second *Intermezzo*, this was a period when environmental concern had started to increase. A measurement equipment prototype was transferred to the PD PIT, and after that the Dutch government ordered equipment for the Rijnmond area. This equipment had started being used in 1969. In 1970 surface water pollution had also been included in the research programme. A group led by Kroon was working on this.

Between 1976 and 1978 the importance of both areas (pollution and energy) already started to decrease. The Kroon group was dissolved in 1977, and the Energy and Stirling groups, led by Spigt, were substantially reduced in 1978.³² Two years later the whole Stirling group was dissolved, and the Energy group was renamed the Thermodynamics group. At the LEP and in Aachen, too, energy research was terminated.

In general, the late 1970s and early 1980s were a period when several groups were dissolved or merged with other groups. This had not often

happened before. Here, too, we see the result of a more selective attitude towards research topics for which there was no direct PD relationship or interest (or, as in the case of glass and ceramics, when the respective PD lost its status as a Product Division and was transformed to a supplier organisation, in 1980; the lab's ceramics group was partially moved to Aachen, and the remainder was merged with the glass group to become the 'inorganic materials' group). Between 1976 and 1986 the following groups disappeared from the Nat.Lab. research programme: Volger (applied physics), Rademaker (futurology), Spigt (Stirling), Botden (physical technology; this group moved to the CFT), Druyvesteijn (thin film heads), Elgersma (biology), Vreken (gas discharges; this group was moved to Aachen), Klosterman (pickup tubes and cathodes), Albers (ceramics), Thomas (glass), Rietdijk (electrical and mechanical engineering), Wolter (experimental physics) and Tichelaar (welding; the group moved to the CFT). The fact that a group disappeared from the Corporate Research Programme did not always mean to say that research in that area completely disappeared as well, but at least the group was reduced sizewise so that it could be included in the research area of another group (e.g. glass/ceramics³³). It is striking to see that several of the groups being moved or disappearing dealt with classical areas which, in some cases, had existed almost since the beginning of the lab's history. The reason for this is not very obvious. In the 1946-1972 period the argument probably would have been that they did not offer sufficient opportunity for 'fundamental' research, but later a more probable argument would have been a lack of relevance to the PDs. It is not clear, however, why there should be a lack of such relevance.

The diminishing interest in 'classical' physics was replaced by increased efforts to make quantum mechanics fruitful for the development of new products. We have already seen the ICs as an example of that. The laser is a second example of something that was to become very important to the research programme. As Pannenberg had stated in general terms in his 1972 speech given to the lab managers, the focus had shifted from developing the fundamentals of quantum physics towards using all the newly gained understanding of the behaviour of microscopic structures and particles for industrial ends. Two application areas in particular should be mentioned: optical recording and optical communication. The first-mentioned has already been described in connection with the case study of the VLP (Chapter 6). The other area will be discussed in Chapter 8, when we study the case of the Berlin optical communication system. At the 14th CRC in 1976, the field of optical communication was a separate issue on the agenda for the first time and great chances were expected because of the perceived change in attitude towards optical communication at the Dutch telecommunications company, the PTT.³⁴ The laser in particular

was a suitable field for research. At that time the GaAlAs laser was seen as the best option for optical communication, and research was to be aimed at attaining longer lifespans, higher power output and wavelength flexibility. Materials research was seen as a prerequisite when it came to achieving those aims. For glass fibre, too, materials research was needed.

The ICs and optical recording and communication fields are examples of new research lines that proved to be viable, but implementing 20th-century physics was no guarantee that a research line would be spared the critical eye that had caused a number of 'classical' research lines to disappear. As was mentioned before, the primary criterion more and more was becoming the interest of the PDs. This can be illustrated through the topic of magnetic bubbles; these were small, cylindrical domains of opposite magnetism in thin films of magnetic ferrite material. Already in 1960 C. Kooy and U. Enz had reported on that finding,³⁵ but it took several years for people to realise that these bubbles could be used for information storage.³⁶ In the Nat.Lab. one group was working on the growth and purity of such layers, and in Hamburg a group was working on the problems that occurred when these layers had to be reproduced within the desired tolerances. It was known that Bell Labs was also working on magnetic bubbles for memories. In 1972 it was predicted that high investments would be needed if these bubbles were to be used for computer memories.³⁷ In 1976 there was again uncertainty about the feasibility of using magnetic bubbles for memories.³⁸ In 1982 the work on magnetic bubbles ended, because there was still too much doubt about the chances of success of this alternative to the much more dominant silicon technology, and as a result, no PD was willing to invest in this technology. Bell Labs had also terminated their activities in this field.

In a similar way, the area of superconductivity was really outback in 1988 when there was still insufficient indication of any concrete result after many years of research.³⁹ Already in 1960 some research was being conducted on superconductivity in the Nat.Lab.⁴⁰ For a long time, the group led by Volger did research into the superconductivity of metals. The invention of high temperature ceramic superconductors in the mid-1980s – realised elsewhere than at Philips – led to a research project in which the Nat.Lab. and foreign Philips labs were involved. The Lubben futurology group had done some work on the possibilities of applying superconductivity.⁴¹ Here, too, the fact that no PD was seriously interested was reason enough for the Nat.Lab. management to decide to abandon working in this field.

The growing emphasis on carrying out research that could more or less be directly used by PDs even in a certain sense again stirred up the debate on

‘fundamental’ versus ‘applied’ research. There was always some space for ‘fundamental’ research into new phenomena for which no application was yet envisioned. In 1974 Polder (from the physics main group) lobbied for keeping ‘a healthy bit of physics’ in the lab programme, even though no continuous stream of new applicable phenomena could be expected.⁴² Several other directors supported this plea for ‘fundamental’ research. In 1976 the research into III-V semiconducting materials was criticised by the Crystal Growth Committee for being too oriented towards applications, too concerned about commercial feasibility, and not sufficiently long-term and ‘fundamental’. It was clear that the role of ‘fundamental’ research was not the same as it had been in the 1950s and 1960s. In 1980 there was a debate at the CRC on the categorisation of research programmes, and Pannenburg again commented on the drop in ‘phenomena-oriented’ research. According to him, this type of research had reached a phase of maturation so that now concrete applications could be looked for.⁴³ This again shows the changed attitude towards the need for this type of research. However, concern remained about the role of the Nat.Lab. as a stimulus for the PDs’ long-term product policy, which in the 1946-1972 period had been the main motive for doing ‘fundamental’ research. In 1986, P. Kramer, chairperson of the CRC, mentioned the need for ‘long-term’⁴⁴ research in his introduction to that meeting. According to him, the number of real breakthroughs at Philips was few compared with the 1950s and 1960s (the period when, for instance, the Plumbicon and LOCOS were developed). He thought this was due to the fact that research had become too reserved about pursuing topics for which no direct applications were yet apparent. In 1988 (at the 20th CRC) it was again stated that such ‘risky’ research was indispensable if the company was ever to be able to catch the big ‘Phish’.⁴⁵ The relationships with universities – outlined in section 5.4 which made it clear that even in the previous period these relationships were not seen as important with respect to the content of their research programmes – should be maintained, but this could never replace the need for the company’s own ‘exploratory’ research. The ideal percentage suggested for ‘exploratory’ research was 20% of the total research effort. At the same time the need to observe ‘Philips awareness’, and not from a laboratory chauvinism angle or an ivory tower position, was underlined. The following new areas of ‘risky’ research were identified: software and systems, new devices, human interfaces, and manufacturing concepts. Evidently, concrete applications were thought of, which indicates that the term ‘risky’ research was not exactly equivalent to ‘fundamental’ research. It was also stated – and here we see another important difference from the 1946-1972 period – that marketing should have a say in the selection of these ‘risky’ research areas. In 1986 though, the Board of Management representatives Van der Klugt and Van Houten (Pannenburg’s successor on the company’s Board of Management) gave the first indications at a CRC

meeting that the company was planning to take steps to further force the Nat.Lab. to focus on the direct needs of the PDs. The amount of corporately funded research (100% of the research budget at that time) would be reduced so that the percentage of short-term research carried out for PDs would grow. This soon became a reality for the research organisation because contract research was introduced only a few years later.

In 1989 the principle of total direct research organisation financing by providing corporate funding through the Board of Management was abandoned, and contract research was introduced. From then on, the research organisation had to acquire approximately two-thirds of its budget by submitting contract proposals to the PDs.⁴⁶ In principle, the inherent danger of this transition was that the amount of short-term research (based on contracts with PDs) would increase at the cost of long-term research. Looking back at the introduction of contract research, some PD managers expressed concern about the continuation of long-term research.⁴⁷ There was also the danger that materials research, which was often not oriented to a specific product, would be more difficult to continue on a contract basis and so would get into a difficult position.⁴⁸

Concern about the continuation of long-term research and maintaining the balance between materials, devices and systems research was couched in new terminology such as 'key technologies' and 'capability management' in the 1990s.⁴⁹ When F. Carrubba, who had moved from Hewlett-Packard to Philips in 1991 and who had succeeded S. van Houten in 1992 as holder of the research portfolio on the Philips Board of Management,⁵⁰ presented a survey of what had happened during the first few years following the introduction of contract research, he spoke of realigning the research portfolio in the direction of the key technologies that should underpin the company's core competencies. He then listed the following areas: optical technologies and optical systems, materials technology for new devices (such as magnetic heads and lasers), process technology for displays and lamps, (electro-)mechanics and mechatronics, data processing and image processing, AD/DA-conversion, source and channel coding, systems integration and architecture, human interaction (speech, visual, tactile interaction), software engineering (methods and tools), BICMOS and bipolar technologies, and analog and digital circuit design. The key technologies and capability management concepts amounted to a reaffirmation of the idea that the research organisation should remain the knowledge centre of the whole company. That meant that a number of key technologies and capabilities should be kept alive even though there were no possibilities for expanding contract research related to these technologies. An effective capability portfolio should ensure that for all the project work, sufficient know-how was present within the research organisation.

One of the ways of financing activities for which no contracts could yet be drawn up, but which comprised strategic key technologies and capabilities, was by means of Self-Financing Activities (SFAs). These were focused activities, which, it was hoped, would yield enough income to be self-supporting and thus to survive either until they could be made independent or until they could be transferred to one of the PDs. With such activities Philips Research took care of the research, development and sales of the product. The budget was kept separate from the normal research budget. The first SFA was concerned with lasers for optical communication. After some years the SFA yielded a turnover of about NLG 60 million and proved profitable.⁵¹ Examples of other SFAs were: image sensors, electronic design and methodology, and consultancy.⁵² In 1991 about 200 people were involved in the SFAs.

The company-wide Centurion operation that started in 1990 was an important aspect of the Nat.Lab.'s search for a new task profile. In March 1991⁵³ a meeting was held with representatives of Philips Research, together with representatives from two other parts of the company. It was chaired by Van Houten. This Centurion II (II, because it dealt with a second management layer in the organisation, I being the top level) meeting ended with a 'Declaration' in which the participants stated the following: 'We, the senior management of Corporate Research, CFT and CPT, hereby declare that to us a research success is a real success only if it is a business success. Specifically, we commit to deliver projects which will increase significantly the number of business successes and decrease the time to market of new products. Our aim is to become the world's leader based on these criteria. We have formulated ten actions that are instrumental to this goal. On the basis of these actions, we will contribute significantly to (1) creating major new businesses for Philips, (2) moving existing businesses to the number one position in the world, (3) changing fundamentally the rules of the competition game to boost substantially the profitability of existing Philips business.'⁵⁴ This text well illustrates the strong rhetoric that was being used in the Centurion process. It also illustrates that Centurion was aiming to push Research further into the direction that had already been established in the 1970s, when the influence of the PDs on the research programme started to be augmented. The Centurion III sessions followed in May and June. This was the phase in the process when the group leaders became involved. The sessions were used to identify issues and proposals for actions concerning the envisioned role for Research to create new business opportunities for the company.⁵⁵ It was decided that these sorts of discussions should be continued in an annual 'Research Strategy Meeting' with representatives from the PDs. Since then, these meetings have been held every January, and about two hundred participants attend them. The Review Meetings with the Group

Management Committee (see section 7.5) were also used to define the strategic fields in which Philips Research should be active. In the 1991 Review Meeting it was proposed that a shift was necessary in the capability portfolio: materials and processes research was to be decreased, 75 people were to go), the mechatronics field had to be strengthened (25 new people had to be taken on), and also the Systems and Software field was to be expanded (by 50 new people).⁵⁶ These numbers refer to the part of the research budget that was covered by the contribution from the GMC.

In January 1992 Centurion III meetings with Research were held again under the leadership of Dr. Kees Bulthuis.⁵⁷ The discussions were about increasing the market input for R&D and comparing the Philips R&D with other, external R&D organisations. In Timmer's response to the outcomes of the meeting, he emphasised the need for collective responsibility for Research and the PDs rather than separate as 'new spirit' activities.

In the 1992 Review Book we can see how the same sort of tools found in product development organisations were increasingly used for strategy defining within Research: we find terms like capability and project portfolios, portfolio plots (competitive position versus maturity, derived from ADL's 'Third generation R&D'), and also the term Total Quality Management is used. In the 1993 Review Book we also find the use of so-called Technology Roadmaps, which were used in the PDs, too.⁵⁸ F. Carrubba had fulfilled a supportive role in introducing such tools and methods into the Research organisation.

When Carrubba presented his review for the period 1989-1993, he called it a 'period of transition' in which the formal decision to introduce contract research was put into practice. By the end of 1993 there had been a certain consolidation of the changes that the Research organisation had gone through in the turbulent years reflected on by Carrubba. The Research organisation had found a new task profile in the company.

7.3 Limited Resources

The end of the period of relative wealth in the late 1960s had consequences for the lab's resources. Cost-effectiveness became an important issue in lab management discussions. We will first see how this influenced the relationship between the Nat.Lab. and the labs abroad, and how it affected the transfer of personnel from the Nat.Lab. to the PDs. The measures that were taken could not prevent the financial position of the Research organisation from being drastically changed by the Board of Management. The transition to contract research will be described, and so will the financial measures that had to be taken when the whole company went through the Centurion process of further rationalisation.

Growing Co-ordination between the Labs Abroad

Although no direct steps were taken to cut the research budget in the late 1960s, the research management realised that this would just be a matter of time, and that it was necessary to get rid of undesirable overlaps in the research programmes of the various labs. In Part II we saw how the labs abroad functioned rather independently from the Nat.Lab. In the 1972-1994 period this changed, and there was a movement towards one coherent research programme, dispersed between the Nat.Lab. and the foreign labs. Thus, the concept of 'Philips Research' as one coherent research organisation for the company emerged.

When Pannenburg took over Casimir's position on the Board of Management in 1972, he created the position of Corporate⁵⁹ Research Co-ordinator. This task was first fulfilled by G.W. Rathenau⁶⁰ and later by E.F. de Haan (from 1974-1984), P. Kramer (from 1984-1990) and K. Bulthuis (from 1990 on).⁶¹ De Haan said that he used to visit each lab two or three times per year to discuss the outcomes of the CRCs and RDCs, thus continuing what Rathenau had started doing. P. Kramer said that he started to interfere with the management of the other labs in order to get some co-ordination of activities. According to him, the resistance came not only from the labs abroad but also from the Nat.Lab. people, who saw the other labs as secondary and certainly not worth deliberating with. Therefore, they were not happy about the fact that Kramer was the first research co-ordinator to not be the chairman of the Nat.Lab's directorate at the same time. In the past, the Nat.Lab.'s managing director had also been in charge of the co-ordination between the labs, which reflected the idea that the Nat.Lab. was the primary lab; from then on, these positions were given to two different persons.⁶² This evolution shows how the status of the labs abroad had gradually changed. In this period we see a move from having a set of mutually independent labs, with their own individual programmes, towards one research programme divided over the labs.⁶³ The CRC and RDC meetings, which until then had mainly facilitated the exchange of information between the labs, were used to establish co-ordination between the labs.

The main lines of research being pursued in 1972 are shown in Table 4.

From the survey presented in Table 4, it is apparent that each lab had its own areas of specialisation, but there were also fields that were studied in more than one lab. The participants of the CRC saw the differences between the various national developments as a barrier for preventing the labs from functioning as one multinational research organisation. At the same time there was awareness that the problematic economic circumstances in which the company found itself forced the labs to enhance co-

Table 4. The research programmes of the labs in 1972.⁶⁴

Lab	Research fields
Nat.Lab. (the Netherlands)	<ul style="list-style-type: none"> – systems department (Teer): computer data systems, communication systems, instrumentation systems, – devices department (De Haan): pickup tubes, displays, videodisc project, IC technology, mechanical research, – chemistry department (Vink): semiconductors, ceramics, glass, luminescent materials, quasi-amorphous materials, liquid crystals and chromic materials, composites, surface studies, molecular biology, colloid chemistry, electro chemistry and inorganic chemistry, photoconductivity, photovoltaic effect, polymer chemistry, analytical services, welding processes, environmental research, magnetic bubbles, – physics department (Rathenau): futurology, magnetism and metals, Stirling technology.
Mullard (UK)	<ul style="list-style-type: none"> – materials (ferro-electrical and semiconducting properties), – image and imaging equipment (image converters, channel plate intensifiers), – semiconductor applications: ion and electron beams, – microwave equipment (mainly for military purposes), – computers, – television receivers.
LEP (France)	<ul style="list-style-type: none"> – special tubes: photomultipliers, gamma scintigraphy, cathode ray tubes, Titus tube, image tubes, – solid-state detectors, – microwave and fast electron equipment, – educational equipment: alphanumeric displays, punchcards and printers.
Aachen (Germany)	<ul style="list-style-type: none"> – light research: light bulbs, halogen lamps, flash lights, high pressure discharges, metal halides and transparent materials, – di-electrical and ferro-electrical materials (capacitors, piezo-electrics for telecommunication applications, high-power acoustics, high-power generators and ultrasonics, ceramics, – physical-chemical processes: electrophotographic processes, chemical vapour deposition, solid-state pyrolysis, magnetic materials and advanced measuring techniques, – small electromotors.
Hamburg (Germany)	<ul style="list-style-type: none"> – measurement and control systems, – sensing and transducer components, – medical X-ray equipment, – two-colour laser display, – CAD for IC design, – office equipment, – holography.

Lab	Research fields
Briarcliff Manor (USA)	<ul style="list-style-type: none"> – silicon vidicons and other solid-state pickup tubes, – infrared devices, – luminescence, – Stirling technology (in particular for an artificial heart),
Brussels (Belgium)	<ul style="list-style-type: none"> – applied mathematics: language theory for software, – network theory and circuit theory (mainly digital circuits).

ordination in order to avoid duplicating research studies which, of course, would be a waste of money. A more rigorous selection of research topics should also be applied to each of the labs' research programmes. These priorities should be determined individually per lab.⁶⁵ Already in 1972 there was an example of prioritising a specific research area per lab, that was with microwave research, which took place in several labs. The lab in the UK had already worked on that during the WWII, when it was not yet part of Philips, and the proposal now was that this research should be split up into specific sub-areas that would be spread out over the labs (Nat.Lab.: only p.i.n. diodes and transistors for microwave equipment, Hamburg: only ferrite materials for microwave equipment, Mullard: radar and navigation systems and accompanying circuits, France: emitter systems and accompanying circuits).⁶⁶ Another early example of prioritising across the whole corporate research programme, and of one specific lab being given entire responsibility for a certain topic, was in 1973 when the Aachen lab received 'concern responsibility' for energy research.⁶⁷

In 1974 the international aspects of research were again discussed in the CRC. This was done within the context of the ideas of the various National Organisations (NOs) in contact with external organisations, such as (national) governments (in most cases not much government support was expected). The conclusion drawn was that the internationalisation of systems research would probably be very difficult. Apparently there was specific concern about the independence of the lab in the USA (Briarcliff Manor). In the conclusions drawn by the CRC, it was mentioned that the contact with this lab should be intensified. For the first time it was stated in 1976 and in the form of a policy that decisions relating to the Briarcliff Manor research programme should be taken in Eindhoven.

In a 1978 RDC the overlap in software research between the Nat.Lab. and the Brussels lab was discussed.⁶⁸ This too was an indication of the growing awareness of the need to co-ordinate the research programmes in the labs. In the 1979 RDC meeting, it was stated again that contacts between the labs had to be improved. No concrete suggestions on ways of attaining that goal were mentioned. Only in 1984 do we find enhancing the staff exchange mentioned as a concrete way of improving these con-

tacts.⁶⁹ In October 1986 this point was stressed again in an RDC meeting.⁷⁰

From 1983 on, the frequency of the RDC meetings about doubled. The reason for that was the growing need to select and allocate research topics.⁷¹ Concrete concentration and allocation decisions were taken in 1984 for the first time at an RDC meeting: lamps and electromotors were to be concentrated in Aachen, III-V semiconducting material electronics in the LEP, and laser research and integrated optics at the Nat.Lab.⁷² When in 1985 proposals for European research in the RACE-programme were discussed, a specific lab was assigned responsibility, each topic (e.g. GaAs high-speed ICs and characterisation of conductor/semiconductor interfaces in the LEP, and the integrated optical transmitter and coherent glass fibre components in the Nat.Lab.). In January 1986 a special committee was appointed to investigate the dispersal of the total research programme over the various labs.⁷³ It was suggested that more research should be concentrated in the labs abroad. In October 1986, it was specifically the overlap between the Nat.Lab. and the American labs' activities that was discussed in order to improve research efficiency.⁷⁴ In 1988 efficiency considerations led to a substantial concentrating of research activities: the Nat.Lab. should stop activities in III-V hetero-epitaxy, phosphors,⁷⁵ robotics, fast GaAs circuits,⁷⁶ and reduce glass and ceramics activities. The Aachen lab was expected to reduce service activities, and finally the Hamburg lab should reduce research into magnetic-optical storage and stop digital angiotomography, broadband optical communication and its materials reliability project. In the same year it was claimed that contacts with the PDs should be made on the basis of the whole research programme and not on the basis of the activities of individual labs. This was quite a change when one thinks of how the labs had originated with their focus on national opportunities. It suggests that for the labs abroad, the concentration had shifted from relationships with the NOs to relationships with the PDs. This was part of a company-wide change (often referred to as the tilting of the NO-PD matrix). For the first time it was stated that the research programmes should not take the individual labs as a starting point but that one programme for all the labs put together should be developed.⁷⁷ No clear conclusion had yet been drawn at that time, but the fact that the issue was debated at all is a clear sign that things were changing.

Financial Constraints as a New Justification for Transferring People

In Part II the transfer of people from the Nat.Lab. to the PDs was described as one of the mechanisms used by the Nat.Lab. to influence the PDs. Transferred people took with them the knowledge and experience gained in the Nat.Lab., and thus augmented the PDs. In the 1970s the

transfer of people continued, but the motive for this shifted. The lab management realised that it was no longer possible to take on new scientists under the 'zero growth' conditions if an insufficient number of people were moved to the PDs, thus not creating enough vacancies in the Nat.Lab.

At an RDC meeting in December 1972, it was decided that a special committee should be set up to study the possibilities of maintaining the current vitality of the research population on the basis of (continued) 'no-growth' in the number of lab employees. The committee consisted of Belevitch (MBLE, Brussels), Harrison (PRL, UK), Hörster (PFA, Aachen), Tummers (Nat.Lab.), Valkenburg (Nat.Lab.), Vellix (LEP, France), and Winkler (PFH, Hamburg). In March 1973 the committee brought out its report. In that report the transfer of research workers to the PDs was seen as the most important contribution to solving the problems of maintaining vitality in the 'zero growth' situation. The consequence of transferring too few people would be that the average age of the scientists in the Nat.Lab. would rise. The authors strongly advised adopting a conscious transfer policy within the company, so that PD and NO vacancies would be filled first with scientists from the research labs (the report dealt with both the Nat.Lab. and the foreign research labs). In this way the research organisation could continue to take on new personnel without growing. According to Valkenburg, the consequence of this was that the possibilities for transfer to a PD became part of the criteria when selecting new people.⁷⁸ According to an appendix of the 1973 report, the average age of the scientists was 37.9 at the Nat.Lab. Of the foreign research labs only the MBLÉ had a significantly younger population: there the average age was 32.5. The annual transfer rate from the Nat.Lab. was 7.8% according to the report. For the other labs this varied from 3.7% (PRL) to 8.8% (PFA) and 8.9% (PFH). Keeping one's eyes open for chances to be transferred to PDs was already part of the existing career guidance policy; for the people in scale ('vakgroep') 5 and lower, this was the responsibility of the group leaders. For those in scales 6, 7 and 8, it was a task of the director or managing director.⁷⁹ In the *Zero growth* report the relationship between the Nat.Lab.'s Personnel Department, the Corporate Staff Bureau and the PDs' Personnel Departments was referred to as 'good' and it was mentioned that the number of transfers from the Nat.Lab. to the PDs was already increasing.

In the 1972-1994 period the largest outflow of scientists was to the PDs Elcoma and Consumer Electronics (Audio and Video). The scientists who transferred to Elcoma were mostly physicists and chemists. Most of the scientists who went to CE were electrical engineers, and only a few were physicists or chemists. The third outflow was that to the CFT, but the situation was different there because this was not a PD but an organisation that, like the Nat.Lab., supported PD activities. In some cases activities

were moved from the Nat.Lab. to the CFT (for example the Botden group was moved in that way). Here the outflow was more equally divided between the disciplines.

The number of years that scientists spent in the Nat.Lab. did not decrease very much over the course of time, it remained on average about seven years, the same number as was found for the 1946-1972 period.⁸⁰ On average, physicists and chemists stayed longer than electrical and mechanical engineers in the 1972-1994 period as well as in the previous period of 1946-1972. Mathematicians and information scientists also had a shorter average circulation time (about five years).

Thanks to the outflow, the Nat.Lab. was able to attract new scientists even in years that were very difficult for the company. In 1990, the last year before the company-wide Centurion operation was mounted, the Nat.Lab. still took on 120 new employees (in 1988 that number was 114).⁸¹

The Introduction of a New Financing Mechanism for the Nat.Lab.: Contract Research

In a meeting with the Nat.Lab. research directors held in 1989, S. van Houten announced on behalf of the Board of Management the introduction of contract research. From then on, about two-thirds of the research budget had to be obtained from contracts with the PDs. The formal ruling was that 0.3% of the company's turnover would be allocated to research directly by the Board of Management (previously that was about 1%), and the remainder would have to come from the PDs.⁸² Meijer still remembered the silence that fell after P. Kramer had told the news in a meeting of the Nat.Lab.'s managing directors.⁸³ At that time the introduction of contract research was seen as a defeat for the research organisation that had always so fiercely defended its financial independence. Not only Holst and Casimir, but also Pannenberg had operated in that way. All three had held the position of Research representative on the Philips Board of Management. Van Houten said later on that he had a different view on his position: research was in his portfolio, but he did not feel himself a representative of the research organisation on the Board. Unlike Holst, Casimir and Pannenberg, Van Houten had spent a substantial period of his Philips career outside the Nat.Lab. In Chapter 5 we saw that he was made responsible for the CFT (Centre for Manufacturing Technologies) and that he had held that position until Pannenberg retired. Probably his emotional distance from research made him the appropriate person to be involved in the introduction of contract research. According to Bulthuis he had not prepared for the introduction of contract research by communication with Research itself.⁸⁴ But none of that made a real difference because by then contract research was known to have been introduced in several other large companies, so Philips could hardly be expected not to be part of that trend.⁸⁵

The idea of contract research was not new to the Nat.Lab. directors. In 1979 there had been a debate on this. A special Research committee had produced a report on the pros and cons of contract research versus the existing, corporate way of financing the Corporate Research Programme. The committee members were: Mooijweer (Corporate Research Bureau⁸⁶), Bosman, Gelling, Valster (Research directors) and Hazewindus (head of the company's Corporate Product Development Co-ordination bureau). This committee had been initiated because of the concern that had been expressed within the Philips Board of Management about the PDs' medium-term product policy and the possible enhancement of Research's role in all of this. In its report the committee sketched four scenarios: (1) continuing the current financing mechanism without any type of formal contracts, (2) using formal 'contracts' in which PDs could express agreement with certain parts of the research programme, while the financing still came directly from the Board of Management, (3) having financial contribution to the research work coupled with the contracts, and (4) making part of the research the direct responsibility of the PDs. The first scenario was rejected by the committee because it would not bring the necessary changes in the co-operation between Research and the PDs. The fourth scenario was also rejected because it would mean losing the advantages of a central company research programme (and in particular the combination of quite different fields of knowledge). Although the committee at first claimed that the differences between scenarios (2) and (3) were only of a 'psychological' nature, later on in the report it was stated that the 'realness' of the contracts with connected financing resulted in a definite preference for the third scenario.⁸⁷ To achieve agreement on the contracts, the committee recommended annual research meetings with each of the PDs, chaired by a member of the Board of Management. An appendix in which the actual situation was described in terms of research effort in contrast to the PDs' development effort accompanied the report. From these data it was concluded that about 40% of all research could be viewed as 'applied' and suitable for doing on a contract basis. One problem, though, was that half of this research effort was oriented to Elcoma, while the development effort of this PD would only allow contracts to be made with Philips Research for half of that. Other PDs that seemed to have the same problem were particularly TDS, Lighting, Audio, Video and Medical Systems. The committee doubted the feasibility of transferring scientists from Elcoma-oriented work to work related to these other PDs, given their educational background. In 1983 a further survey yielded about the same results: about 40% of all applied research was for Elcoma, while for each of the PDs TDS, Lighting, Audio, Video and MSD, this value was only about 5%.⁸⁸

Already in a response to the Board of Management meeting during which the role of Research in the PDs' medium-term product policy had been discussed, Pannenburg had made clear that in his opinion the current financing mechanism should not be changed.⁸⁹ What he had suggested as an alternative was in fact what was to become the committee's second scenario. In a meeting on October 22, 1979, during which the report was discussed, the third scenario was rejected, and structural deliberations on the research programme in the R-PD management committees were seen as sufficient for enhancing the role of research in the PDs' medium-term product policy. For the time being that was the end of the discussion. In his introduction to the 1980 CRC, Pannenburg mentioned that contract research had been rejected but that Research should 'keep thinking about it'; however for years to come this remark did not seem to have many practical implications. In the report later on, Kramer picked up scenario (2) in his Transfer Projects (see later in this chapter).

Soon after the introduction of contract research, in April 1990, K. Bulthuis, who had succeeded P. Kramer as international research co-ordinator, had to come up with an Economising Plan for Philips Research (the Nat.Lab. and the labs abroad). The negotiations with the PDs that had started after contract research had been announced had shown that the PDs were very cost-conscious when responding to the contracts offered them by the Nat.Lab. In the Economising Plan a cost reduction of about NLG 120 million was scheduled for three years.⁹⁰ This even exceeded the amount decided upon by the Philips Board of Management, which had been announced in the 1989 Review Meeting. According to the plan, more than 70% of the reductions had to come from the foreign labs. In the 1990 Research Review Book the amounts were translated into personnel numbers. The entire population of the labs had to be reduced from about 4,300 to about 3,000 employees. The reduction would lead to the Hamburg lab being reduced and made into an annex of the Aachen lab, to closure of the Brussels and Sunnysvale labs, and to the reduction of the labs in the UK and France.⁹¹ The remaining 30% of the reductions had to come from the Nat.Lab.

In relation to costs, the term 'quality' also became an important issue for the lab management. That was remarkable, because this concept was usually concerned with reproducible and predictable processes, mainly in production. In 1987 there was a first 'quality action', led by M. Carasso and a first Mission Statement was formulated for the Nat.Lab, in co-operation with M. Weggeman.⁹² In January 1990 a second quality action was announced, this time to be co-ordinated by F. Carpay.⁹³ As Bulthuis remarked in 1988 in his annual speech, quality assurance actions were quite new to the Nat.Lab. culture and perhaps also for research organisa-

tions in general, and concrete quality assurance actions were not easy to realise. The fact that quality management was clearly emerging at Philips Research shows that in that respect, too, the previous distance between research and the PDs, where the need for quality management had already been recognised, had diminished.

In September 1990 the company's CEO, J.D. Timmer, published his plans for getting the company out of its downward spiral.⁹⁴ From October 1990 on, his 'Centurion operation'⁹⁵ was discussed with the top 100 upper management people in the various areas of the company during a series of workshops held in the conference centre 'De Ruwenberg' situated in the village of St. Michielsgestel. Huge personnel reductions were announced in all segments of the company. According to the Centurion plans, each part of the company had to relinquish 15% of its personnel. September 30, 1990, was taken as the starting date for that. This put Research into an unfortunate position, because Bulthuis's Economising Plan had already become effective, and substantial reductions had already been carried out by that date. It would mean that Research would be cut more drastically than most of the PDs. During the 1990 Review Meeting with the Board of Management, the Research representatives tried to prevent this by claiming that what had already been accomplished through the Economising Plan should be included in the Research reduction targets. This meeting was of course a very important one for the future of Research. The tension could be felt when the Board of Management entered the meeting room, and Timmer sat down and proceeded to stare out of the window.⁹⁶ Bulthuis gave an introduction that had been rehearsed over and over again during the days leading up to the meeting. Only after about an hour did Timmer actively take part in the discussion, and in the end the research management was very relieved when it was decided that the Centurion reductions would not be imposed on top of the Economising Plan reductions.⁹⁷ The realisation of the Economising Plan had to be accelerated so that the total reduction could be finished by the end of 1991.

The Nat.Lab.'s Contribution to the Patent Portfolio as a Defence for the Budget

Many people in Philips Research may have seen the introduction of contract research as a diminishing of the lab's independent position. What was positive for them was that the company had maintained the research organisation as such, even during the Centurion operation. Rather than splitting the research programme into parts and dividing those over the PDs, as had happened at some other companies,⁹⁸ the Philips company management retained a corporate research organisation. It was recognised that the multidisciplinary nature of a corporate research organisation

yielded an input synergy that would be lost if Research were to be divisionalised.⁹⁹ Another argument that the research organisation brought forward, was that of the contribution of the Nat.Lab. and the labs abroad to the company's patent portfolio. As we saw in the 1946-1972 period, concern was sometimes expressed about this. Such concerns were evident in the 1972-1994 period, too, but of a different nature. In his introduction to the 1986 CRC meeting, Kramer passed on the remark that was made by the Philips Board of Management that long-term research deserved extra attention, because the number of real innovations made by Philips was small. During the next CRC, in 1988, the patent position was again mentioned, and it was stated that although in terms of output quantity the research contribution was quite good – on average 14% of the company's R&D budget was spent on the 'R' (the Nat.Lab. and the foreign labs) and this effort had resulted in 40% of the company's patents – whether or not the patent position was strong enough in the most strategic fields (CAD, VLSI and software) could be questioned.¹⁰⁰ In his annual speech given in January 1989 Bulthuis mentioned the following threats to Philips' patent position: growing Japanese competition, increasing aggression in the patent world, growing patent awareness in smaller firms and non-industrial patent owners, patents in shared projects thus meaning that patent ownership might be problematic, and the expiry of a number of important Philips patents (such as LOCOS). Bulthuis showed that the value of patents was not only to be found in one's own freedom of acting and the direct licence incomes, but also in cross-licence possibilities and the saving of licence costs that would otherwise have to be paid to other firms. This argumentation was used to show that Philips Research for the company was not only an expenditure post but also a source of income. According to Bulthuis, this could well justify the existence of the research organisation as a corporate entity. Still, in 1988, Bulthuis mentioned this argument in an interview and stated that Philips then had 60,000 patent rights, 40% of which came from Research and that on an annual basis the value of those patents exceeded the annual Research budget.¹⁰¹ Probably, Timmer had acknowledged this when he decided to include Bulthuis's Economising Plan in the Centurion reductions for Philips Research. In 1993 a calculation was presented to the company's Board of Management to ground the claim that the annual Research budget was well covered by the patents that were produced.¹⁰² The income was calculated by adding the direct monetary income of the licences and an estimation of the value of cross-licences (this was separately done for each of the PDs) and subtracting the cost of the licences which Philips needed from third parties and for which Philips actually paid.

7.4 *Changes in the Lab Culture*

Some of the influences that the changes in economic circumstances had on the culture of the lab, such as the introduction of quality actions and the use of management tools, have already been described in previous sections when the task profile and the means were also discussed. Not much has yet been said about the influence that the change in public attitudes towards science and technology had as a social factor. In this section we will concentrate on that facet. Valkenburg, who was the Head of Social Affairs at the Nat.Lab. in the 1970s, once spoke in an interview about a 'democratisation wave' that had invaded the lab at that time.¹⁰³ This may be a somewhat exaggerated way of characterising what happened, but it does reveal that real changes did take place in the early 1970s. These changes were to have a lasting impact on the lab culture. In Parts I and II the lab's culture was described in the context of the tensions seen in professional organisations. Here we find that there was tension between management control and the influence of the individual researcher. The 'democratisation wave' in fact meant that, even more than before, scientists became aware of the importance of their professionalism and decided that they should have quite a say in the management of their organisation and in the research programme. This led to a change in the contacts between scientists and the lab's directorate.

It was not only the 'general public' that was concerned about the possible negative impact that science and technology might have on society and the natural environment. It was also a feeling among the Nat.Lab. scientists. This is furthermore evident from the debates that took place in the Nat.Lab in the early 1970s. In 1971, the Contact Committee (CoCo, see Chapter 5) proposed having a series of lectures in the lab on the Club of Rome's report, centering on the theme 'Science and society'.¹⁰⁴ In the 1950s a similar activity had already taken place, and Dr. Dippel, a Nat.Lab. scientist who had published several critical articles and monographs about the role of science and technology in society, had been one of the speakers. The lectures had been collected and published under the title 'Research and ethics'.¹⁰⁵

In 1972 the minutes of the working groups, which had accompanied the new series of lectures, and the lecture texts themselves, undersigned by 123 Nat.Lab. scientists (including the directors Rathenau and Teer), were distributed as Technical Note no. 274/72. This shows that at least part of the lab management was not opposed to this activity and, to a certain extent, even supported it. On the other hand, we find a remark made during a later Directors-CoCo meeting to the effect that management forbade the publication of 'controversial' texts.¹⁰⁶ A proposal by the CoCo to initiate a council for the relevance of scientific know-how was rejected by the Nat.Lab. management.¹⁰⁷ Likewise, the Nat.Lab was prevented from par-

ticipating in the conference on 'Faith, Science and Society' in 1979.¹⁰⁸ Such facts show that the lab management did create boundaries for the social debates continuing within the lab. That is something that may have disappointed certain people and confirmed their ideas that the lab directorate was still serving the interests of industrial growth too much. The Nat.Lab. directorate itself felt that it had succeeded fairly well in finding a balance between providing an opportunity for social debate while at the same time preventing an atmosphere of social unrest that might have hampered the continuation of the research work.¹⁰⁹ In particular, it was Dr. H.J. Vink, chairman of the Directors-CoCo meetings, who had the delicate task of keeping that balance. In a speech given at the Eindhoven Polytechnic in 1970,¹¹⁰ Vink expressed his opinion as follows: 'That is why we, engineers and non-engineers, in a free and conscious forming of opinions must try to find social structures and mechanisms through which our social priorities and all the relevant functions can be explicitly formulated, while at the same time conserving democracy. These structures should be such that industrial enterprises can realise optimal social efficiency, while conserving competence and responding to the social challenges.'

Nat.Lab. scientists also participated in social debates continuing in other organisations, such as the 'Verbond van Wetenschappelijke Onderzoekers' (Association of Scientific Researchers). This Association provided a platform for debate among scientists about the role of science in society. From its very beginning in 1946, Nat.Lab. people like E.J.W. Verwey, J. Volger, J.J. Went, J. Voogd and G. Ittmann and some years later C.J. Dippel and H.J. Vink, who have already been mentioned, participated in this Association. In the early 1970s it was particularly P. Bolwijn who tried to realise a revival of the VWO's activities in Eindhoven. The Nat.Lab. directorate was not always happy with the VWO's activities. In 1975 it did not permit the Nat.Lab.'s works council (Ondernemingsraad) to participate in a VWO survey on the activities of such councils in industrial organisations.¹¹¹

It was not only the emergence of the social debate on science and technology that changed the agenda of the Directors-CoCo meetings. In 1970 we see the arrival of another new social issue, namely the drive to democratise the personnel policy. The CoCo asked the directorate to give the personnel a say in the appointment of group leaders.¹¹² The CoCo also wanted a clear definition of the task of group leaders.¹¹³ The directorate did not always respond positively to such requests: a proposal made by the CoCo to use a list of fixed criteria to appraise the performance of scientists was rejected by the directorate.¹¹⁴ Yet in the same meeting, the CoCo reported with some appreciation that the openness with respect to such appraisals had already grown. Several years later we find that the performance appraisal of assistants had found its way onto the agenda of the

Directors-CoCo meeting.¹¹⁵ In another meeting the CoCo proposed letting scientists (i.e. those lower than group leaders) participate in the decision to stop certain research programmes. In general, the discussions in the Directors-CoCo meetings were open, and the CoCo, where the employees who were scientists chose the chairman, was able to determine most of the items on the agenda.

In this period we also see that various issues at the management level replaced the debates on all sorts of practical issues, which characterised the agenda of the Directors-CoCo and Kern meetings in the 1946-1972 period. A discussion in the Directors-CoCo meeting took place on the relationship with the PDs.¹¹⁶ The CoCo expressed concern about the possibility that the number of research topics that were directly relevant to some PDs would increase and that there would be less 'fundamental' research.¹¹⁷ The CoCo also expressed doubt about the usefulness of the Corporate Research Exhibitions.¹¹⁸ On a number of occasions, the CoCo sought clarification of the rumours about the introduction of contract research.¹¹⁹ The CoCo also challenged the structure of the research programme. The discipline-oriented structure of the lab (with its main groups for chemistry, physics and electronics, which according to the directors corresponded with research into materials, devices and systems) seemed to the CoCo to form a barrier to contact with the product-oriented PDs.¹²⁰ Finally, we find that the CoCo introduced contact with the universities to the agenda. In that respect the CoCo wanted more formal contacts because the universities seemed to have more to offer to the industrial research labs than before, but the directors refused to further formalise contacts with any universities.¹²¹

Thus we see that the CoCo almost becomes a sort of second works council for the scientists, leading to discussions in the Directors-CoCo meetings on the different roles of CoCo and the formal works council.¹²² In 1974 the Kern (see Chapter 5) was turned into a formal works council for the research organisation. Before then, only a works council for the company as a whole had existed. Composing the Kern agendas, and later the works council agendas, we see the same sort of changes emerging as those seen in the Directors-CoCo meetings' agendas: varying from practical issues to more strategic ones. Yet the CoCo continued, the argument being that for the Nat.Lab. management the CoCo could constitute a useful informal counterpart to the more formal works council.

The co-operation between the directors and the works council was positive.¹²³ The directors and the works council discussed various parts of the Collective Agreement. In the works council it was not only the interests of the scientists that were dealt with, but also those of the assistants and technicians. Regarding the assistants, the increasing automation of the

measurement equipment and the processes gave rise to problems: more and more the need was felt to ensure that there would be sufficient opportunity to acquire more responsible tasks, like processing measurement data and reporting outcomes, making suggestions for new experiments, doing theoretical background studies, participating in the formulation of the research programme, and taking care of external contacts.¹²⁴ As we saw in Part II, this change in the role of the assistants had already begun in the 1960s. Around 1980 concerns were also being aired about the technicians. A report was produced by a working group that saw medical problems, the ageing of know-how and decreasing motivation as the possible bottlenecks for the functioning of people in 'vakgroep' 35 or lower (glass technicians, electro-technicians, maintenance technicians, depot workers and mechanical technicians).¹²⁵ It was recommended that changes should be made in the working environment, to invoke more commitment by offering further schooling and mobility, by increasing responsibility and by increasing the intake of students drawn from secondary vocational education.

In Part II we saw how the hierarchical structure in the laboratory grew in the 1946-1972 period. In that period the appraisal and assignment of a salary scale ('vakgroep') were still rather 'hidden' processes. The emerging democratisation was to change all of this, though. People spoke more openly about their scales, and salaries were no longer confidential. For assessment a method was established that was known as the 'n+2' rule. This meant that each employee was assessed by someone who was two steps higher in the hierarchy (this meant that an assistant was assessed by his group leader, a research scientist was assessed by his director, and a group leader by his managing director). The 'n+2' rule made it necessary for the directors to visit 'their' scientists on a regular basis (normally twice a year) enabling them to remain well acquainted with what was going on in their groups. Furthermore, every two weeks there was a scientific management meeting, where each director discussed the publications, patents and internal reports that had been issued in his or her sector during the preceding two weeks. Valkenburg had meetings with each new scientist who had been in service for a year, and after four years of service a scientist would meet with the company's central Corporate Staff Bureau.¹²⁶

7.5 Increasing Co-operation with PDs

The changed task profile of the lab (see section 7.2) had an impact on the contacts with the PDs. In Part II we saw how during the 1946-1972 period the contacts with the PDs were used to try to transfer research output to the PDs. Most of this output was not presented on demand by the PDs, but was supplied of the lab's own accord. The PDs often felt that they had no real influence on the research programme. The contacts could be very

different with different topics and groups. In the 1972-1994 period, this changed. The contacts started being used to establish two-way communication, but it was not so easy to get rid of all the attitudes and habits that had developed over the course of about two decades. A certain effort had to be made to put the new relationship between the Nat.Lab. (and the foreign labs) and the PDs into practice. Subsequently, there were R-PD management meetings, Review meetings, Transfer Projects, and finally there was the introduction of contract research. A specific example of intensive practical co-operation, even before contract research was introduced, was to be found in the Geldrop project centre. This centre was an early example of the inclination towards mutual commitment.

The R-PD Management Meetings

Several people in managerial positions at the Nat.Lab. when Pannenberg took over Casimir's position on the Board of Management expressed the opinion when asked to characterise the changes that took place that it was Pannenberg, in close co-operation with E.F. de Haan, who had started to formalise the contacts between the Nat.Lab. and the PDs.¹²⁷ Until then, those contacts had been of an ad-hoc nature, and they had not been very intensive at the directors level. Others, in particular those who held managerial positions in the PDs, said that they did not find the transition from Casimir to Pannenberg a clear one. According to them, Pannenberg was just as keen to protect the research organisation's independence as Casimir had been.¹²⁸

In the 1970s Pannenberg set up special committees to establish contacts with individual PDs at the management level. These were clear signs of the growth towards mutual commitment. They were called the 'R-PD management committees' (R-PD standing for Research-Product Divisions). They were meant to serve the need to achieve agreement between Research and the PDs at an early stage of innovation and to obtain more adequate market information for research. The approach adopted to attain these goals would not be the same for all PDs because it was believed that the need for pre-development differed from one PD to another. In 1979, 34 R-PD management committees were established. There were different numbers of R-PD management committees for the different PDs.¹²⁹

The contacts with the PDs were assessed at several CRC meetings. Mixed feelings were expressed. The debates at the 1978 CRC meetings clearly illustrate that point. The agenda created for that meeting was, as has been mentioned before, structured according to PD groups.

First of all there was the Professional I group. When it came to the relationship with the PD Telecommunication and Defence Systems (TDS), the CRC members were of the opinion that it was difficult to determine

a central research programme because of the local character of most TDS activities. These activities were in the fields of telephony, optical communication, digital transmission, radar, infrared devices, microwaves, radio connections, facsimile coding and networks.¹³⁰ TDS was seen by research as a follower in the market that had problematic relationships with Elcoma. Research support for TDS was mainly provided by the Nat.Lab. and the lab in the UK (PRL). With the Philips Data Systems (PDS) PD research was primarily carried out in the Nat.Lab. and in Hamburg. The topics were speech recognition, printers and other peripheral equipment for computers, modelling and simulation, computer architecture and parallel processing, system design, and optical recording. In the case of the Nederlandse Kabel Fabrieken (Dutch Cable Factories, NKF) the Nat.Lab. and the Brussels lab (MBLE) worked on cable modelling.

The Professional II group consisted of the PDs Medical Systems and Science & Industry (S&I). The first-mentioned PD had expressed in its contacts with Research that there should be better contacts between Research and the market (not only medical top experts but also hospitals). The 'hot areas' mentioned were CT scanners, ultrasonics, nuclear medicine, and medical electronics, and research support was given mainly by the Nat.Lab., the LEP in France and the Hamburg lab. Later, in 1982, the discussion concerning the contacts end, and the conclusion was that long-term research remained problematic because the links between research and the PD's activities were too strong. In its contacts with Research, the PD had remarked that Research should take into account geographical differences when dividing the research tasks between the labs. According to Van Geuns¹³¹ the Research organisation retained much in the field of medical equipment in the Hamburg lab, while the PD had its main facilities in the village of Best, near Eindhoven. The geographical distance was a barrier when it came to the exchanging of people, and in general, it hampered contact between Philips Research and the PD on agreements as to what should be researched. In a similar way, the field of ultrasound medical equipment caused problems because Research had concentrated these activities in the LEP in France, while all the PD's industrial ultrasound activities were in the USA. In addition, the PD felt that the Research organisation had insufficient know-how in the bio-related sciences. For the Science & Industry (S&I) PD work was done in the fields of analytical measurement equipment and industrial welding equipment. Pannenburg was of the opinion that S&I should be more critical in their selection of the research output to be developed.

The relationship with Elcoma had been problematic in the 1946-1972 period (see Chapter 5). This did not change after the introduction of the R-PD management committees. During CRCs some lab managers complained about the 'not-invented-here' syndrome experienced in their contacts with this PD. The transfer of research output to this PD was also

hampered by a lack of qualified personnel within the PD, by the existence of separate developments groups that did not co-operate very willingly, and by the excessively high short-term activities undertaken by Research but which in fact belonged to PD work. On the other hand, there was the criticism that Research was not well enough acquainted with market requirements and that it was often unable to estimate costs for new products. So not much had yet changed when compared with the period described in Part II.

The Consumer I group comprised the Audio, Video and Electro-Acoustics (ELA) PDs. With Audio and Video, bi-annual contact at the management level had been established. Here, too, we find the CRC participants complaining about a lack of intake capacity for research output and a lack of market information. Research support was provided in the fields of recording, signal processing, displays, IC Large Scale Integration (LSI), software, man-machine interaction and memories. With respect to the ELA PD new market opportunities were seen in microelectronics and digital systems applications, but from the Research side there was doubt as to whether the PDs in this group were eager enough to innovate.

The products of the Consumer II group (Light, SDA, MDA) were seen as being 'classical' (mainly related to classical physics), and here Research conceived that its role was offensive rather than defensive. Here too we find that bi-annual management meetings were seen as a way of staying in contact with the PDs. In the case of the Lighting PD, it was suggested that the Central Lighting Lab sometimes did the same work as that done in Research. For some reason the PDs development departments seemed to be more willing to accept research output from this Lab than from the Nat.Lab.

The mutual feelings were that there was a gap between research and development that seemed too difficult to bridge, and the R-PD management committees had not succeeded in dissipating those feelings. The bi-annual scheme for the R-PD meetings established for several PDs was obviously not frequent enough in the eyes of the Philips Board of Management, because in 1980 Pannenburg announced that the Board wanted a more intensive discussion with the PDs on the research programme.¹³² Although it is difficult to draw any firm conclusions on this, the number of negative remarks about relationships with the PDs, recorded in the CRC minutes of the 1980s, did decrease noticeably. This may have been a sign that gradually an understanding of the position of the PDs was growing within the research organisation. It was particularly during the 1980 CRC that Pannenburg emphasised that the company was in serious trouble and that an arrogant attitude towards the PDs was not appropriate in this situation. Instead, a better relationship between the research organisation and the PDs was to be the goal of both Research and the PDs in

order to improve the company's position. Pannenberg recommended the following: maximise co-operation with the PDs in the case of market pull developments, do not get lost in new adventures but capitalise instead on existing projects, reduce the total budget, reduce efforts in fields where PDs are reducing effort, push up the development capacity of the PDs, speed up transfer of output, leave social activities to personnel departments and concentrate on research work. In this list we can recognise several of the problems that have been presented before as complaints from Research about PDs, but now Pannenberg was clearly trying to advocate a shared responsibility instead of having Research and the PDs making reproaches.

The Review Meetings

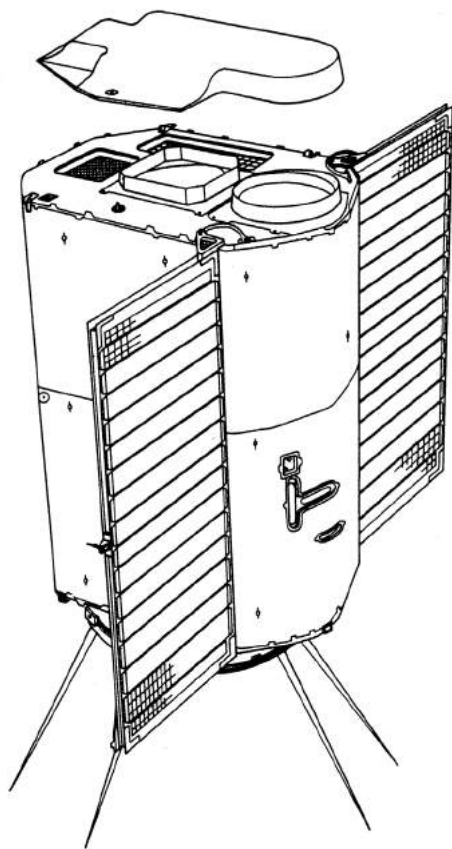
In 1983, when Van Houten had succeeded Pannenberg on the Board of Management, another series of structural meetings was initiated to discuss the ways in which the research programme could be oriented towards the company's needs. In Review Meetings, the Philips Board of Management met annually with the managing directors of the Nat.Lab. and the foreign labs. The first of these meetings served as a kind of introduction and was designed to provide insight into the research organisation's tasks and ways of operating. During the second meeting, in 1984, a more strategic discussion was held with the Board of Management about the Corporate Research Programme. The strategic fields identified were IC design and IC technology, consumer electronics (television in particular), information technology, and product automation. In the 1987 Review Book the following key technologies were identified: IC technology, IC design and tools, system knowledge, software, recording, and displays. These areas are not very different from the ones identified in 1984. The argumentation for selecting these fields was that they were 'of course, central to Philips' current and future business'.

In the Review Meetings all the important developments taking place in the Research organisation were discussed with the Board of Management. Therefore, the agenda of these meetings can be seen as an outline of what is to follow in this section. In 1988 the concept of the Transfer projects was discussed. In 1989, the year in which contract research was introduced, the first experiences gained from negotiating with the PDs were presented. In 1990 a substantial cost reduction plan was discussed. Finally, we find in 1991 a new approach to defining the research programme in terms of capabilities and application areas, and at this time the Self-Financing Activities (SFAs) also emerged. All these steps on the road to mutual commitment will be described here, but first we will examine an early example of narrow co-operation with PDs, namely the Geldrop project centre.

The Geldrop Project Centre

The Geldrop project centre had its roots in the Van Dorsten group (electron microscopy) that had moved from the Strijp campus to a location in the village of Geldrop, which is situated not far from Eindhoven and Waalre.¹³³ In 1963 the premises were extended, and new research fields, like the cyclotrons, neutron tubes and clystrons areas, were introduced in Geldrop. In particular, it was the work on the cyclotrons that was to provide an early example of project work in close co-operation with a PD (in this case that was PIT).¹³⁴ In 1967 an initiative was taken to set up a project that was to become the one for which the Geldrop project centre would become well known, namely the ANS project. ANS, the *Astronomische Nederlandse Satelliet* (the Astronomic Dutch Satellite), was a satellite that was developed by a consortium of Fokker-VFW and Philips, founded in 1968.¹³⁵ The practical work started in 1970. Philips' role (it was particularly the Nat.Lab. that delivered the know-how for this) was to develop the instrumentation for this satellite. The work was being carried out for the European space organisations ESRO and ELDO, and it provides an example of the growing European co-operation in the 1972-1994 period. Space technology was not particularly a market in which Philips had been very involved before that, but in the 1946-1972 period, as we have already seen in other examples of this, it was Casimir's philosophy of being involved in a scientific field purely by developing instrumentation for it (e.g. instrumentation for nuclear physics). According to Valster, who was in charge of the Project centre in the years 1970-1975, Tromp, a member of the Philips Board of Management, was a sort of patron of this space project.¹³⁶ The ANS was to contain equipment for astronomical measurements. The project was typical of the Geldrop annex,¹³⁷ in that it was a joint project between the Nat.Lab. and the PDs. In this case the Nat.Lab. co-operated with PTI (the Philips Telecommunications Industry) and HSA (Hollandse Signaal Apparaten), and occasionally people from the PIT PD were also involved. The Nat.Lab.'s role was to develop the instrumentation for positioning the satellite (some mechanical work was also done on, for instance, reaction wheels for stabilising the satellite¹³⁸) and the board computer. The satellite was launched in 1974, thus marking the end of the project. Valster had found that the project-oriented way of working with deadlines and periodical external assessments was unusual for the Nat.Lab. After the project ended, many people were transferred to other parts of the company, back to PTI and HSA, but also to the Audio and PIT PDs.

The project-oriented way of working was repeated in later projects, all of which were more communication-oriented. An example of such a project was CARIN: a navigation and information system for cars. This system had emerged from a previous project, known as the O-bus, an idea for a sort of flexible public transport system, whereby people could hail a bus



The exterior of the satellite. The radiation to be measured reaches the measurement systems through the openings at the top. The solar panels can be seen on the left and the right in the deployed position. Between the solar panels there is an opening through which light can reach the solar sensors. The rods underneath are the aerials. The maximum total mass will be about 130 kg.

Figure 27. The exterior of the ANS (from *Philips Technical Review* Vol. 33, p. 119).

The solar panels of the satellite can be seen on the right and left sides. The rods underneath are aerials.

in the same way they might hail a taxi.¹³⁹ CARIN enabled car drivers to type in their starting point and destination, and the system would then provide a route and inform the driver during the journey making use of audio and visual means. This project was carried out in co-operation with the PD Audio's Car System unit.¹⁴⁰ The next step was SOCRATES, a European project, in which the routing was coupled to actual information about traffic situations, broadcast by radio transmitters. Another field in which Geldrop was active was optical communication. The DIVAC proj-

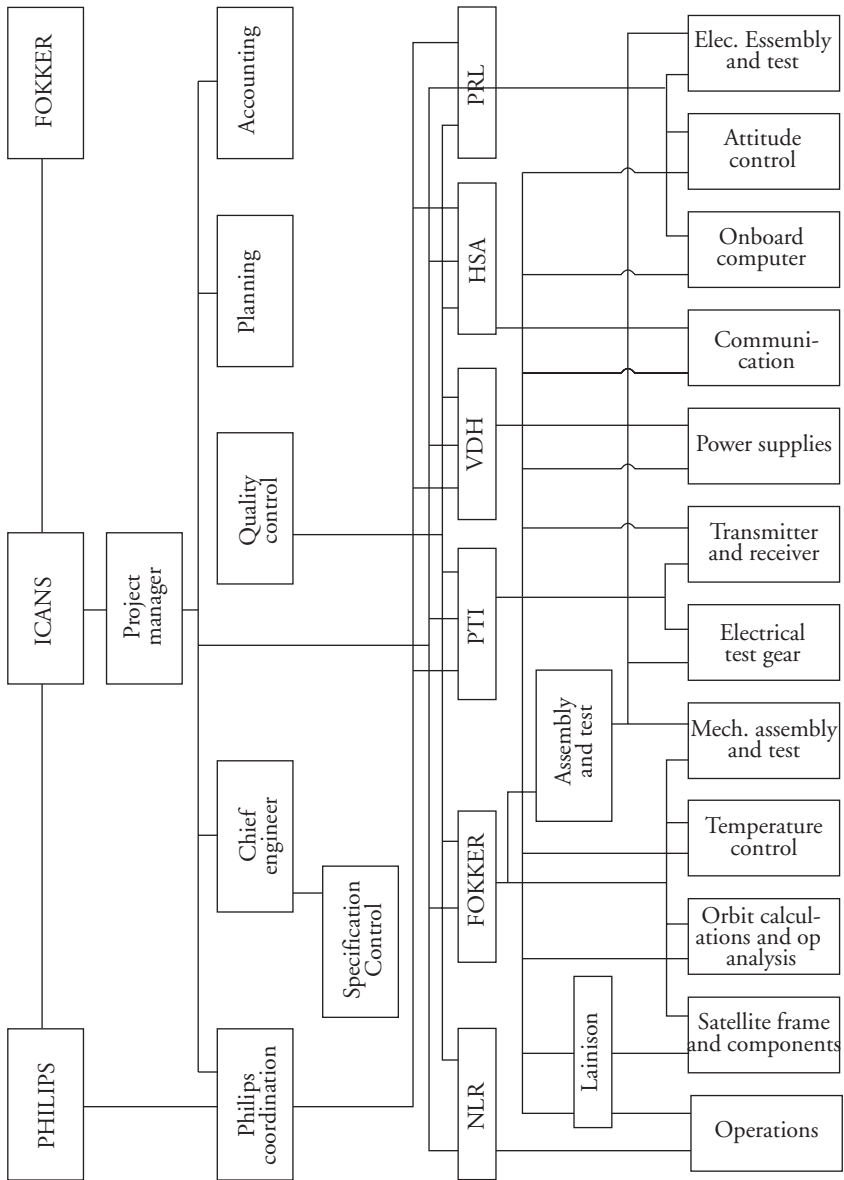


Figure 28. Organisation scheme for the groups that were involved in the ANS project through the ANS Industrial Consortium (ICANS).

PRL is the Philips Research Laboratories in Eindhoven (the Nat.Lab.), PTI is the PD Philips Telecommunication Industries in Hilversum, HSA is Hollandse Signaal Apparaten in Hengelo (that used to be a part of Philips), VDH is Van der Heem Electronics, Inc. in Voorburg (a private company), NLR is the Dutch Aerospace Laboratory (a government-funded laboratory).

From *Philips Technical Review* Vol. 33, p. 128/9.

ect is a good example of work in that field. Here, too, the co-operation with PDs and external partners constituted an important aspect of the work: the Nat.Lab. worked with the PTI PD, Eindhoven University of Technology, and the Dr. Neher Lab at the Dutch PTT telecom company.¹⁴¹ At the same time a project like PHILAN (Philips Local Area Network), the creation of an optical fibres network for offices, was an exclusively Nat.Lab. activity.¹⁴² Finally, the field of software should be mentioned as a focus of the Geldrop project centre activities. In the Megadoc project, for example, software was developed which could be used to archive, search for, sort, distribute and multiply any relevant office documents in an electronic mode. The Digital Optical Recording (DOR) disc was used as a medium. The results were transferred to the computer PD in Apeldoorn, but as this PD had many problems with the selling of its products, it never became a success.¹⁴³ In addition, software played an important role in other projects as well (for example, the O-bus project was presented in the *Philips Technical Review* in a special issue on software).

The Transfer Projects

The Project centre in Geldrop was dissolved in 1990, and the activities were moved to the main Nat.Lab. premises near Waalre. By then, project work was no longer specific to Geldrop, because a new type of research activity, known as Transfer Projects, had been defined as part of the Corporate Research Programme. P. Kramer had designed this type of project in 1987, and the concept was accepted by the Board of Management in the 1987 Review Meeting.¹⁴⁴ The definition of Transfer Projects was as follows: 'Transfer Projects are sizeable research topics, whose technical feasibility has been proven, which are wanted by the PDs and where the results of the work in the form of components, devices, systems, tools or processes justify and require a common approach to transfer knowledge, skills, hardware or software to the PD. A concise document is signed by both parties; this contains the agreed goal of the project and its key topics, specifications, the project leader, time schedules and milestones, resources (including the names of the people involved), initial marketing and business plans.'¹⁴⁵ Bulthuis remembered how P. Kramer used to say that if something was to be transferred to the outside world (the PDs), researchers tended to prevent that, arguing that they first wanted to find further improvements of some sort. In that context, Kramer saw Transfer Projects as a commitment designed to 'freeze' part of the research programme, to define it as being 'in state of transfer' and to allow that part to focus on the transfer to the PD rather than to seek further improvements.¹⁴⁶

In 1988, 72 Transfer Project proposals had been outlined, but during the January 1988 RDC it was stated that there were only a few projects for Lighting and for I&E, and that many of the Transfer Projects were rather small. Furthermore, in the case of many Transfer Projects a marketing and business plan was lacking.¹⁴⁷ In the June 1988 RDC meetings Van Houten stated that his target was to see that the research programme contained about 25% Transfer Projects, 25% explorative research activities and 50% activities of a disciplinary and service nature, aimed at preparing new Transfer Projects.¹⁴⁸ By then, a list had been circulated with the status of the various Transfer Projects of that time.¹⁴⁹ It is striking to see that the strong bias towards Elcoma-oriented research did not feature very prominently in the list of projects. Of the whole list only twelve projects were Elcoma-related. Only six times do we find Lighting mentioned as the PD involved, and only one time I&E. The status categories in the list make it clear that most projects had already started even before the agreement was formalised.

The Mega Project

A major project that was a shared activity between research and the Elcoma PD was the Mega project, already referred to in section 7.2. The emergence and finally the collapse of this project are certainly some of the most dramatic events marking the 1972-1994 period of the history of the Nat.Lab. According to the project leader, R.P. Kramer, the relationships between research and the PD were problematic during this project.¹⁵⁰ This was partly due to internal tensions within the PD:¹⁵¹ the top-level managers did not want the project, but other people managing the PD's IC-activities, supported by the Board of Management, did have an interest in the project, and this situation made it difficult for research to discuss the project with the PD. Kramer felt that there was insufficient support from the PD, and when problems arose, the Nat.Lab. was easily blamed for them. The PD had not defined what applications the project should be aimed at, and there were no clear production targets. Both Kramer and Bulthuis afterwards regretted that they had not intervened more strongly to rectify this omission.¹⁵² On the other hand, Kramer maintained that part of the problem was on the research side, because the research culture within the Nat.Lab. was not optimal for carrying out such an industrial project. As Kramer was on the directorate of both the Nat.Lab. and the PD, he felt himself to be in a difficult position. The Mega project showed that shared Research-PD projects could be very problematic when no firm agreement existed between Research and the PD about the conditions agreed to. In fact, this was the same problem that was also seen with a number of the Transfer Projects. Getting rid of the unclear conditions surrounding the projects was one of the aims of the next step in bending the

research programme towards the needs of the PDs, namely by introducing contract research.

Negotiations for Contract Research

After the announcement of the introduction of the contract research, in 1989, negotiations with PDs commenced, and the outcome of that process was discussed at the November 1989 Review Meeting with the Board of Management. It appeared that the research programme for 1990 would to a large extent be a continuation of the existing research programmes,¹⁵³ which according to the research representatives was not surprising because it came as the 'result of years of continued discussion with and listening to the PDs combined with our own insights into the technical world and the operation of Philips as a whole'. This was probably a reference to the R-PD management committees, among other contacts. When it came to drawing up the contracts with the PDs, a new contact means was established: the R-PD co-ordinators in the research organisation and their counterparts in the PDs, the Chief Technology Officers (CTOs), were brought together. Both were to function at the director's level and be responsible for determining the research fields and contract topics. Several of those CTOs were scientists who, in the past, had been transferred from the Nat.Lab. to a PD.¹⁵⁴ At the same time Van Houten stimulated people to come from PDs and take up management positions in Research.¹⁵⁵ The advantage of CTOs originally coming from Research was that they had a good understanding of the type of research that Research could offer to their PDs. In some cases the new CTO did not see his position as very being different from that in the past. Van Nieuwland (a CTO in the Consumer Electronics PD for the Audio Business Group) said that even before the CTO function had been formalised, he had already influenced the research programme in a concrete way through M. Carasso, who, for him, served as a sort of R-PD co-ordinator *avant-la-lettre*. As far as he was concerned, introducing contract research meant a further formalisation of the Transfer Projects and a further improvement of the relationship between Research and his PD. R.P. Kramer also knew Research well because he had worked there, had become director and, via the Mega project, had moved to the Semiconductors PD. Later, he formally became a CTO in this PD (which was part of the former Elcoma PD, which had first been renamed Components in 1988 and which, in 1991, was split up into Semiconductors and Components). He too conveyed that a good relationship between Research and his PD had developed in the years after the introduction of contract research. According to Bulthuis, the Transfer Projects had also helped to create a co-operative atmosphere between Research and the PDs.¹⁵⁶

It soon became evident that the PDs had become very cost-conscious and were reluctant to spend more money on research than was absolutely necessary. In February 1988, even before contract research had been introduced, all PD's except one had indicated that they wanted to pay less for research (the exception being the Lighting PD; this is remarkable because in the past Lighting had seen its own central lab as being in competition with the Nat.Lab.¹⁵⁷). The outcome of conversations with the various PDs was not very promising: CE wanted to reduce research investments and so did TDS. DAP (Domestic Appliances and Personal care, formerly SDA) claimed that they should be exempted from having to contribute to exploratory research. Another problem was that in 1989 the Defence and Control Systems PD had been sold to Thomson, which meant that Philips Research had lost a contract partner. In the annual Review Meeting with the Philips Group Management Committee, this problem was discussed.¹⁵⁸ The problem that was identified was that PDs understandably cut on costs whenever profitability was problematic, while it was, in fact, research that could particularly bring improvement for those areas. In section 7.3 the consequences for the finances of Philips Research (the Economising Plan) were discussed.

As we saw, the 1972-1994 period was one in which the Nat.Lab. searched for a healthy relationship with its research work and with PDs' development work. Already in 1974, Pannenberg had, in a presentation for the Nat.Lab. group leaders, expressed his views about coupling research to development as follows: if we draw a diagram that represents the relationship between – horizontally – the strength of the relationship between research and development and – vertically – the effectiveness of research, a bell-shaped curve can be expected to emerge. If the coupling is too weak, then an ivory tower situation will develop, which can be very unfruitful, and if the coupling is too strong the difference between R and D becomes too small, and R loses the basis for its existence.¹⁵⁹ Later on, this curve would come to be known as the Pannenberg curve ('de kromme van Pannenberg').¹⁶⁰ In fact, the lab management's considerations about the task profile in the 1972-1994 period, presented above, can be seen as a struggle to find a proper place along this curve. At the beginning of this period, the coupling was rather loose. This was the legacy of the previous period under Casimir. Pannenberg had expressed a need to enhance this coupling. The fear displayed by a number of managers that the coupling would become too strong sometimes made it difficult to achieve the right balance. In the next chapter we will see some examples of research projects that illustrate how the coupling increased in the 1972-1994 period.

8. Examples of the Road towards Mutual Commitment

In Chapter 7 we saw how the role of the Nat.Lab. changed in the 1972-1994 period. It was the growing conviction that in the 1970s it was time to make the outcomes of 'fundamental' science fruitful for technological developments, together with the end of economic growth that prompted the Nat.Lab. to look for a more PD-oriented task profile. For that reason communications with the PDs were formalised, and practical co-operation such as that seen in the Geldrop Project centre and in the Transfer Projects emerged. These steps did not immediately result in sufficient mutual commitment, which was why the mechanism of contract research was introduced by the Board of Management. From then on the Nat.Lab. had to negotiate with the PDs to obtain two thirds of its research budget from contracts. The remaining one-third was still allocated directly by the Board to protect the long-term 'risky' research projects. Economic circumstances also changed the situation of transferring people to the PDs. Apart from transferring the knowledge that those people had, it also allowed the Nat.Lab. to take in new employees even under zero-growth conditions. Efficiency was increased by transforming the relationships between the Nat.Lab. and the labs abroad. Rather than having a central lab plus a number of rather independent satellite labs, the overall research programme was divided between the Nat.Lab. and the other labs. The name Philips Research indicated the cohesion of the Nat.Lab. and the labs abroad as one corporate organisation.

The culture and structure of the lab also changed under the new conditions. It was the process of democratisation that influenced the relationships between the various layers of the lab's structural hierarchy, making communication more open and more informal.

As in the previous parts, here too a number of case studies can serve to illustrate those changes. They have been selected to represent a number of the research topics that were important in the 1972-1994 period. In the case studies the difficulties of growing towards mutual commitment, an aspect that was so prominent in the previous chapter, seem to be virtually absent. On the one hand, that has to do with the selection of the case studies; they were chosen deliberately to illustrate the growing co-operation between Philips Research and the PDs. On the other hand, the case studies reveal more about the practical level of the research being done

rather than about the discussions continuing at management level, and that was particularly the level where difficulties surrounding the reaching of mutual commitment were experienced.

8.1 Optical Telecommunications¹

The first case study for the 1972-1994 period deals with a project in the field of optical telecommunications. The case was selected because it nicely illustrates the growing co-operation developing with the PDs. The project is an early example of larger co-operation projects, and it is not surprising to see that the Geldrop Project centre was involved. As we saw in Chapter 7, this centre co-operated in projects with PDs at an early stage during the 1972-1994 period, when efforts to achieve a situation of mutual commitment between the Nat.Lab. and the PDs were still in the R-PD management committee phase, and Transfer Projects and contract research were still way beyond the scope of both parties.

The project also illustrates the growing co-ordination between the Nat.Lab. and the foreign labs because the Aachen lab was also involved. Both the Nat.Lab. and the Aachen lab worked on alternative ways to produce glass fibres for optical communications. During the project their efforts were compared, and the most successful technology was chosen for implementation.

Finally, the project illustrates how the boundaries between materials research, devices research and systems research could be crossed when a complex study necessitated this, and the optical communication project with its very different components certainly was complex. Research into the properties and production of glass, research into lasers and avalanche photodiodes, and research into signal processing as a systems aspect of optical communications were all brought together in a single effort to realise three optical communication systems in Germany and in the Netherlands. In that respect, this case study has analogies with the VLP case study described in Chapter 6, where we also saw that the strength of the research organisation could lie in the bringing together of very different types of know-how. One important difference, though, is that the VLP research took place in the VLP lab within the Nat.Lab., where people from a PD were temporarily stationed. In the case of optical communications, we see activities both inside and outside the Nat.Lab. being combined to form one single effort.

In the end, most Philips optical telecommunication activities were sold to other companies and thus resulted in income for the company. These activities are still flourishing. In addition, the technologies that were developed for this professional application still remained important for the consumer applications that form the basis for Philips business today. This was another reason for selecting this particular case study.

The Beginning of the Nat.Lab. Involvement in Optical Communications

Both Philips and the Nat.Lab. had been involved in telecommunications for many years, not only in the cases of radio and television, but also in the field of two-way telecommunications (telephone). In the past Philips and the Nat.Lab. had worked on the telephone, both through wave transmitters and electrical cables. With the cables it had been the carrier wave technique that had been developed to enable more information to be transported through conventional cables. The invention of coaxial cables brought further improvements. Then came the option of digital instead of analogue transmission. A serious disadvantage attached to electrical signal processing was the high attenuation at high frequencies. To provide a signal/noise ratio that was high enough, an amplifier had to be inserted into the cable at every few kilometres. That made this technology expensive and failure-sensitive. In the 1960s serious effort were devoted to developing microwave telecommunications through hollow pipes. In principle, this technique yielded good results for amplifier distances of up to 20 kilometres, but there were all sorts of problems that gave rise to general doubt about its feasibility (problems surrounding the generating of microwaves, problems because the pipes had to be almost straight to enable good wave propagation). Laser technology, another crucial component of optical telecommunications, was developed in the 1960s. The Nat.Lab. was one of the industrial labs to work on lasers, and it soon became evident that lasers could be used for communication purposes. Also in the 1960s, Kao and Hockham at ITT in England proved the feasibility of optical fibre systems for optical communication by investigating the theoretical (and extrapolated) properties of glass and lasers.² In those glass fibres light was damped much less than electrical signals in cables. British Telecom had taken up this idea and had commissioned Corning Glassworks to develop the technology so that glass fibres with the required level of purity could be produced. During the September 29 – October 2, 1970, conference on ‘Trunk Telecommunications by Guide Waves’ in London, British Telecom and Corning presented the resultant glass fibre with just a 20 dB/km attenuation (this was the usual standard for the electrical communication systems).³ The practical feasibility of optical communication was then demonstrated. The potential advantages identified compared with electrical communication were: larger bandwidth, small sizes and low weight, low transmission losses, no crosstalk, and insensibility to electrical disturbance. The GaAlAs lasers had also been developed and had come to possess a sufficient lifespan, so this component also contributed to the feasibility of optical communication as well. These results inspired other companies to start working on optical communication. In 1971 the Nat.Lab decided to work on optical telecommunications.⁴

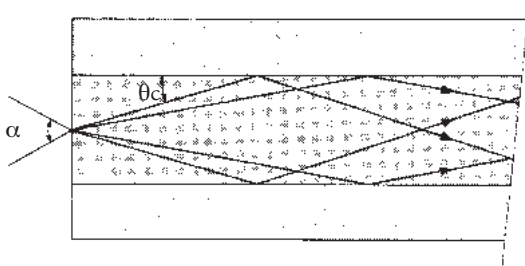
In the Nat.Lab. there had been a microwaves research group in the

1960s that had later been renamed the 'Wideband communication systems' group, at the same time that the department of Electronic Systems was established and put under the leadership of K. Teer. In the group, led by Gieles, Mouthaan and an assistant of his, H.P.M. Rijpert started doing experiments into optical communication. This research was seen as being relevant to the Telecommunication and Defence Systems (TDS) PD. At the 1970 CRC there had been a separate discussion on the research being done for this PD, and it had been stated that the increasing complexity of telecommunication systems (at that time optical communication was given as an option for this) meant there was a growing need for better co-ordination between research and development activities.⁵ In 1972 a first experimental set-up using a 20-metre glass fibre was realised.⁶ For this, the input of other groups was needed as well. The Stevels glass group (later led by F. Meijer) and two device groups, namely the Vlaardingebroek group (later led by G. Acket) and the De Nobel group (later led by H. Koelmans), became involved. The glass group in the Aachen lab, led by H. Lydtin, also became involved.

The first tests with glass fibres were done with pulsed GaAs lasers that had been bought from RCA. This type of laser had been available since 1962.⁷ In 1967 continuous GaAs lasers had been invented, which were more suitable for optical communication than the pulsed lasers. GaAs lasers were more suitable than HeNe lasers because their wavelength (0.85 micrometer instead of 0.63 micrometer for the HeNe lasers) gave a lower attenuation in the fibres and less dispersion.⁸ Originally, these lasers were pulsed because they demanded such a high current that the heat production made it impossible for them to work continuously. The first continuous GaAs lasers had to be cooled with liquid nitrogen. Substantial work on such lasers was done at the Ioff Institute in Leningrad, led by Zhores Alferov.⁹

In the Nat.Lab. the Vlaadingerbroek/Acket and De Nobel groups worked on lasers and photodiodes. In 1974 the groups succeeded in demonstrating an uncooled GaAs laser.¹⁰ At that time the length of the glass fibre was limited to about 20 metres because the quality of the fibre did not allow greater lengths to be used. For the detection of the signal, avalanche photodiodes were developed.¹¹ Work on avalanche photodiodes had originally been conducted for microwave research, but technically it was not difficult to transfer the outcomes to the optical communication field.

For the glass fibres, the Nat.Lab. group used the double crucible technique, while the Aachen group used the Chemical Vapour Deposition (CVD) technique. With the double crucible technique two concentric crucibles with liquid glass of different types were used from which fibres were drawn. The different types of glass resulted in differences in the



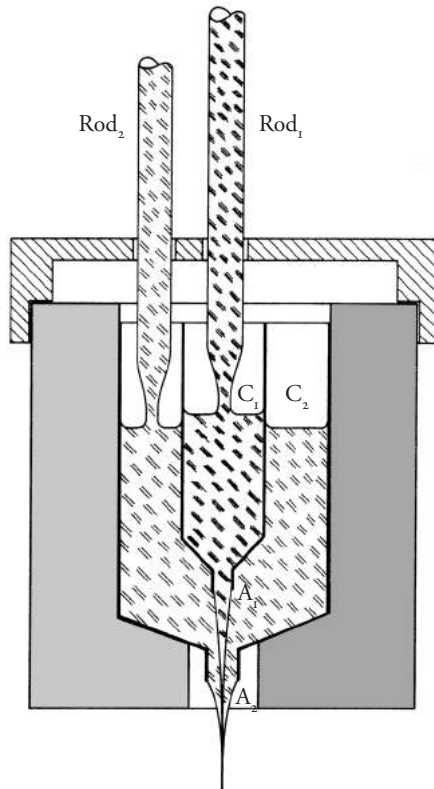
Wave guidance by total internal reflection in a glass fibre. The maximum angle of incidence on the interface between the core and the cladding is θ_c . The magnitude of this critical angle is determined by the ratio of the refractive indices of the core and the cladding. The critical angle in turn determines the apex angle α of the acceptance of the glass fibre.

Figure 29. Optical waves in an optical fibre (from *Philips Technical Review* Vol. 36, p. 179).

The wave is totally reflected when it moves from core to cladding, and the angle of incidence is smaller than the indicated critical angle. In this case the transition from core to cladding is not continuous. In other types of optical fibres, the transition is continuous, and the wave reflection is a smooth curve.

refractive index. The inside glass was known as the ‘core’ and the outside glass as the ‘cladding’ of the fibre.

Ion exchange at the boundary created a graded index profile. The objective was to make this grading parabolic because then the different light signal modes would have the same speed, which would prevent the modes from getting separated during transmission through the fibre. Multimode fibres could be given a larger diameter, thus simplifying the coupling of the fibres. The CVD technique also allowed these ‘multimode’ fibres to be produced. With this technique the glass was damped on in layers. The CVD technique worked with quartz glass. Both techniques had their pros and cons. The double crucible method was the most promising for continuous production, which was of course attractive for the Glass PD’s glass factory. In addition, the method used soft glass, which was a familiar type of glass for the glass factory. The advantage of the CVD was that it yielded a better purity of glass which, from a functional point of view, was important (there was less attenuation in such cases). The groups in the Nat.Lab. and in Aachen had each taken one method so that both options could be developed and tested and a final selection could be made on the basis of the outcomes of the work of both groups.¹² In 1974 the Aachen lab came up with a variant of the CVD technique; the plasma-activated CVD technique (PCVD).¹³ With this technique the decomposing and deposit-

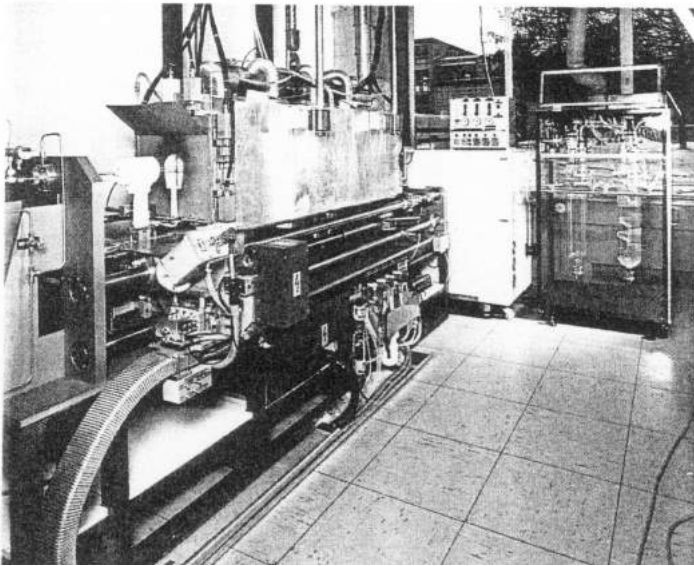
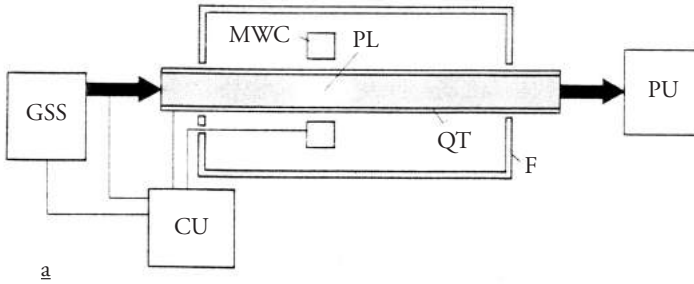


The double crucible for making glass fibre. C_1 and C_2 two concentric crucibles of pure platinum. Rod_1 and Rod_2 rods made in the apparatus shown in fig. 1; Rod_1 consists of alkali-germanosilicate glass with a high Na⁺-ion content, and hence a high refractive index, Rod_2 of the same glass but with a high K⁺-ion content, and a low refractive index. A_1 outlet of the inner crucible C_1 , A_2 outlet of the outer crucible C_2 . In the region between A_1 and A_2 there is an exchange between the Na⁺ and K⁺-ions, which gives a smooth variation for the refractive-index profile in the resulting fibre. F resistance furnace.

Figure 30. The production of glass fibres using the double crucible method (from *Philips Technical Review* Vol. 36, p. 184).

C_1 and C_2 are concentric crucibles filled with glass melted from two rods (Rod_1 with a high refractive index and Rod_2 of a lower refractive index). The glass from C_1 becomes the core; the glass from rod 2 becomes the cladding. F is the furnace.

ing of the material were done by means of a plasma and not by using local heating as with the normal CVD processes. Originally, the method was developed for the depositing of very pure carbon, but the Aachen lab realised that the same technique could be used for glass. The technique allowed a better-controlled deposition of the glass layers to take place, thus resulting in lower absorption and dispersion within the fibre.¹⁴ The PCVD process resulted in a preform that could be turned into a fibre.



a) Diagram of the PCVD process. GSS gas-supply system. MWC microwave cavity. PL plasma. PU pump system. CU control unit. QT quartz-glass tube. F furnace.
 b) The laboratory arrangement. The cavity can be seen on the left of the furnace insulation; it is white and encloses the quartz-glass tube.

Figure 31. The PCVD process (from *Philips Technical Review* Vol. 44, p. 245).

The optical telecommunication research groups actively took part in the international conferences in this field. Valster was on the steering committee of the Integrated Optics & Optical Communication (IOOC) conferences. He was also chairman of the annual European Conference on Optical Communication (ECOC), while Acket and Van Heuven were on the IOOC programme committee.¹⁵

The Optical Telecommunication Projects: Three Test Systems

So far, the research done on the various components of the optical telecommunication systems in the Nat.Lab. and in the Aachen lab have been described. The next step was to bring these components together to develop complete optical communication systems. This was done by means of three related sub-projects that were carried out by the Geldrop Project Centre. One of these projects was the optical communication system for the city of Berlin in Germany. The way in which Philips got involved in that was through its previously established contacts with the German cable industry.

Before the advent of optical communication, producing cables for telecommunications had not, of course, been a matter that concerned the glass factory. Since 1949 Philips had, for that purpose, worked in co-operation with a German company, called Felten & Guillaume (F&G). In 1951, at the Orco meeting of April 10, emphasis had been placed on co-operating with cable firms to ensure that a combined development of the cables and the total communication systems took place.¹⁶ Philips saw the increased interest in cables as a chance for improving its market position in telecommunications.¹⁷ In 1969 Philips therefore became financially involved in the F&G company, and in 1970 it also took over the Nederlandse Kabel Fabrieken (NKF)¹⁸. Although Philips owned only 35% of F&G, it was given a key role in this company.¹⁹ This was important because at that time the national telecommunication organisations (the PTTs) always turned to their national industries for cables and telecommunication systems. That was one good reason for the TDS PD to work with nationally based companies. In the Netherlands, there was PTI (Philips Telecommunications Industries), in the UK the TMC (the Telephone Manufacturing Company), in France the TRT (Transmission Radio Electrique et Téléphonique), and in Germany TeKaDe (Telefon Kabel und Drahtwerke). The latter was (as of 1962) a daughter company of Philips and F&G.²⁰ Other companies involved in the telecommunications and cable market in Germany were AEG-Telefunken, Siemens and Standard Elektrik Lorenz (SEL). Although Philips also owned NKF, which was also a cable firm, there seemed to be no conflict with optical fibres. NFK had not yet expressed any interest in this field.²¹

Regarding the co-operation between Philips and F&G in the field of optical communication, the Philips Board of Management decided to initiate an 'Arbeitsgruppe' (working group) at the directors' level. The Nat.Lab., F&G, the Glass and TDS PDs, and the Corporate Legal Department were represented in this Working Group.²² The Working Group delivered a report in which it proposed that an 'in-house trial system' for optical communication should be developed. The purpose of developing this system was to prove the ability of the co-operating companies in the field of optical communication. The system developed was

then to be demonstrated for telecommunication organisations (PTTs) in various countries. The Nat.Lab.'s premises in Geldrop were proposed as a suitable place for creating the system. On the basis of the Working Group's report, a contract, or 'Vereinbarung', was formulated by Philips and F&G²³ and signed by the Philips Board of Management in November 1974. The project was the result of co-operation between the Nat.Lab., PD Glass, TDS, and F&G, with the option of involving Elcoma at a later stage to deliver components such as lasers. In the meantime, the Nat.Lab. set up a production line for small numbers of these components. The project was supervised by a steering group, the Optical Communication Co-ordination Committee (OCCC), which in fact more or less amounted to a continuation of the previous 'Arbeitsgruppe'. In June 1975 a task-setting group was put together to define the technical and financial specifications for the in-house pilot system. In the task group all the parties were once again represented: Mouthaan, De Nobel and Meijer (Nat.Lab.), Grotjohann (PTI), Kats (Glass), Weller and Krahn (F&G), Ballering (TeKaDe) and Gorissen (NKF). The group submitted its report in 1975, and the OCCC approved the contents after having consulted its directors (some of whom also consulted the Board of Management). In March 1976 the decision was taken to start the actual development of the system. The pilot system was planned to be a simulation of a real system which meant that the fibres would not be put into the ground, but would remain on the rolls. For the bit-rate, 140 Mbit/s was defined as a target. At that time the choice was a compromise between a high value that would constitute a real challenge, and a lower value that fitted in with the existing telephone network.²⁴ Later, 140 Mbit/s became the international standard. For the length of the cable, two sections of 4 kilometres each were originally chosen (with a repeater in between). Later, however, it became evident that the damping could be even further limited, so that two sections of 8 kilometres each became quite feasible. The cables were planned to consist of six parallel multimode glass fibres and some copper cables for feeding the repeaters. The cables were to be made in rolls each containing 1 kilometre of cable. The one-kilometre rolls had to be connected by means of welding connections and demountable connectors. Both at the transmitter end and the receiver end, a rack of equipment was needed to house the transmitter, a receiver, line-decoding equipment and equipment needed to check possible failures in the repeaters. In addition, equipment was needed to digitalise the television signal for 140 Mbit/s.²⁵

Meanwhile, in 1975, in Germany the Bundesministerium für Forschung und Technologie (BMFT; i.e. the State Ministry for Research and Technology) and the Bundespost (the German PTT) had decided to set up an optical communication pilot system in the city of Berlin. It was expected that four parties would be invited to submit proposals for the envisioned system: the Siemens company, AEG-Telefunken, SEL and

TeKaDe/F&G, and then the Bundesministerium would select the best proposal and draw up a contract. For some reason – according to Mouthaan²⁶ the argument was that TeKaDe had never submitted proposals before – TeKaDe/F&G was not invited to submit a proposal, but (again according to Mouthaan) F&G found out about this procedure and quickly contacted the Minister of Post und Fernmeldewesen, under whom the Bundespost fell, to ask him to extend the invitation to TeKaDe/F&G. By then only three weeks were left before the proposal had to be submitted. The minister agreed with the argument that even if TeKaDe/F&G had never submitted proposals in the past, it did, on this occasion, have the advantage of being connected to Philips through its lab in Aachen. Together with that lab, TeKaDe/F&G became a serious contender alongside the other companies which had already been approached. The Minister gave TeKaDe/F&G a month to formulate its proposal. This, together with the fortunate coincidence that the Task Setting Group was then working on the definition of the in-house pilot system, made it possible for the proposal to be submitted in time. The following features were established: 34 Mbit/s bit-rate, 4 km length, light produced by lasers. Another option was to propose a 2-kilometre length, in which case Light Emitting Diodes would have been sufficient as light sources, but this option was rejected.²⁷ For the Berlin project a separate Task Setting Group was formed, consisting of Ballering and Thielmann (TeKaDe), Mouthaan and De Nobel (Nat.Lab.), Lydtin (PFA lab in Aachen), Kats (Glass), Grotjohann (PTI) and Weller and Krahn (F&G). This group also fell under the auspices of the OCCC.²⁸ The effort to get the project funded was successful, and it was agreed that the system would be tested in April 1978 and delivered to the city of Berlin in July 1978. This extra activity affected the planning of the in-house project, but in the end, both projects were realised almost on time (the Berlin project was given priority and delivered according to the agreements so that the resulting delay for the in-house project was limited to a couple of months).

The Geldrop project centre was chosen as the location for the realisation of the in-house optical communication system. This was because in the Geldrop project centre the Nat.Lab. people were more used to doing projects than the Nat.Lab. scientists in Waalre.²⁹ Valster had been in charge of the Geldrop project centre and had later taken over Mooijweer's position as Director of the Telecommunications sector. He had also become a member of the OCCC, and had given a positive advice on the feasibility of the project to be carried out in Geldrop. In March 1976 the project began.³⁰ A Project Co-ordination Committee (PCC) for both the in-house and the Berlin projects was initiated by the OCCC, and the participants were Mouthaan (Nat.Lab.), Kats (Glass), Schadé (PTI), Krahn (F&G) and Thielmann (TeKaDe). Meijer, Lydtin and De Nobel were

added as ad-hoc members. Mouthaan served as chairman, Schadé was the secretary for the in-house project and Thielmann for the Berlin project. Mouthaan saw the co-operation as positive, although he also recalled tensions between different parties within the PCC. For example, when a cable broke, questions arose of whether or not Glass was to blame because the cable had been too brittle, or F&G because the cable had been treated too roughly.³¹ Another source of tensions was the fact that Van Avoort, who was then in charge of the Geldrop project centre, was not supposed to influence the course of the project, but at the same time he was sometimes under pressure to give priority to the project because of its strict deadlines. Mouthaan felt that TeKaDe had sometimes had problems because its role was only a minor one. Mouthaan had to manage the project, which was not an easy task because of all these tensions.³²

All the parties contributed in different ways. The Nat.Lab. delivered the photodiodes and the lasers, PTI delivered receiver equipment and measurement equipment, Glass took care of the glass fibres and F&G processed them into glass fibre cables and welded the cable sections together.³³ The Glass PD and F&G did not take part in the project team that had been established in Geldrop (consisting of people from the Nat.Lab., PTI, TeKaDe and TRT), but instead they served as suppliers for the system. In 1976 a 140 Mbit/s experimental set-up was realised. In 1977 the system was produced with full 8-km cables. In that year the Dutch PTT also started co-operating with PTI and NKF to pave the way for a field test in the Netherlands. TeKaDe was to take responsibility for delivering the Berlin system to the German PTT. The German normalisation rules required separate equipment for the Berlin system.

As has been seen, the Nat.Lab. and the Aachen lab worked simultaneously on two different options for the manufacturing of the glass fibres. This effort, as well as the other glass research activities, was co-ordinated by what was known as the 'KLM³⁴ group', in which Kats (PD Glass), Lydtin (Aachen lab) and Meijer (Nat.Lab.) took part. The Aachen lab continued to work on the CVD process using quartz glass, and the Nat.Lab. worked on the double-crucible method using soft glass. Some people were sent from the Glass development centre (Dutch abbr. GOC) to the Nat.Lab. to co-operate with Meijer's soft-glass people. In the end, the CVD process appeared to yield better results in terms of glass fibre attenuation. The CVD process could result in 3-6 dB per km for 0.85-micrometer wavelengths, while the soft glass could not do better than 20 dB per km. This was because of impurities in the material from which the glass was made. The difference became even greater when lasers with higher wavelengths were used (up to 1.5 micrometer). The co-operation of the Kats's predevelopment group then shifted from the Nat.Lab. to the PFA lab in Aachen (Lydtin).³⁵ In other laboratories in the world, too, experiments were being carried out using both glass-melting techniques. The

CVD technique turned out to be successful, and the other methods were abandoned for three main reasons: (1) the CVD technique yielded very low absorption fibres, close to the theoretical limit, (2) single mode fibres could be used since coupling techniques were so well developed that multimode did not provide important advantages, and (3) the preforms could be made so large that in practice the preform-drawing process became continuous; more than a hundred kilometres of fibre could be drawn from one preform; this overcame the disadvantage of the batch process approach.

As has been mentioned, the original plan was that the Nat.Lab. should deliver the lasers, because only small numbers were required, but in 1977 Elcoma became more interested in lasers. The PD expected potential industrial activity because of the VLP, the optical communication systems and other applications, such as Direct Read After Write (DRAW).³⁶ Elcoma sent people to the pilot production facilities in the Nat.Lab, in which the De Nobel and Acket groups participated. The TDS PD, Elcoma and the Nat.Lab. set up a tripartite organisational structure: there were Tripartite Management meetings, there was a Tripartite Steering Committee, and there were Tripartite working groups (e.g. one for optical components).³⁷ In 1978 a Business Co-ordination Committee for Optical Fibre Communication was also initiated in which NKF, Glass, TDS and Elcoma participated.³⁸ This committee dealt specifically with marketing and project execution within Philips in the field of glass fibre optical communication. From the above points, we can see how the complexity of the optical communication field led to various co-operation and co-ordination structures, in which the Nat.Lab. and the PDs participated. The Nat.Lab.'s activities in the field of optical communication projects were thus directly related to the PDs.

The Berlin system was completed and delivered in November 1978. F&G and TeKaDe installed this system under the streets of Berlin. The cables had to be drawn through underground cylinders (ducts). The cables were tested in the streets using measurement equipment that had been installed in cars. In March 1979 the in-house system was finished as well. For that system Hitachi lasers were used because those lasers were better quality than the ones that had been produced for the Berlin system by Elcoma.³⁹ PTTs in various countries were invited to go and look at demonstrations. The Dutch PTT decided to install a system that would connect Eindhoven with the nearby city of Helmond. A working group was set up in which the Dutch PTT, Philips' PTI and NKF participated.⁴⁰ Until then NKF had been hesitant about using glass fibre cables, but now they had become interested in this field. Probably, it was the success of the Berlin and the in-house system that had contributed to that change of attitude.

Between Eindhoven and Helmond there were no underground cylinders such as those in Berlin, and so the cables had to be inserted into the ground directly over a distance of 14 km. As had been the case with the previous systems, the Nat.Lab. again delivered the photodiodes. In 1980 the system was finished and put into operation.⁴¹ As with the two previous systems, this system was developed under the auspices of the OCCC.

This case study illustrates how the relationships between the Nat.Lab. and the PDs started to change in the 1970s. What we should take into account is the fact that the Geldrop project centre was exceptional in that it had projects continuing with PDs at a time when such projects in general were certainly not common practice in the rest of the Nat.Lab. Perhaps Teer was right when he remarked that systems research, by its very nature, could not be done as autonomously as devices and materials research.⁴²

The optical communication project had some characteristics that were to become common practice in the rest of the Nat.Lab. later in the 1972-1994 period. In the first place, there was an exchange of personnel between the Nat.Lab. and the PDs. The PDs stationed their people in the Geldrop project centre;⁴³ later, these same people returned to their PD with new knowledge and experience. From Geldrop, Nat.Lab. scientists were transferred to PDs, and they took with them their know-how on optical communication technology. Mouthaan, for example, moved to PTI in 1978. A number of people who worked on lasers in the De Nobel group moved to Elcoma.

Secondly, in terms of finance there was shared responsibility in the projects that have been described. Each of the parties paid for its own people who took part of the project.⁴⁴ The project took place long before contract research was introduced, and project financing was not simply a matter of a contract between the Nat.Lab. and the PDs. The fact that both the Nat.Lab. and the PDs invested in the project created a commitment for all parties.

A third feature of the project was the contribution of the Nat.Lab. and the Aachen lab to building up a patent position in the field. The Aachen lab obtained a patent for the Plasma-activated Chemical Vapour Deposition process. This patent later served as a basic patent for other patents in the field of glass fibre production. The most important names behind the patents were: Lydtin, Küppers and Geittner. Most patents were obtained by the Nat.Lab.: the De Nobel and Acket groups obtained patents on lasers for optical communication (some of the names that are mentioned in a Philips Corporate Patents & Trademarks list are: Acket himself, Nijman, De Waard and Tjiburg), and the Wideband Communication group also obtained several relevant patents. Khoe obtained several patents on laser-fibre couplings and fibre connectors. The patents on soft glass soon became obsolete when quartz glass became the preferred material.

Together these characteristics constituted a potent formula. In the early days of optical telecommunications, experimental lasers were used that had to be cooled with liquid nitrogen. The bit rate was no more than 10 Mb/s. Glass fibre could not really be produced in quantities, but within ten years the joint effort had led to the delivery of three complete systems (in-house, Berlin and Eindhoven-Helmond). Nearly all the aspects of the project were continued by the PDs, except for the construction of lasers, since it was Elcoma's belief that the market was insufficient. Laser development was discontinued in the Nat.Lab., which resulted in 1991 in a Self-Financing Activity and the initiation of Philips Opto-electronics. In later years PTI installed optical telecommunication systems in the Netherlands, Germany, Denmark and Saudi Arabia.⁴⁵ In the Geldrop project centre the project was continued in the form of the DIVAC project and the PHILAN project (see Chapter 7). The continued work on optical fibres in the Glass Development Centre, resulted in a factory called the Philips Optical Fibre plant.

In the 1990s most of Philips' industrial activities in optical telecommunications were sold to other companies (former PTI activities went to AT&T, the Philips Optical Fibre factory was taken over by Draka Holding and Philips Opto-electronics was sold at a good profit to Uniphase Netherlands, Inc.). NKF is no longer part of Philips today either. The field of optical telecommunications was no longer seen as 'core business' at the time when the company had to slim down for economic reasons.⁴⁶ At the time of writing this text, it had just been made known that Draka was about to double its capacity for the making of optical fibres. This illustrates that even today the outcome of the research programme had some successful business spin-offs, even though in this case they have been outside Philips. Another example is the extension of the Uniphase activities in laser systems, which were also recently announced (an increase in the number of employees from 350 to 650).

Thus, the optical communications field gives a good example of the advantage of close co-operation between the Nat.Lab., the research labs abroad (in this case: Aachen) and the PDs.

8.2 Integrated Digital Audio Converters

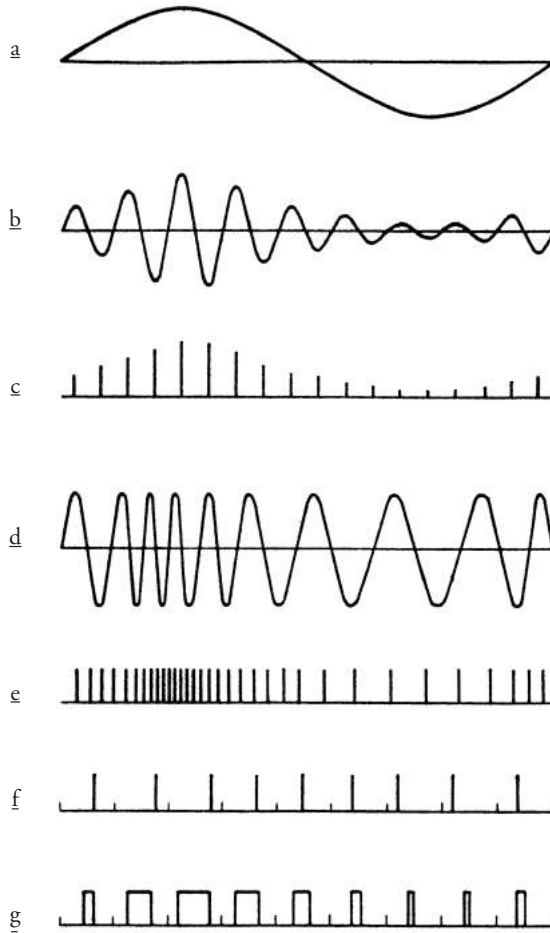
In a way, this case study is the counterpart to the LOCOS case study. Both were centred in the area of ICs. LOCOS is an IC-technology and the A/D-D/A converter development is an example of IC-design work. Together, IC-technology and IC-design constituted an important part of the Nat.Lab.'s research programme.

The case study selected here relates to one of the best-known Philips products, the compact disc (CD). As we already saw in Chapter 6, the CD is a follow-up to the Video Long Play (VLP) innovation from a technical

point of view. In fact, the CD started as an audio variant of the VLP. It was therefore originally known as the ALP. However, when it came to the development of the VLP and the CD, the role fulfilled by the Nat.Lab. was quite different in each case. The two case studies illustrate the changes that took place in the 1972-1994 period. The VLP was a product that was conceived in the Nat.Lab., developed there and afterwards transferred to the PD. The CD was an idea that emerged in a PD, and its development was also led by a PD, but it was only thanks to the know-how support from the Nat.Lab. that it was possible to develop the CD. Thus, in the case of the VLP, the Nat.Lab. tried to serve as an innovation source, while in the case of the CD, the Nat.Lab. served as a source of know-how. Of course, it would be naive to argue that the failure of the VLP and the success of the CD prove that being a source of know-how is a more apt role for the Nat.Lab. than being an innovation centre. In the 1946-1972 period we saw at least two examples in which the Nat.Lab. very successfully fulfilled the role of innovation source, namely with the Plumbicon and LOCOS. During those years, however, the circumstances were different. There was ample opportunity to bring out new products and technologies, the company could cope with many failed efforts, and there were certainly some very costly failures in those days, the Stirling engine (see Chapter 6) being a striking example of this. In the 1970s and 1980s costs had to be monitored more carefully, and big ventures could no longer be undertaken without the assurance of a reasonable chance of success. The story of the D/A converters as a Nat.Lab. contribution to the CD shows how the Nat.Lab. fulfilled a crucial role in this changed product development strategy, in which the PD played a leading role.

The Emergence of Digitalisation and the Need for A/D and D/A Converters

Digitalisation started in two areas, namely in telecommunications and control technology. The idea to express the amplitude of a signal in binary digits was launched in 1938 by a certain Alec Reeves, who worked at the Laboratoire Matériel Téléphonique in Paris.⁴⁷ In the late 1940s the use of digitised signals in telecommunications had the advantage that noise could be reduced much better than with analogue signals. The two new modulation techniques developed to deal with digitised signals were Pulse Code Modulation (PCM) and Delta Modulation (DM). In the 1940s PCM was standardised by the Comité Consultatif International de Télégraphie et Téléphonie (the CCITT standard for telephony),⁴⁸ but the first digital telephone line did not come into service before 1962 because digital systems remained expensive for a long time. Delta Modulation was a variant of Pulse Code Modulation, which made use of feedback.⁴⁹ Interest in control technology digitalisation grew, so that computer control

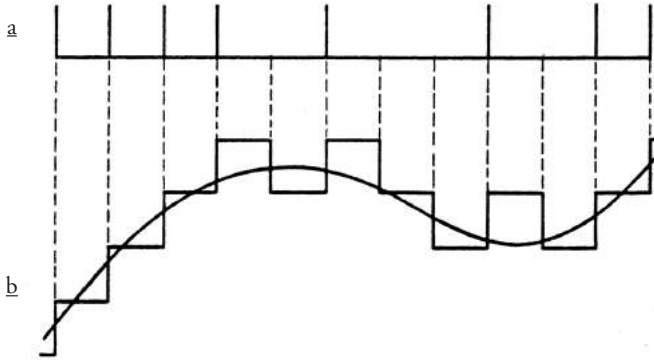


Diagrammatic representation of various non-quantized modulation systems.

- | | |
|----------------------------------|------------------------------------|
| a) The modulation signal. | f) Pulse-position modulation |
| b) Carrier amplitude modulation. | (note the marked time intervals |
| c) Pulse-amplitude modulation. | within which each pulse can move). |
| d) Carrier frequency modulation. | g) Pulse-width modulation. |
| e) Pulse-frequency modulation, | |

Figure 32. Modulation principles (from *Philips Technical Review* Vol. 13, p. 238).

Figure a is the modulating signal, e.g. a radio signal. Figure b represents the principle of Amplitude Modulation (AM): the amplitude of the carrier wave changes according to the amplitude of the signal. In figure c the carrier wave is replaced by a pulse: pulse-amplitude modulation. In figure d the frequency of the carrier wave changes according to the amplitude of the signal: Frequency Modulation (FM). In figure e the frequency of the pulse changes according to the amplitude of the signal: pulse-frequency modulation. In figure f the pulse position changes within a marked time interval according to the amplitude of the signal: pulse-position modulation. In figure g the width of the pulse changes according to the amplitude of the signal: pulse-width modulation.



Principle of delta modulation. The series of pulses emitted by the transmitter (a) is generated after comparison of the modulating signal (the continuous curve in b) with a step-like approximating signal with equidistant steps built up in the transmitter. When the approximating signal is smaller than the modulating signal a pulse is emitted, but if it is larger the pulse is suppressed. At the receiving end the approximating signal is formed from the series of pulses by integration and from that signal the modulating signal is reconstructed by smoothing out the steps.

Figure 33. The principle of Delta modulation (from *Philips Technical Review* Vol. 13, p. 240).

The signal (the curve in figure b) is approximated by a step-like signal. The modulated signal (figure a) is generated by emitting a pulse only when the step-like signal shows an upward step.

became available in the 1950s. It was not until the 1970s that digitalisation merged into the field of consumer electronics (audio and video applications), and signal quality improved.

Because the input and output signals were mostly analogue, there was a need to convert analogue signals into digital signals (A/D conversion) and vice versa (D/A conversion). At first, this was done by using discrete semi-conductors, such as diodes and transistors, but in the 1960s ICs became widely available, and it became possible to integrate many such devices on a wafer of silicon material. This also had an impact on the development of A/D and D/A converters. From 1970 on, converters were made in ICs. Developing converters then became a matter of designing the layout of an IC so that the necessary components for making the conversion happen would fit into the silicon chip. Over the course of time, more and more components were integrated, and this led to terms such as Large Scale Integration (LSI) and Very Large Scale Integration (VLSI). In telephony, LSI circuits caused quite a digital telephony cost reduction.⁵⁰ The USA was the first country in which large-scale digitalization of the telephone net took place. In the UK developments were much slower: a field trial started in 1966, and an experimental digital exchange was set up in 1968.

In the 1970s and 1980s more and more exchanges and trunk lines were digitalised. In France it was not until the late 1970s that telephone digitalization really took off. That was due to the 1971 state plan for telecommunication, which was the first large French state investment in telecommunication systems for many years.

In the Nat.Lab. in the late 1940s, the scientists F. de Jager, J.A. Greefkes, H. van der Weg and others worked on PCM systems for digital telecommunications. They looked particularly at the possible advantages of DM techniques. In the mid-1960s in the telecommunications group led by De Jager, E. Aagaard worked on new A/D and D/A converters that had to be PCM compatible, together with the necessary filters. From 1968 on, the work was continued by L.D.J. Eggermont. In the mid-1970s M.H.H. Höfelt joined him. It was Höfelt who in particular studied the potential of VLSI for the converters. Aagaard was made group leader of the 'Telecom switching systems' group. The group came under the Telecommunications sector, where H. van de Weg was director until 1972, when he was succeeded by H. Mooijweer. After him F. Valster was made director of the sector. The knowledge that was gained in this group later became important for another group in the Nat.Lab., namely the control technology group.

That brings us to the other main area of digitalisation, namely control technology. In that area, R.E.J. van der Grift worked on A/D conversion for integrated digital voltmeters in the 1970s, that was based on 1-bit technology. This was done for the PIT (Products for Industrial Applications) PD, and more specifically for the IRIA department (IRIA being the Dutch abbreviation for Indicating and Registering Instruments Almelo, and Almelo being a town in the eastern part of the country). In 1977 the PD put the first integrated digitised DC-voltmeter – developed by R.J. van de Plassche and Van der Grift – into production. This meter was based on a variant of the Delta Modulation.

In the same year, but in a different field of research, a study into 14-bit D/A converters for the ALP was started for the Audio PD, which was a successor to the 12-bit D/A converters⁵¹ that were available already.⁵² This work took place in the 'Measurement and control' group that was led by Th.J. van Kessel and A.F. Verkruijsen, and C.A.A.J. Greebe was director of the Instrumentation sector, where the group was located. Both the Telecommunications and the Instrumentation sectors were part of the Electronic Systems department, of which Teer was managing director.

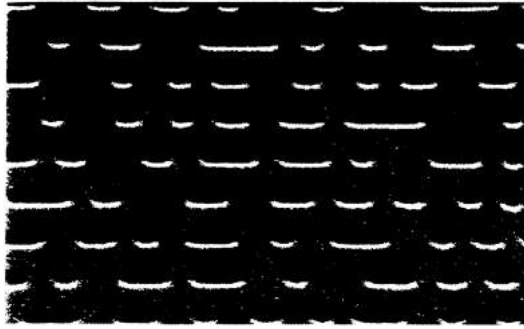
The Development of Audio Converters

While the Nat.Lab. worked on the VLP, L. Boonstra of the Audio PD started to investigate the possibilities for an audio version of this new

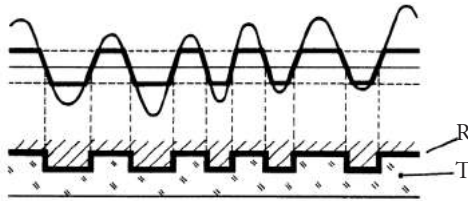
recording medium. Within the Audio PD L.F. Ottens had expressed an interest in this. Boonstra had been transferred from the Nat.Lab. to the Audio PD, but for some time he physically remained at the Nat.Lab. Later he moved to the Audio premises and continued to work there on the ALP, in co-operation with J.J. Mons. In 1974 they gave an ALP demonstration, and an FM signal was used.⁵³ Because Ottens and Boonstra preferred to optimise the product for audio, the compatibility with the VLP was abandoned.⁵⁴ The demonstration had created the impression that the use of analogue signals would not bring improvements that would make the ALP a serious competitor for the normal LP. Meanwhile, digital signals had become feasible thanks to developments in the fields of telecommunications and control technology. The decision to switch to a digital system meant that the VLP and the ALP activities had to be separated once and for all.

The first idea was to use Delta Modulation, which at that time seemed to be the only realistic option for use in audio applications. Contacts were established with the telecom groups of Aagaard, Zegers and Peek, where Delta Modulation research was concentrated.⁵⁵ Apparently, though, there were some difficulties that could not be easily overcome: in particular the integration of systematic signal error correction remained problematic. Therefore, it was decided that DM should be abandoned and that turn to Pulse Code Modulation (PCM) should be deployed. As we saw before, PCM had already been standardised for telecommunication use. The researchers had no experience with the use of PCM for audio applications.⁵⁶ From 1977 on, an effort was made to realise this with a 14-bit resolution. Halfway through that year, a demonstration was given using a 13-bit resolution.⁵⁷ By then, J. Sinjou had joined the team. He would play a leading role in the further development of what was from then on to be called the Compact Disc. In 1977 he was appointed as the leader of the new Compact Disc Development Lab.⁵⁸ The word 'Compact' was a good indication of what was one of Ottens's priorities, which was to have a small sized disc (with a diameter of 12 cm) rather than a large VLP disc (30-cm diameter).

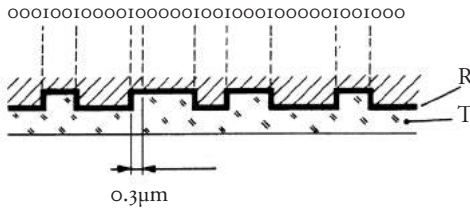
Meanwhile, in the Nat.Lab. not much work was being done on the ALP, but this changed when the use of digital signals required converters. As the Audio Division did not have any expertise in the area of converters and ICs the support of the Nat.Lab., in particular of Van Kessel's group, was called for.⁵⁹ In that group expertise in the use of PCM in telecom applications was introduced by E.C. Dijkmans, who had been transferred from the Telecommunications group to the Measurement and Control group (the Van Kessel group).⁶⁰ It was decided that the group should start working on a 14-bit converter. That would give them a slight head start on the current 12-bit technology. The Audio work meant a shift from measure-



a



b



c

- a) The pits and 'lands regions between the pits' in a disc for Laser Vision or Compact Disc Digital Audio. The width of the pits is about $0.5 \mu\text{m}$. The spiral of pits forms the 'track'.
- b) The conversion of an analogue video signal into a sequence of pits and lands in Laser Vision. Above: signal; cross-section of the disc. The frequency-modulated signal is limited in both positive and negative directions. The leading and trailing edges determine the location of the land/pit and pit/land transitions.
- c) The conversion of a digital audio signal into a sequence of pits and lands in CD-DA. Length of a pit or land is always a multiple of $0.3 \mu\text{m}$. Above: bit sequence; below: cross-section of the disc. Every pit/land or land/pit transition corresponds to a '1' in the digital signal. In between the signal always has the value '0' for every $0.3 \mu\text{m}$ of distance along the track. R reflecting layer, T transparent material.

Figure 34. The conversion of an analogue signal into the pits in a CD (from *Philips Technical Review* Vol. 44, p. 327).

Figure a shows the surface of the disc with pits. Figure b has the analogue signal above and a cross-section of the disc below. The signal is limited in both upper and lower directions. A pit starts at the trailing edge of the signal and ends at a leading edge of the signal. R is a reflecting layer (needed to reflect the laser beam), T is transparent material. In c the signal has been converted from analogue to digital before being recorded. The resulting sequence of 0s and 1s is recorded as pits by starting a pit at a 1 and ending it at the next 1, then starting the next pit at the next 1 and ending it at the next 1, etc.

ment and control techniques to audio techniques as far as the focus of the group was concerned. This shift was accompanied by a transfer of director since De Kruijff was moved from the PIT PD to the Audio PD. In the work the Nat.Lab. did for the PIT PD (for Indicating and Registering Instruments Almelo, ARIA), De Kruijff had been the primary contact person for the Nat.Lab. at the PD. Those contacts had always been good, and they remained good when De Kruijff moved to the Audio PD.⁶¹ In 1978 agreements were made about the delivery of converter-ICs. It was evident that in order to make the converters affordable, they would have to be produced as ICs. Production was planned to take place in Nijmegen, at the Elcoma factory, so for those purposes Elcoma prepared an 'acceptance of type development' agreement.⁶² In 1979 the work on 14-bit converters both for D/A and for A/D conversion resulted in a demonstration of a functioning CD system with a prototype of a 14-bit D/A converter⁶³ (D/A conversion was used to transform the digital information on the CD to an analog signal for the speaker; we shall come back to the A/D converter later). The prototype was then sent to Elcoma and production preparations started. In 1979 the first ICs were produced. The performance appeared to be well above the minimal requirements.⁶⁴

The experience with the video recorders (see section 8.3) had made Philips and other companies aware of the importance of standardisation. Both Sony and Philips had lost the video market with their Betamax and V2000 systems because the competitive Matsushita (VHS) system had become the accepted standard, and their systems were not compatible with that standard. Already in the 1970s Philips had contacted Sony about the standardisation of digital audio systems. In 1979 a principle agreement was signed, and extensive debates were held on technical issues. Sony thereby had insisted on creating a 16-bit converter IC, while Philips was at that time using 14-bit converters. For Elcoma that would mean a completely new development process, and for obvious reasons the PD was not enthusiastic about that prospect. The Nat.Lab. scientists were able to avoid this by developing a 14-bit converter that could yield a 16-bit resolution. This was achieved by employing a number of techniques that were again derived from the telecommunications area: oversampling, digital filtering and noise shaping.⁶⁵ The application of fourfold oversampling together with digital filtering – used in telephony – had already increased the resolution by 1 bit, and the noise shaping technique (implemented by E.F. Stikvoort) added another bit. Early in 1980 the converter was realised and given the number TDA1540.⁶⁶

The way in which the Nat.Lab. had managed to achieve a 16-bit standard using 14-bit converters was very unconventional. The Japanese companies worked with true 16-bit converters, and at first they criticised the Philips concept, but the Elcoma and Audio PDs accepted this solution,

and by the end of 1982 CD players with the upgraded 14-bit converters were being produced. Elcoma delivered the converter ICs and Mullard in Southampton (a Philips subsidiary) delivered the filter and noise-shaping ICs. The process of assembling the ICs into the CD players was carried out at the Audio factory in Hasselt, Belgium.⁶⁷ However, the Japanese comments were taken seriously, and in 1980 a start was made to develop a 16-bit converter. It was particularly the commercial people in the Audio PD who said that they were often met suspicion about the 14-bit solution.⁶⁸ There was also a technical reason for starting the work on a 16-bit converter, although marketing and prestige were probably more important. It appeared that when recording music from a CD to a cassette tape, bleeps were produced, and that was, of course, unacceptable. The cause of this was known to be the 'noise-shaping' technique that had been used to achieve the 16-bit standard with the 14-bit converter. A true 16-bit converter without additional techniques such as filtering and noise-shaping would not have such a problem.⁶⁹ In 1980 the Van Kessel group began developing a 16-bit converter.

By 1983 Elcoma had made it clear to the Nat.Lab. that they were not interested in pursuing the 16-bit converter IC development. As has already been mentioned, this PD did not like the idea of putting a lot of effort into the production of new ICs. Here we have another example of the problems that the independent position of the Elcoma PD could create for the Nat.Lab. in its contacts with the other PDs (in this case: the Audio PD). Even though the Audio PD supported the 16-bit converter, Elcoma did not see sufficient advantages in it, and so it did not want their own business to co-operate. After 1984 and under the pressure of the Audio PD B.J.M. Kup in Nijmegen (Elcoma) was given the opportunity to work on the production of the 16-bit converter that the Nat.Lab. had worked on. He was able to integrate both stereo channels into one IC, which reduced the price of the converter part of the CD player. In the same year (1984) the 16-bit converter was completed and given code number N2990.⁷⁰

Meanwhile, the Measurement and Control Group in the Nat.Lab. had taken the next step, which was to develop the 18-bit converter. The advantage over to the 16-bit converter lay in the possibilities for the digital processing of the audio signal: the signal could remain digital until just before the loudspeaker. In principle, each extra bit would yield a 6-dB improvement in the signal/noise ratio.⁷¹ In 1983 the design was finished, but the converter was never put into production because of the lack of demand (this again illustrates the problem of lack of commitment where new products were concerned), but the research resulted in a lot of knowledge being patented.

All of this has to do with D/A conversion (from digital information on the CD to an analogue signal produced by the loudspeaker). Meanwhile, work on A/D conversion was also continuing. In 1981 Van der Plassche finished a 14-bit A/D converter. The application was sought in medical systems.⁷² In such systems scanners read analogue signals that had to be converted into digital signals for further processing. In 1982 the group started working on a 16-bit A/D converter, but Elcoma's negative attitude to the 16-bit D/A converter made the group hesitant about putting too much effort into this.⁷³ A different application for the A/D converters was the video camera, where the conversion of the original analogue signal into a digital signal would allow for better signal processing. The layout for an 8-bit video A/D converter IC was completed in 1982.⁷⁴ For these converters, contacts were made with the Elcoma factory in Caen, France.

As far as the Audio D/A converters were concerned, it was mainly the Measurement and control group that designed these converters for audio applications, but as we have already seen, the Telecommunications group (Aagaard) was also active in digital signalling and conversion. The Audio PD never had much contact with that group because they worked on the digital concept, while Van Kessel's group worked on analogue electronics. There was also a gap between the techniques used by Aagaard's group and the applications in the Audio PD. The work in the Telecommunications group had focused on the use of Pulse Code Modulation (PCM), and a 1-bit converter had been developed to enable the conversion of PCM signals to 1-bit digital signals. These converter-ICs were realised in MOS, which was seen as being more suitable for digital electronics than bipolar ICs (see Chapter 6 on LOCOS IC-technology). This 1-bit converter in MOS used much less energy than the (bipolar) 14- and 16-bit converters that the Measurement and Control group had designed for the Audio PD.⁷⁵ When the portability of the CD player became a serious issue, this energy use also of course became an important condition. The less energy needed, the longer a CD player can run on one set of batteries. From 1986 on, the Measurement and Control group recognised the importance of low energy use, and so it also started working on a 1-bit converter.⁷⁶ As the Elcoma factory in Nijmegen was specialised in bipolar ICs and the Mullard factory in Southampton was specialised in MOS, contacts with Mullard were established for the 1-bit converter. There were contacts with Elcoma in Nijmegen as well.

P. Jochems and A. Schmitz worked within the IC-technology group led by W.G. Gelling that was working on MOS. The two aided the Measurement and Control group in its work on converters with their IC-technology experience, but they were also invited by the factory in Nijmegen to do some experiments there, because sometimes their know-how was found to be useful to Nijmegen, too. Their presence was not enough,

though, to cause Nijmegen to switch completely to MOS techniques. Mullard remained the best place for producing the MOS converter ICs. The Audio PD that in 1985 had merged with Video to become the Consumer Electronics PD then built these new D/A converters into the CD players. Later, when Nijmegen also started producing MOS ICs, the contacts through Jochems and Schmitz appeared to have paved the way for co-operation with the Nat.Lab. Thus, in the 1990s, the contacts with Elcoma concerning converters became more intense. The Measurement and Control group gradually also became involved in production problems and so had less time to develop new converters. The effect of the Centurion operation reinforced this. The work on D/A and A/D converters is still continuing today. Improvements for the implementation of converters and new applications areas are still being investigated.

In the introduction to this case study, it was already indicated that the role of the Nat.Lab. was primarily to serve as a source of know-how rather than as an innovation centre. This was different in the case of the VLP, because there the Nat.Lab. had taken the lead in development, and the PD had only supported by sending people. In that respect, both the VLP and the CD are examples of how the Nat.Lab. worked together with PDs in the 1970s and 1980s. The difference, though, lies in the basic responsibility that shifted from the Nat.Lab. to the P. Neither the VLP nor the CD had been developed on the basis of contracts between the Nat.Lab. and the PD. We characterised the 1972-1994 period as 'the road that led to contract research'. At the time of the VLP and the CD developments the Nat.Lab. was still on that road and not yet at its 'destination', but the experiences that both the Nat.Lab. and the PDs gained through events like the VLP and the CD development paved that road. We should thereby remark that in the case of integrated digital audio converters, the contacts with the Elcoma PD were mainly at the workfloor level, while the contacts with the Audio PD were at the directorate level, too. This had to do with the special position of Elcoma as the device PD among the system PDs. For contract research, contact at the workfloor level was of course important for the preparation of contracts, but it was not sufficient for making the required formal agreements. In this particular case it illustrates the 'road to contract research' with respect to the Audio PD more than with respect to the Elcoma PD.

Another aspect of this case study that deserves special attention is that of the transfer between different fields of know-how within the Nat.Lab. In the case of the VLP, we have already seen how quite different knowledge domains, like optics and mechanics, were brought together in the development of this product. In the case of the A/D-D/A converters, it was demonstrated that knowledge from two domains (telecommunications

and control technology) was transferred to the domain of audio equipment. Again, this could only happen in a laboratory that has all these different disciplines under one roof. This certainly appeared to be one of the major strengths of a corporate research laboratory such as the Nat.Lab., where a broad source of knowledge areas was created for the PDs.

8.3 Magnetic Recording Heads for Consumer Video Cassette Recorders⁷⁷

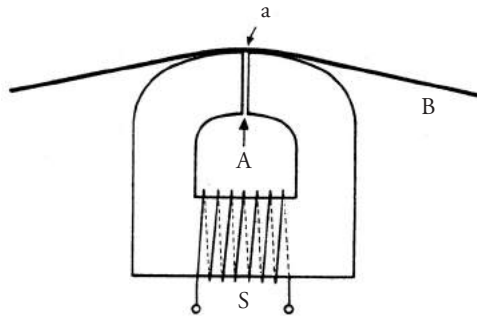
When selecting topics for case studies, the representativeness of the Philips product range has been taken into account. Recording is closely related to important Philips products such as radio and television. Optical recording is one of the areas where the contribution of the Nat.Lab. has been quite substantial, and in the VLP case study there were good examples of that. The other recording technique which existed long before optical recording was magnetic recording. As with optical recording, magnetic recording also required a wide field of research, which cannot be completely described in this case study. Therefore, description limits must be set. This is done by choosing the recording heads as a specific device to which research has contributed. The case study will be narrowed down further by focusing on magnetic recording heads for consumer cassette recorders. Of course, this cannot be seen as separate from previous work on professional equipment and on video tape recorders. To understand the role of the work on recorder heads, the development of the whole video system has to be looked at. Therefore, the case study description will contain the development of the whole system, but particular attention will be paid to the work on the heads.

This case study takes us into the main area of materials research. One previous case study in that field was the study on ferrites (see Chapter 4). The work on magnetic recording heads is in a way an extension of that because ferrites were the materials to be used for the recording heads. The fact that the Nat.Lab. had built up a lot of expertise in this field was in some respects a disadvantage. It sometimes made them disregard important developments elsewhere (such as the potential of monocrystals for ferrites). In a way, this is similar to the fact that the Nat.Lab. was a latecomer in the use of silicon for electronic components because of the successes booked with germanium (pushed-out-base) transistors. Research on magnetic recording was done in the 'systems' department. Here the focus was on the recording equipment as a whole, while in the 'materials' department the materials used for the recording heads were investigated. Of course there were contacts between the two, but each had a distinct field of research.

Magnetic Recording: Initiation and First Commercial Developments

The idea of magnetic recording goes back to 1898 when a Dane, Valdemar Poulsen, submitted a patent for recording sound signals on a steel wire that was pulled between the two poles of an electromagnet. At that time it was not yet possible to amplify the signal, and the idea was not developed further. In 1927 Pfleumer in Germany and O'Neill in the USA had invented tapes of paper and plastics that were covered with a layer of magnetic material. Such tapes had been used in 1935 when Stille, Schüller (working at Telefunken) and others in Germany had brought out a recording machine, the Magnetophone, based on Poulsen's ideas. It recorded signals with frequencies of up to 6,000 Hz, and the tape speed was about 1.5 m/s. Approximately half an hour of sound could be recorded, and this required a tape length of 2.7 km. Both wire and tape recorders were developed further in Germany during WWII. The wire recorders were generally more simple and cheaper, but they had the disadvantage of breaking and tangling the wire.⁷⁸

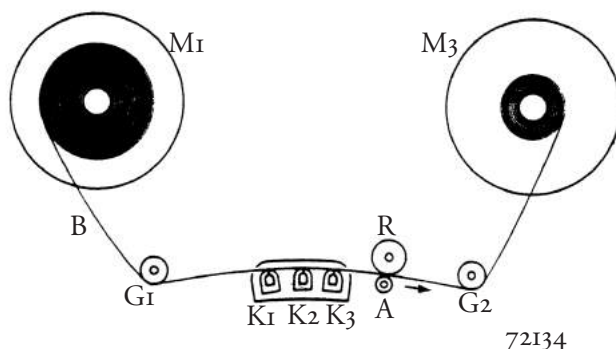
The signal heads for writing and reading consisted of ring-shaped electromagnets with small slits or gaps along which the tape ran. A varying current in the electromagnet corresponded to the changing magnetic field as the tape with its changing magnetic pattern ran along the head. The smaller the crystals in the material, the more precise the changes in magnetic pattern and the better the signal quality were.



The recording head, over which the magnetic tape passes. The modulation current flows in coil S. The tape B passes in a gentle curve over the gap A. Recording takes place at a.

Figure 35. A recording head with a magnetic tape passing over it (from *Philips Technical Review* Vol. 14, p. 182).

S is a magnetic coil that is driven by the electrical signal (the sound signal that is to be recorded). The alternating current causes a changing magnetic field in the gap A. The alternating field is concentrated in a, where the recording on tape B takes place.



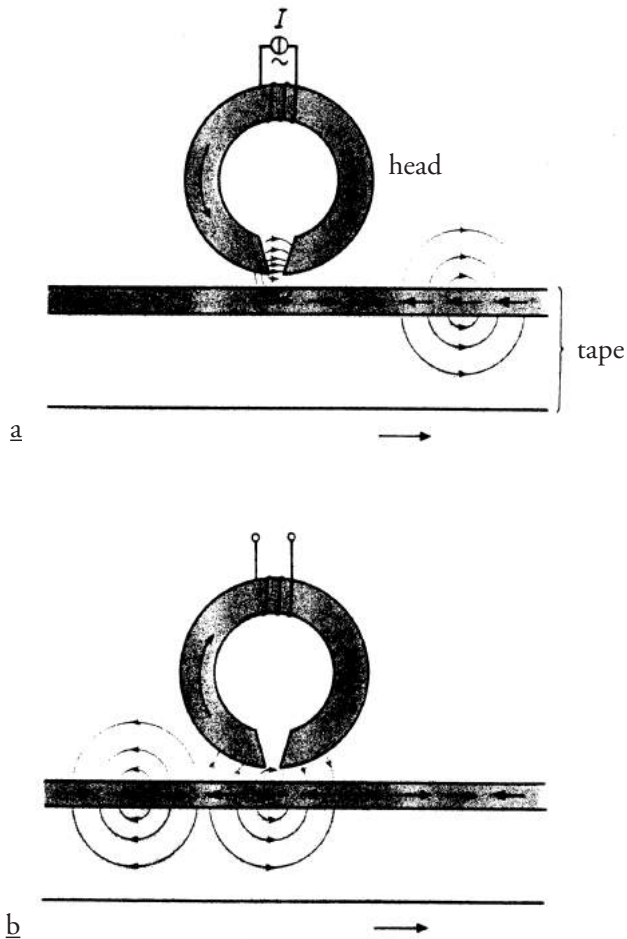
Principle of the tape drive. *A* = transport spindle: *B* = magnetic tape: *R* = rubber roller holding the tape against the transport spindle. Erasing, recording and playback heads *K*₁, *K*₂ and *K*₃ are assembled in a common unit. *M*₃ = winding spool: *M*₁ = supply spool: *G*₁ and *G*₂ = guide rollers.

Figure 36. Erasing, recording and playback heads (from *Philips Technical Review* Vol. 14, p. 186).

The tape is drawn by the rotating spool *M*₃ from spool *M*₁ via guide rollers *G*₁ and *G*₂, the transport spindle *A* and the rubber roller *R*. The three heads *K*₁, *K*₂ and *K*₃ are assembled in a common unit.

Within Philips, the idea of magnetically recording television signals was launched by Holst in 1952. In an Orco meeting in 1953, a distinction was made between professional and consumer applications.⁷⁹ According to Hartong (Product Division Apparatuses) it would be useful to pay attention to both options.

In 1956 the Ampex Corporation in the USA brought out its first commercial professional video tape recorder for the recording of television programmes for later use in broadcasting.⁸⁰ It cost a staggering \$50,000 and used a two-inch tape and four recording heads. Two years later Siemens and Cinema Television introduced the same equipment in Europe.⁸¹ In 1958 Philips, too, had its own video tape recorder with 125 recording heads on a rotating disc. Compared with the Ampex equipment, the Philips recorder had a more expensive disc, and with its 20-minute recording time, it could not compete with the 60-minute recording time of the Ampex machine.⁸² The Nat.Lab. researchers discussed the options for improvement and saw the best research possibility in the reduction of the number of heads and in the registering of more than one television line (e.g. five) per track, while maintaining the existing rotation speed of 30,000 rpm. This was, however, a rather dramatic change, and so



a) Diagram representing longitudinal magnetic recording with a tape-recorder head on a moving tape coated with a magnetizable material. Magnetization directions in the materials are denoted by red arrows; magnetic lines of force outside them are denoted in blue. The signal current I generates a magnetic leakage field in the head gap via the magnetization of the head material. This leakage field records the signal on the tape by magnetizing the coating parallel to the surface of the tape.

b) The recorded information is read with the same head. The magnetic flux from the tape magnetizes the head material, which induces a signal voltage V_{ind} in the coil in the head.

Figure 37. Recording and reading with the same head (from *Philips Technical Review* Vol. 44, p. 102).

the 'one-line-per-track' option was retained.⁸³ According to J. Haantjes, one of the researchers, the main problem of magnetic recording was the movement of the head in relation to the tape, and not the construction of the head. For the heads, ferrites were used. Ferrites had a number of advantages compared with the alternatives; a smaller slit length was possible, the Eddy current losses with high frequencies were smaller, and the material used allowed very precise shaping. Moreover, the wear with oxides was less than with metals. In the USA and in the UK the slit was made of metal, because ferrites were considered to be too brittle. The brittleness caused problems particularly at higher tape speeds (these were used mainly for professional applications). Because of its knowledge of ferrites, the Nat.Lab. expected that it would be able to make better ferrite slits without the need for metal pieces. In 1960 a prototype was tested in which a number of other improvements were implemented, too. This prototype was developed in co-operation with the RGT (Radio, Gramophone and Television) Product Division.⁸⁴ The experiments done with this prototype and a second prototype revealed several technical problems, such as the poor contact which existed between the head and the tape. More research was therefore done on the shape of the head for minimal wear. The Ferroxcube used for the head was produced by the Icoma Ceramics factory. Because of the strong position that Ampex was in, good contacts with that company were seen as important. The know-how that Philips had developed attracted Ampex's interest, but a proposal for cross-licensing was not accepted by Ampex. The licence offered by Philips had to do with the construction of the heads, while Ampex saw the wear of the heads as the main problem. Philips' knowledge would probably solve just a minor part of that problem.

Around 1962 a survey of the state of affairs regarding magnetic recording heads was carried out by Kuipers, and the results were discussed in a meeting of representatives from the ELA, Icoma, RGT, X-ray and Medical Equipment PDs, and from the Nat.Lab.⁸⁵ The wear problem (the crumbling of the slit ferrite that occurred when the tape ran along the head at high speed) was identified as one of the main problems. The Duinker group in the Nat.Lab. worked on this. The slit was constructed by melting two Ferroxcube blocks together with a glass strip. The crumbling of the slit was caused by bubbles formed in the glass. Apart from this, the slit of the head had to be made smaller so that lower tape speeds could be realised (thus resulting in a longer playing time), and higher frequencies could be recorded. Other problems that had more of a systems character were studied in the systems department that was led by Teer. Kuiper's study also revealed that there were different opinions in the ELA, RGT and Icoma PDs. Icoma preferred to develop and make heads for professional use and to sell them to external parties. This was attractive for them

from a financial point of view, since one recording head could yield as much as several hundred Dutch guilders. ELA was opposed to that, though, and would have preferred Icoma to deliver heads only to them. RGT was more interested in heads for consumer applications. It was decided that Icoma would deliver Ferroxdure heads to both ELA and RGT for development purposes. Icoma would then be very careful when delivering heads to third parties not to damage the interest of RGT and ELA.⁸⁶ The issue was seen as being important enough to be discussed at the company's Board of Management level, where the decision was supported. The problem of slit erosion was to be studied by the Nat.Lab. in co-operation with the Icoma Ceramics Lab. In the RGT PD also, research was particularly done into the melting of glass onto the Ferroxcube. When it came to bringing out the first commercial video recorder, the EL3400, two Ampex patents were required, and Philips' existing agreements with Ampex allowed this.⁸⁷ Nevertheless, some care was taken to prevent possible problems, as previous efforts to co-operate with Ampex had failed, as has been illustrated. By 1964 Philips had submitted about 50 patent requests, 18 of which were related to the head construction.⁸⁸ In that year the EL3400 was presented in Vienna, Stockholm, Amsterdam and Copenhagen. The price of this semi-professional recorder was NLG 7,000. It used a one-inch tape and had a recording time of 45 minutes.⁸⁹ The development and production were not completely without problems. The development work took up much of the PD's capacity, and there was serious concern about Icoma's abilities to supply enough heads. Therefore, in 1965 the PD sounded the alarm bell.⁹⁰

In the same year Mr. Ohga, manager of the New Products Division from Sony, visited Philips to talk about possible co-operation in the field of home video recording. Like Philips, Sony too had tried in vain to settle an agreement with Ampex. It was Sony in particular that was interested in Philips know-how in the fields of recorder heads. In 1966 two Sony directors visited Philips, and it was decided that a limited form of co-operation should be set up.⁹¹ Sony aimed at developing a system with 0.5-inch tapes and a minimum playing time of one hour. During the first years of co-operation, the Nat.Lab. was not very involved. Probably the reason was that the Nat.Lab. was not very active in recorder heads at that time, but gradually the Nat.Lab. involvement increased.

In this VTR (video tape recording) or pre-VCR (video cassette recording) period, the Nat.Lab. did supportive materials research for the ELA, RGT and Icoma (from 1965 on, Elcoma) PDs. There is no evidence of revolutionary new concepts being promoted by the Nat.Lab. Most of the work was of a developmental nature and done within the PDs, even up to capacity limits. The focus was on professional and semi-professional

equipment. The contact with Sony, which at first was mainly a PD matter, provided one of the incentives for shifting towards more work on home video recording. It was evident that much work had to be done in this area. Given the capacity problems that had already emerged within the PDs, an increase in the Nat.Lab.'s role was seen as necessary. Elcoma put in a request to Pannenburg to set up a meeting with Haaijman and Rathenau and PD representatives.⁹² This was the beginning of a period of more intensive Nat.Lab. involvement in the video recording developments within the company.

Towards a First Home VCR

In 1966 it appeared that extra effort by the Nat.Lab. in the Ferroxcube field was not a luxury. A trip to the USA made clear that Japanese firms had brought out Mn-Zn and Ni-Zn ferrite material that had very high permeability and low losses. In a meeting about this problem, it was decided that for recorder heads, developing single crystals might provide an interesting answer to the Japanese developments.⁹³ The same idea was mentioned in a meeting in 1967 with respect to the application of radar equipment, but that was rejected as too expensive. The result was that neither for radar nor for recorders was any substantial work on monocrystals initiated.

Meanwhile, the co-operation with Sony materialised, and meetings were held on the development of magnetic video recorders. During those meetings two important issues were raised. In 1968 wear was identified as a crucial problem. Damage to the slit and the degrading of recording properties had to be reduced. It seemed as if a non-magnetic layer was formed on the surface during head use, and that removal of the top layer (0.2 micrometer) increased the output. According to Yamakawa (Sony), the Sony heads had the opposite problem, because during use output increased, and that was not desirable either. Substantial improvements were achieved by adapting the construction and the ferrite material of the heads.⁹⁴ In 1969 Philips brought out a new type of recorder, the LDL 1000, with a 0.5-inch tape that was priced at 'only' NLG 2,000. This was the first type for which a serious marketing campaign for the consumer market was set up.⁹⁵

The second important issue raised in the contacts with Sony was that of the use of tape cassettes rather than tapes on reels.⁹⁶ There the recording density had to increase to enable sufficient playing time for a video-cassette compared with the videotapes. The width of the tape also had to be reduced to make the cassettes sufficiently compact. This had implications for the recorder heads. The Nat.Lab. suggested using transversal scanning, thus enabling narrower tracks to be used. The idea was favourably received by RGT and ELA.⁹⁷

In 1970 Sony took a prototype of their first VCR to a fair in Osaka, Japan. The name of the system was U-Matic. It used a 0.75-inch tape that had a playing time of one hour. The machine was expensive and bulky. It was primarily intended for studio broadcasting and not for use as a consumer product.⁹⁸ In the same year Philips announced that it was bringing out a VCR system with a 0.5-inch tape. In 1971 this VCR system appeared as the N 1500. It cost about NLG 3,000. There were tapes for playing times of 30, 45 and 60 minutes.

The Development of the V2000

Meanwhile, the position of Philips in the recorder head market was not very good. In 1972 a survey conducted by Vrolijkx revealed a decrease in turnover and profit.⁹⁹ There were also problems with the ferrite material that was delivered by Elcoma to the recorder factory in Vienna. The Nat.Lab. had worked with the RGT ELMA development lab on MnZn ferrites for recorder heads. This material proved to be more difficult to produce than the previously used NiZn ferrite. In the process leading from the ceramic material to the completion of the recording head, the fall-out rate was as high as 60-70%. This even rose to about 90% after production had been fully transferred from the Nat.Lab. to Elcoma.¹⁰⁰ Cracks would appear in the ferrite tiles, and after ultrasonic cleaning 50-60% of the tiles would become unusable, and there were losses during the slit shaping process. Again the Nat.Lab. was called in to search for a different production process, or for a better control of the process. Together with the ELMA Lab some improvement was made (30-40% of the original ferrite now found its way into the recorder heads).¹⁰¹ The situation remained problematic and Elcoma seriously considered halting production. Elcoma did not expect that much more could be done by the Nat.Lab.

In 1974, in a Directors' contact meeting, the issue of aligned polycrystalline material was discussed as a possible improvement.¹⁰² The Nat.Lab, in particular Wijn, was asked to investigate the possible merits of this material which, at a recent conference, had been claimed to be better not only in terms of magnetic properties, but also in terms of mechanical properties. At the next meeting doubt was expressed about these claims, in particular with respect to the magnetic properties of the material. In addition, the Nat.Lab. stated that it was too late to start working on aligned ferrites, either polycrystalline or monocrystalline. Matsushita already had good, aligned polycrystalline ferrites, Sony had produced large monocrystals in its laboratory, and Toshiba also had monocrystalline ferrites, so in the case of both options, Philips was lagging behind.¹⁰³

In the meantime, complete video recorder system development continued, and new versions were brought out. In 1975 the N 1501 was

launched as the N 1500's successor, and in 1977 the N 1700, which used cassettes that had a playing time of more than two hours due to the lower tape speed. Unfortunately, the old (N 1500 and 1501) pre-recorded cassettes could not be used in the new machine. The Japanese competitors had been active as well. In 1974 Sony brought out its Betamax system that used 0.5-inch tape. In 1977 JVC's Shizuo Takano visited a number of European companies, including Philips, to demonstrate the VHS system, which had been introduced to the American market the year before. Philips responded negatively, unlike Thomson (in France), Telefunken (in Germany) and Thorn (in England).¹⁰⁴

In all these years the struggle to combat the recorder head wear and to cope with production problems continued within Philips, and in spite of extensive research on the wear of various materials, no clear ideas about how to proceed existed.¹⁰⁵ In 1976 the Video/Audio PD expressed a clear preference for the use of single crystals in the new N 1900. These were seen as being necessary for solving the problems with the recorder heads. Alongside the Japanese (notably Matsushita and Toshiba), Philips had a backlog of about two years. In the Nat.Lab. a crystal growth process was developed which by the end of 1977 could be transferred to Elcoma.¹⁰⁶ The work had taken place in co-operation with the Elcoma and Video PDs (in a joint project group). An explorative study using Fuji single-crystal ferrite material had shown good results. The Nat.Lab. was soon able to produce its own version of this material. By then it was possible to produce crystals with a diameter of 2 cm. A target was set to achieve 4 to 6 cm diameter slits. A study conducted by Elcoma indicated that single-crystal recorder heads indeed functioned much better than the polycrystalline recorder heads being produced up until then. The Philips Board of Management responded positively to a request put in by Elcoma to set up production facilities for monocrystalline heads in February 1978. In April the test oven started producing crystals. The yield was not very high at first, and the Nat.Lab. co-operated with Elcoma and Video to improve this. The new heads were intended for use in a new type of video recorder that the company wanted to bring out in October 1979, the V2000. This new type was seen as a good answer to the Betamax and VHS VCRs that Sony and JVC had on the market. In June 1979 the first demonstration was held, and by the next year the V2000 was on the market.

The Commercial Failure of the V2000

In the beginning the chances of the V2000 being a success were estimated as reasonable. The machines were considered to be technically superior to the Betamax and VHS, and not only because of their heads, but that certainly contributed to the technical merits of the system. The reputation

of the system worsened somewhat when reliability problems emerged. More importantly, the prices of the competing systems had by then been substantially reduced, so that from the very beginning the V2000 was problematic from a marketing point of view. The V2000 also had a backlog with respect to pre-recorded cassettes. Even before the V2000 was demonstrated in the Nat.Lab., its possible successor, the V4000, was being discussed.¹⁰⁷ In the Bongers group the search for other suitable head materials continued. Elcoma was prepared to deliver magnetic materials for head research. In October 1982 a study into MnZn ferrite with a high saturation value for better writing heads was started in the Bongers group.¹⁰⁸ For the V2000 these research efforts, though, did not bring reprieve. Within a few years it became evident that the V2000 had lost its market share – which had never been very high – to the competing VHS system. The only comfort was that Sony had the same problem with its Betamax system. In 1984 Philips started producing VHS systems – in addition to its V2000 production – and in 1985 the production of the V2000 system came to an end.¹⁰⁹

This case study illustrates a number of characteristics for the 1972-1994 period. In this period the role of the Nat.Lab. on the road to mutual commitment gradually changed. Fewer efforts were made to transfer new ideas from the Nat.Lab. to the PDs and more often than before, the Nat.Lab. provided the PD with specific knowledge on the development of new products. In the field of magnetic recording heads, this shift in initiative was evident. In the early 1960s the Nat.Lab. searched for new developments and tried to ‘sell’ those to the PDs, but in the 1970s the PD turned to the Nat.Lab. for research support in this field. In the late 1960s and 1970s we see a continuous flow of material research support on the part of the Nat.Lab. for PD activities. This research clearly came in response to the problems that were put forward by the PDs in the Directors’ contact meetings and management meetings. Sometimes the Nat.Lab. was called in to supplement the insufficient PD development capacity. This can almost be regarded as a sort of contract research *avant-la-lettre* (‘almost’, because the PD did not pay for the Nat.Lab. support).

8.4 A Knowledge Centre for the Company

The three case studies have revealed the increasing co-operation with the PDs and the growing mutual commitment. As we saw in Chapter 7, contract research was introduced to formalise this mutual commitment. The company-wide Centurion operation of the 1990s further forced the research organisation to co-operate more intensively with the PDs and to focus on market needs (see section 7.2). The result was that for the PDs the Nat.Lab. became a contract partner able to support the development

work of the PDs. The Nat.Lab. and the foreign labs served as a source of scientific knowledge for the PDs. This knowledge was delivered according to the contracts established in a communication between the research organisation and the PDs. This was still the current practice when this text was written. Rather than trying to serve as an innovation centre – as the Nat.Lab. had tried to do in the previous period – Philips Research now functions as an invention and knowledge centre for the company. For now, that can be seen as the outcome of the road towards mutual commitment.

As we saw in the previous Parts, serving as a knowledge centre and as a source of patents is a continuous contribution that the Nat.Lab. makes to the Philips company. In Part I we saw how this resulted in a knowledge base for the company, which enabled it to realise the desired extensions of the product portfolio. In Part II we saw how knowledge was gained both in fields for which no industrial applications were yet foreseen (this is what we called ‘fundamental’ research) and in fields that related to existing products, where the knowledge could be used for further developing or improving the products. In some cases the knowledge culminated in successful new products or processes being initiated and followed through by the Nat.Lab. itself (in particular the Plumbicon and LOCOS should be mentioned in this connection). In Part III we saw how the increasing concentration on the PDs’ interests and the increasing co-operation (illustrated in particular by the case studies) led to the development of knowledge that was more directly related to specific needs within the PDs.

9. Epilogue: The Dynamics of the Nat.Lab. as a Professional Organisation within the Philips Company

In the previous chapters we have seen the characteristics of the Nat.Lab. as a professional organisation in the context of the Philips company. Now we are able to review the historical dynamics in the role that the Nat.Lab. envisioned playing for Philips.¹

9.1 Changes in the Role of the Nat.Lab.

In the first main part of its history (1923-1946), the Nat.Lab. can be characterised as follows:

- the main goal of the Nat.Lab. was to enable the company to realise the product diversification that it had decided on; the Nat.Lab. developed new knowledge, that knowledge was protected by patents and was then used to realise the desired product diversification;
- the Nat.Lab.'s means grew as the company grew; both the company and the Nat.Lab. underwent a substantial growth in this period;
- to attract top scientists, Holst created a culture in the lab in which academic status played an important role; at the same time he stimulated an industrial awareness among those scientists;
- the structure of the lab was rather informal; this allowed some individuals to behave very independently;
- the main way in which the Nat.Lab. influenced the company was through its many contacts and co-operation with the company's directors (in connection with the company's product portfolio) and with the factories (practical co-operation in the developing of concrete products).

The last part of this period (1940-1946) was a turbulent one. World War II forced the Nat.Lab. to cease some of its research projects because nobody wanted any of the findings to be abused by the Germans, and other research topics had to be continued in secret; after WWII the company's structure was formalised. In this structure there were Product Divisions (PDs) each of which had its development lab(s). This placed the Nat.Lab. in a new position with respect to debates on new products for the company.

The second main part of the lab's history (1946-1972) can be characterised as follows:

- the Nat.Lab. maintained that its specific goal and contribution to the company (unlike the development labs' contributions) were to gain an understanding of all sorts of natural phenomena which in the long term could be useful for developing new products ('fundamental' research), as well as contributing to major improvements to existing products (this type of research was also done in the foreign labs that were added to the research organisation). At the same time Nat.Lab. scientists often felt forced to do development work because in their eyes the PDs' development labs were not sufficiently competent to make optimal use of the research output. Just as in the first period, the outcome was much patented knowledge; the knowledge was not only passed on to the PDs by transferring product ideas, but also through the transfer of people to the PDs;
- the favourable economic circumstances facilitated a growth in means for both the company and the Nat.Lab. that even exceeded that of the previous period;
- the academic dimension of the lab culture was enhanced, and during many discussions at the management level, concern was expressed about 'fundamental' research (this term was used to denote the type of research that was particularly viewed as the terrain of Nat.Lab. research);
- the lab's structure became more formalised; groups were established, with group leaders, and a hierarchy of managing directors, directors, group leaders, scientists and assistants and technicians emerged;
- exerting influence on the company in conjunction with new products was a painful process due to the lack of mutual commitment on the side of the Nat.Lab. and the PDs. The PDs felt that they had no say in the Nat.Lab.'s research programme, and conversely the Nat.Lab. felt frustrated because the PDs left much of the research output unused. When it came to improving existing products or production processes, the contacts at the workforce level were good.

By the end of the 1960s the social attitude towards science and technology had become more critical. At the same time, the period of economic growth had come to an end. These new circumstances had a huge impact on Philips and on the Nat.Lab.

In the third main part of its history (1972-1994), the characteristics of the Nat.Lab. were as follows:

- the goals of the Nat.Lab. slowly shifted towards the needs of the PDs rather than reflecting the Nat.Lab.'s own ideas on desirable new developments; gradually, a balance emerged between maintaining certain key capabilities that were seen as necessary by the lab itself and unfold-

- ing activities in PD application areas; new knowledge and patents were gained on the basis of the PDs' needs and interests;
- the Nat.Lab.'s means had become more limited; in 1989 the introduction of contract research forced the Nat.Lab. to acquire about two-thirds of its budget from the PDs (before that, the whole budget had been allocated by the company's Board of Management); the limited means became a new motive for transferring people to the PDs, but this transferring of course resulted in the transferring of 'embodied' knowledge as a boost to the company's existing body of knowledge;
 - the culture of the Nat.Lab. gradually became more PD-oriented, although having a high level of scientific quality was still seen as important;
 - the structure of the Nat.Lab. was further extended to contain new functions that related to the orientation of PDs' needs (e.g. to the R-PD co-ordinators, who were responsible for co-ordinating the Research effort for the PDs);
 - several initiatives aimed at creating better relationships between the Nat.Lab. and the PDs. Of these initiatives the R-PD management meetings appeared to be insufficiently effective; the project centre in Geldrop proved that good co-operation was possible for concrete projects; the transfer projects created experiences in formalising co-operation. All this contributed in a certain way to the fact that the Nat.Lab. became recognised in the company as a knowledge centre on which the PDs could draw for their product development. It was especially in the field of design and systems work that the Nat.Lab. became an important knowledge centre for the company.

The late 1980s and the 1990s saw a dramatic reduction and rationalisation of the whole company. For the Nat.Lab., this constituted a strong incentive to focus on the PDs' needs and to learn to live with limited resources, the acquisition of which required serious effort. By the mid-1990s this process had to a certain extent been completed.

To summarise: there was, and is, a continuous role of the Nat.Lab. as a centre for the development of new knowledge about phenomena on which existing products are based or on which new products can be based. Thus, throughout its history the Nat.Lab. has contributed to the company's patent portfolio. The role of the Nat.Lab. in determining the company's product portfolio (as an 'innovation centre') has changed over the course of time.

9.2 The Nature of Industrial Research

In the pre-WWII period the Philips Nat.Lab. seems to have been a follower rather than an initiator with respect to the company's product port-

folio. Sometimes we get the impression that other labs differed from the Nat.Lab. in that respect. According to Chandler, product diversification was often instigated by a 'push' from the research laboratories,² but his level of analysis is not such that he is able to distinguish between the research laboratories taking the lead in identifying new product fields or enabling the company to realise the desired diversification. In the case of Philips, the latter situation appears to be a better description of the lab's role than the former. A more careful study of the relationship between the lab's research diversification and the company's product diversification for other companies is needed if more clarity is to be obtained on this issue. The image of the research lab as a follower also seems to fit the description given by C.E. Kenneth Mees in his 1920 book on the organisation of industrial scientific research: ' . . . the primary business of an industrial research organisation is to aid the other departments of the industry'.³ This image of an industrial research lab might be surprising for those who subscribe to the popular, intuitive image of industrial labs leading companies into totally new product fields. The history of the Philips Nat.Lab. can be helpful if one wants to obtain a more realistic image of the nature of industrial research. In the second period (covering roughly the 1950s and 1960s) it would seem easy to criticise the lab's autonomous behaviour and problematic relationships with the PDs, but we should realise that the economic circumstances were favourable, which allowed a lot of innovations to take place. It was pointed out that a number of very successful innovations were closely related to the lab's own initiative and not to PD demand (Plumbicon and LOCOS). A lot of new knowledge about materials was also gained, which had a more indirect impact on the development of new products. The shift towards a more market-oriented and PD-oriented research approach in the last period (1972-1994) was, it seems, the most logical reaction to the changing economic and social conditions. At present, the role of the lab as a knowledge centre designed to support the development work in the PDs seems to fit in well with what is a turbulent and difficult electronics market. Soon after the introduction of contract research, some concern was aired about the long-term role of the research labs.

The fact that the Nat.Lab. was able to make these adaptations to the changing environment relates to its position as a corporate laboratory within the company. This highlights an important divergence from a number of other companies that have been mentioned. Those other companies only had small laboratories for the long-term and 'fundamental' research activities and separate laboratories for the more development-oriented type of research work. At Philips, one corporate research organisation (Philips Research) was maintained that housed a combination of different types of research (ranging from more 'fundamental' to more devel-

opment-oriented research). That enabled the lab management to adjust the balance between the various types of research work according to changing circumstances. Thus, the amount of 'fundamental' research could be extended or diminished according to what was seen as appropriate given the circumstances in which the company found itself. The Philips Nat.Lab. acted as a knowledge centre for the company, comprising a wide variety of disciplines and an adaptable relative amount of 'fundamental' research in the first 80 years of its existence.

Throughout the years the Philips Nat.Lab. appears to have fulfilled a continuous role as a source of inventions that has led to a patent portfolio that has given Philips a worldwide position of independence and has made it a centre of knowledge covering a wide spectrum of disciplines, knowledge that has been used by the company to develop new products and to improve existing products and processes.

9.3 Comparison with Other Industrial Research Laboratories

In the introductory chapter of this book, it was mentioned that the history of a number of major industrial research laboratories has been written. This book has added a new lab to that list. Now the question can be raised: how does the history of the Philips Natuurkundig Laboratorium relate to other lab histories?

There are several pitfalls that must be taken into account when seeking an answer to this question. An important one is the following. At first sight, it seems that all laboratories have gone through similar changes. Many of these changes can be described in terms of the position of 'fundamental' or 'basic' research in the lab's research programme. It has already been stated before that often, these terms suggest a different meaning than they really represent. Rather than indicating a specific type of research (either distinguished by its methodology or by its objects) often these terms refer to research in which the research organisation wanted to be autonomous in its choices and preferences. Both the Philips Research Laboratories and other labs have gone through periods in which this autonomy was strong and periods in which it was weakened by the demands of its 'mother' company. But it would be naive to report on these similarities without taking into account that the driving forces behind these changes may differ substantially between the various labs. This may be the case particularly when we compare laboratories that operated in different industrial sectors.⁴ Even within one sector, it is possible that changes that seem to be similar are caused by different factors. One important one is geographic: the American context for industrial business corporations was quite different from the European context. This should make us careful when comparing

the history of different laboratories. It also makes it very difficult to draw lessons from historical accounts of lab histories. In Reich's words: 'what worked well for one company might not work for another'.⁵ Yet in this section an effort will be made to draw some comparisons with other large laboratories.⁶

In the history of the Philips Research Laboratories, we have seen two main transitions: one shortly after WWII and a second in the early 1970s. The same transition moments were identified in other lab histories too. In his account of the GE Labs' history, Reich identifies WWII and the 1980s as turning points. But reading his account carefully shows that what happened in the 1980s was the outcome of a process of change that started in the early 1970s just like we saw in the Nat.Lab.'s history, in which the transition to contract research in the 1980s was the end of a 'road towards mutual commitment'. Buderer's brief survey of the Siemens laboratories' history also has WWII and about the early 1970s as turning points (he mentions 1969 as an important year of change). Because these two laboratories operated in the same industrial sectors as the Philips Nat.Lab. did, the similarities in transition periods are a good reason to draw comparisons with these two labs in particular.

The General Electric Research Department was initiated in 1900 with Willis Rodney Whitney as its first director. Like Philips, General Electric originally had light bulbs as its main product. The motivation to start a lab for General Electric was acquiring patents to ensure its survival in the competition, as it was in the case of Philips, but under different circumstances. In Europe, for Philips the reason for being keen at building up a patent portfolio was the formation of cartels; in the USA, however, it was the antitrust legislation that motivated companies to develop a patent portfolio. Like Holst, Whitney wanted to attract the best scientists by offering them a place for doing research with a lot of freedom and an academic atmosphere. The analogies between the two labs caused high-level contacts between the Philips Nat.Lab. and GE's Research Department. Oosterhuis visited the GE lab a number of times in the years between 1921 and 1935, as the GE Lab's guest book indicates, and in 1925 Van der Pol visited the GE Lab. Conversely, GE managers visited the Philips Nat.Lab. in those years (e.g. Rice, GE's honorary chairman, in 1922-23, and also Whitney himself). From both sides positive remarks were made about their colleagues on the other side of the ocean.⁷ In the mid-1920s about 300 employees worked at the GE lab, and Whitney, probably more than Holst, was keen to protect his lab from company influences. Although the GE lab differed from the Philips Nat.Lab. in that respect, the outcome for both labs was that they went through the same broadening of scope as their companies did, and even in the same directions: from light bulbs to

X-ray tubes, radio and other communication devices. In both cases the laboratories served as a means for the company to realise its ambitions with respect to extending the product portfolio (we saw this for Philips in part I; for GE Buderer refers to the title of 'House of Magic' that was sometimes used to express the importance of the GE lab for its company). As for Philips, WWII meant a time of change into a new era for the GE Research Lab, but as in their early years, again we see differences in circumstances between the two laboratories. For Philips the changes were caused by the revision and formalisation of the company structure, resulting in the establishment of independent product divisions. As we saw in Part II, the Nat.Lab. responded to this by claiming a unique 'fundamental research' task for itself. For GE, however, WWII meant a strongly increased influence of government on the research programme and a stronger emphasis on war-related research topics.⁸ After WWII the government policy towards scientific research was formulated in the Vannevar Bush 'Science: The Endless Frontier' report, which for American companies such as GE had a more direct impact than for European companies such as Philips. After WWII the Cold War caused the American government to continue its influence on industrial research, whereas Philips in Europe did not experience this sort of influence. And again there is an analogy with the Philips Nat.Lab. in that the circumstances resulted in a great deal of freedom for the researchers in the 1950s and 1960s.⁹ The next change for both the Philips Nat.Lab. and the GE Lab is in the late 1960s and early 1970s, when a move towards PD-oriented research starts. In Europe the main cause for this is worsening of economic circumstances, together with the emergence of Japan as a competitor in several electronics markets. For GE the increasing Japanese and European competition are mentioned as main causes for the decreasing freedom that companies allowed their laboratories in that time.¹⁰ This too, seems to be an economy-related cause. As in the case of the Philips Nat.Lab., the end of this road was the introduction of contract research: from 1987 onward, the GE lab had to acquire three-quarters of its research budget from PDs by offering contracts.¹¹ Comparison of the history of the GE Lab and the Philips Nat.Lab. reveals similar changes in about the same periods, but under different circumstances and with partly different causes.

Like the Philips company, Siemens started as a family-owned company. In 1847 Werner Siemens, a German scientist and entrepreneur, started the company. His main interest was in telegraphy. He found a precision engineer, Johann Georg Halske, prepared to join him in the business and the company's name therefore became: Siemens & Halske. As had been the case for Philips, Siemens & Halske also broadened their product scope quickly. In 1903 a German company called Schuckert was taken over to become Siemens-Schuckertwerke, a company which built power stations,

turbines and other heavy equipment. Meanwhile, the Siemens & Halske mother company took up the production of X-ray tubes and light bulbs. These developments brought Siemens into a number of markets in which Philips also operated. Siemens' first major research laboratory was founded in 1905: the *Physikalisch-Chemisches Laboratorium*. A German chemist, Werner Bolton, was its first director. One of its first outputs was the invention of the tantalum filament lamp, which became a great market success. Bolton's successor, Hans Gerdien, proposed combining all research work in the company into one major lab, and this idea was realised in 1924, after a lot of delays due to WWI. This lab worked both for Siemens & Halske and for Siemens & Schuckertwerke. A great variety of research topics was covered in the lab's research programme, several of which were also present in the Philips *Natuurkundig Laboratorium* (e.g. acoustics, gas discharges, electrical devices). In 1935 Gustav Hertz was appointed head of an adjunct to the lab. This nephew of Heinrich Hertz had also been employed by the Philips Nat.Lab. (see Chapter 2). Another important scientist in the Siemens lab was Walther Schottky, who worked on semiconductors.¹² The position of the lab was not the same as in Philips. Unlike Philips, Siemens had many R&D activities outside the central lab. There was a whole series of specialised laboratories in the Siemens company, and the central lab therefore did not have the unique development role that the Philips Nat.Lab. had in the Philips company in the pre-WWII decades. Consequently its existence was much more debated within the company than the Philips Nat.Lab. had been. As in the case of Philips and GE, WWII was a period of transition for the research organisation, but again for a different reason. Germany lost this war and as a result the country was deprived of many of its resources. The Siemens company broke up its research organisation into three parts: Siemens & Halske built a new lab in Munich, in Southwest Germany, Siemens-Schuckertwerke built its lab in Erlangen, a town near Neurenberg, as well as Siemens-Reiniger-Werke, since 1924 the company's medical branch. Then a new period of expansion began, especially in the solid-state and semiconductor research field which lasted until 1969 when economic recession forced the company to combine its three main business parts into one company, Siemens AG. The Siemens & Halske and Siemens-Schuckertwerke laboratories were combined to become one main research department for the Siemens AG. As in the Philips Nat.Lab. the high degree of research freedom that had characterised the lab in the 1950s and 1960s was gradually replaced by a stronger influence from the product divisions.¹³ One of the research successes in the 1980s involved the work on microelectronics. In these years Siemens worked with Philips on the development and production of random access memories (RAMs). Whereas this MEGA project for Philips became a very problematic venture, Siemens was very successful in its choice for dynamic rather than

static RAMs. By 1984, Siemens was the only European company that produced these devices. This brief account of the history of Siemens research shows the same two points of change: WWII and the late 1960s. In particular for the first, again the circumstances were different from both Philips' and GE's circumstances. In the case of the second transition, there were clear similarities with Philips and GE: all these companies were hit by the economic recession in the late 1960s and early 1970s, and responded to this by initiating a shift towards more PD-oriented research. In the 1990s contract research was introduced in Siemens, as it had been in Philips some years before.

Comparisons with other industrial research labs are only possible in those cases for which the lab history has been described. That is the case only for a few major laboratories. For GE such a description exists. For Siemens we had to rely on a very concise study. Another company whose research history has been described extensively is Du Pont. Although this company belongs to a different industrial sector than Philips, the availability of the written history of Du Pont's labs makes it tempting to draw comparisons.

As in the case of GE, the chemical company of Du Pont, which had started in 1802 with black powder mill activities, also operated in the context of the American government antitrust policy, and for Du Pont too, this was an important motive for starting a corporate research organization.¹⁴ In 1902 the Eastern Laboratory was founded, and a year later the Experimental Station followed. The antitrust laws forced the company to diversify, and the research labs played a vital role in that.¹⁵ This is similar to the role that the Nat.Lab. had within Philips in the 1923-1946 period. The end of this period for Philips was marked by the formalization of a divisionalized company structure. At Du Pont this divisionalization already took place in 1921 and, as at Philips, raised questions about a new role for the research organization as each of the product divisions claimed and got their own research facilities.¹⁶ This resulted in a reduction of the central research labs, but soon they grew again to become a major know-how force in the company. As in the case of Philips, a problematic period started after WWII. Like other American companies, the war had an impact on the company in that a lot of work was undertaken for the government. In addition, Du Pont was again faced with antitrust indictments by the end of the 1940s. The company decided that growth through acquisition was no longer the appropriate strategy and made the central research organisation invest heavily in 'fundamental' research. As at Philips this would last for almost two decades.¹⁷ In this period the contact between the central research organization and the divisions was problematic, similar to the situation at Philips. In 1961 a bold programme of diversification, called

the New Venture Program, was launched to give a new impetus to diversification of Du Pont's product portfolio. Large-scale R&D programmes were set up in the context of this New Venture Program, but by the end of the 1960s it became evident that it had not resulted in successful new products. Capital shortages in the mid-1970s forced the company to be more selective in setting up R&D programmes, and this had consequences for the freedom that the central research organisation had had in the 1950s and 1960s. Gradually, the company's Executive Committee exerted more and more influence over the research programmes.¹⁸ As at Philips, the Du Pont company continued to have a corporate research organization, although of a much more modest size. By the 1990s, most laboratories worked for specific PDs.

Although a lot more study is necessary to make more systematic comparisons between the histories of the various industrial research laboratories, the brief comparisons described above show that, like Philips Research, similar research organisations in other companies have gone through changes in the balance between independent and PD-oriented research. The periods in which these changes took place are about the same for the labs that we have studied. The causes of change were partly different for the various laboratories.

Our survey shows the need to adjust a naive image of an industrial research lab as if it has always been an ivory tower, with academically oriented researchers in it, functioning as the first phase in a linear development process. Certainly, there were periods in the histories of these labs in which they tried to fulfil such a function, but then it became evident that such a function and such a perception of how innovation processes should work out resulted in many problems. For other periods the ivory tower metaphor is entirely inadequate as a description of the functioning of the lab in its company. The lab histories show that there were other ways for a lab to fulfil functions that are no less important than the (perhaps totally inappropriate) ideal of the ivory tower. Both internal and external factors have caused significant changes in the relationship between the research lab's activities and the corporate strategy.¹⁹

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Notes

Chapter 1

- 1 Chandler, A.D., Jr. (1990), *Scale and scope. The Dynamics of Industrial Capitalism*. Cambridge, MA: The Belknap Press of Harvard University Press, Part II.
- 2 The idea of using these characteristics for a historiography of the Nat.Lab. was suggested by Dr. I.J. Blanken during a presentation in Groningen, in 1999. So far, Blanken has written Volumes III, IV and V of the Philips company's history (the first two volumes were written by A. Heerding).
- 3 R. Szakonyi (1994), 'Measuring R&D Effectiveness', *Research Technology Management*, March-April 1994 (part I) and May-June 1994 (part II).
- 4 M. Weggeman (1992), *Leidinggeven aan professionals. Het verzilveren van creativiteit*. Deventer: Kluwer Bedrijfswetenschappen, p. 13. The foreword of this book was written by Dr. K. Bulthuis, at that time the General Manager of Philips Research. Part of Weggeman's book is based on the experience gained from advising at the Philips Nat.Lab.
- 5 The same two fields of tension were mentioned by C.C. Furnas in his chapter 'The Philosophy and Objectives of Research in Industry' in Furnas, C.C. (ed.) (1948), *Research in Industry*. New York: Van Nostrand, pp. 12 and 13.

Chapter 2

- 1 This chapter was written in dialogue with F. Kees Boersma.
- 2 This section is based on Blanken, I.J. (1992), *Geschiedenis van Philips Electronics N.V., Vol. III*. Leiden: Martinus Nijhoff, 1992, Chapter 1.
- 3 This section is based on Schot, J.W., Lintsen, H.W. and Rip, A. (1998), 'Inleiding: Technische ontwikkelingen in de twintigste eeuw', memorandum for the TIN-20 project, included in the reader for the *Fourth TIN-20 Workshop*, October 9, 1998 in Eindhoven, and Jonge, J.A. de (1978), 'Het economische leven in Nederland 1895-1914', in: *Algemene Geschiedenis der Nederlanden*, Vol. 13. Haarlem: Fibula-Van Dishoeck, pp. 249-284.
- 4 Hutter, H. (1986), 'Nederlandse laboratoria 1860-1940, een kwantitatief overzicht', *Tijdschrift voor de geschiedenis der geneeskunde, natuurwetenschappen, wiskunde en techniek*, Vol. 9, No. 4, pp. 150-174.
- 5 Vledder, I., Houwaart, E. and Homburg, E. (1999), 'Particuliere laboratoria in Nederland. Deel 1: Opkomst en bloei, 1865-1914', *NEHA-Jaarboek 1999*. Amsterdam: NEHA, pp. 249-290, in particular p. 254.
- 6 Homburg, E. (1986), 'De 'Tweede Industriële revolutie', een problematisch historisch concept', *Theoretische Geschiedenis*, Vol. 13, No. 3, pp. 367-385.
- 7 Reich, L.S. (1985), *The making of American industrial research: science and business at GE and Bell, 1876-1926*. Cambridge, MA: Cambridge University Press.
- 8 Carlson, W.B. (1997), 'Innovation and the Modern Corporation. From Heroic Invention to Industrial Science', in: Krige, J. and Pestre, D. (eds.), *Science in the Twentieth Century*. Amsterdam: Harwood Academic., Chapter 12.

- 9 Hughes, Th. (1989), *American Genesis: A Century of Invention and Technological Enthusiasm*. New York: Viking.
- 10 A discussion about the differences between Hughes' and Chandler's perspectives is presented by Hounshell, D.A. (1995), 'Hughian history of technology and Chandlerian business history: parallels, departures and critics', *History and Technology*, Vol. 12, pp. 205-224.
- 11 Lintsen, H.W. (1986), 'Kennisverwerving in de Nederlandse industrie in de 19e eeuw', *Tijdschrift voor de geschiedenis der geneeskunde, natuurwetenschappen, wiskunde en techniek*, Vol. 9, No. 4, pp. 175-189. About the same types are distinguished by C.E. Kenneth Mees in his 1920 book *The Organization of Industrial Scientific Research*. New York: McGraw-Hill, pp. 62, 63.
- 12 Letter G. Holst to A.D. Fokker, November 30, 1913, PCA. The English translation is quoted from Heerding (1988), *The history of N.V. Philips' Gloeilampenfabrieken*, Vol. 2. Cambridge: Cambridge University Press, p. 314.
- 13 Heerding (1988), p. 314.
- 14 Heerding (1988), p. 315.
- 15 Heerding (1988), p. 313, 317.
- 16 In the Proceedings of the Physics Department of the Royal Academy of Sciences of 1915.
- 17 Garratt, A.J. (1976), *The story of the Philips Laboratory at Eindhoven 1914-1946*, Vol. 1, p. 79, mentions this, but it is unclear from what sources he has derived this information.
- 18 According to a list that has been produced by B. van Gansewinkel (PCA); Garratt 1976, Vol. 1, p. 76, mentions 1915.
- 19 Garratt 1976, Vol. 1, p. 80, who refers to Holst's notebooks.
- 20 Patent no. 4122, Nederlandse Octrooiraad.
- 21 Interview with C. Bol, PCA.
- 22 Blanken 1992, p. 213, 214.
- 23 Koopmans came to the Nat.Lab. in 1916, but he did not stay long. Not enough was found out about his background to include him in this short analysis of the Nat.Lab. scientists' backgrounds.
- 24 A.E. Pannenberg (letter to H.E. van den Brom, dated December 22, 2002) explains Holst's decision to leave Leyden as follows. Holst's ETH diploma did not qualify him to teach at a Dutch university. Neither was it accepted as an admittance for acquiring a Ph.D. Therefore, Holst defended his thesis at the ETH in 1914, while earning his money in Leyden as Kamerlingh Onnes's assistant. According to Pannenberg, it is probable that Holst by that year had realised that his ETH background would make a career at Leyden University problematic, and the idea of becoming the director of his own laboratory at Philips must have seemed more attractive to him.
- 25 Casimir, H.B.G. (1983), *Haphazard Reality. Half a Century of Science*. New York: Harper & Row, Chapter 8.
- 26 B. van Gansewinkel, 'Intern Verslag 1989', PCA. Garratt also mentions the name of W. Koopman, but according to Van Gansewinkel's list, he had been appointed at the chemical lab (in 1912).
- 27 Memorandum 'Collectie aantekeningenboeken G. Holst', Ad Otten, August 25, 1989, PCA.
- 28 Blanken (1992), p. 219.
- 29 Blanken (1992), p. 249, 251.

Chapter 3

- 1 This chapter was written in dialogue with F. Kees Boersma.
- 2 Zanden, J.L. van and Griffiths, R.T. (1989), *Economische geschiedenis van Nederland in de 20e eeuw*. Utrecht: het Spektrum, p. 112.

- 3 Blanken (1992), p. 68.
- 4 Van Zanden and Griffiths (1989), p. 116.
- 5 Blanken, I.J. (1997), *Geschiedenis van Philips Electronics N.V., Vol. IV*. Zaltbommel: Europese Bibliotheek, p. 12.
- 6 Blanken (1997), p. 18.
- 7 Blanken (1992), p. 147.
- 8 Van Zanden and Griffiths (1989), p. 129.
- 9 Blanken (1997), p. 18.
- 10 Blanken (1997), p. 75.
- 11 Schot, Lintsen and Rip (1998), p. 16.
- 12 Throughout this section we will constantly come across new names of scientists who joined the lab. Sometimes some of the information about them has been included, mainly their disciplinary backgrounds. Their names have been added to the description to show that research is not only a matter of topics but also of people. This information was derived from the 'Lijst van doctoren en ingenieurs' (list of doctors and engineers), compiled in 1938 and to be found in the PCA.
- 13 Interview with C. Zwikker, PCA.
- 14 Blanken (1992), p. 217.
- 15 Blanken (1992), p. 249.
- 16 Blanken (1992), p. 228.
- 17 Tellegen was awarded a patent for the Pentode, the first of a series of about 57 patents that he received. Later his attention shifted to electrical networks and in 1952 he published an important paper on this topic in which he described 'Tellegen's Theorem', which gives a simple relationship between magnitudes that satisfy the Kirchhoff laws.
- 18 Groot, W. de (1951), 'Wetenschappelijk onderzoek bij de N.V. Philips' Gloeilampenfabrieken 1891-1951', *Philips Technical Review*, Vol. 13, No. 4/5, p. 99-143, see in particular p. 112.
- 19 Groot, W. de (1951), 'Wetenschappelijk onderzoek bij de N.V. Philips' Gloeilampenfabrieken 1891-1951', *Philips Technical Review*, Vol. 13, No. 4/5, p. 99-143, see in particular p. 113.
- 20 See the article by Zaalberg van Zelst in the *Philips Technical Review*, Vol. 9, No. 1, pp. 25-32.
- 21 See the article by Bouman in the *Philips Technical Review*, Vol.1, No. 3, pp. 76-81.
- 22 Letter sent by Holst to Gerard Philips, dated August 10, 1924, cited in Blanken 1992, p. 228.
- 23 Garratt (1976), Vol. 2, p. 63.
- 24 Blanken (1992) p. 357.
- 25 See for example Seitz, F. and Einspruch, NG. (1998), *Electronic genie: the tangled history of silicon*. Chicago: Illinois University Press.
- 26 Patent drawing in Garratt (1976), Vol. 2, p. 66. The work of Posthumus is mentioned in Seitz and Einspruch 1998, p. 114.
- 27 Tielen, J. (undated), *Analyse van de innovatie frequentiemodulatie (FM) van 1896 tot en met 1955*. Unpublished M.Sc. thesis, Eindhoven University of Technology.
- 28 Groot, W. de (1951), 'Wetenschappelijk onderzoek bij de N.V. Philips' Gloeilampenfabrieken 1891-1951', *Philips Technical Review*, Vol. 13, No. 4/5, p. 99-143, see in particular p. 116.
- 29 Blanken (1992), p. 343.
- 30 According to the 'List of doctors and engineers' given in the PCA, Strutt was a chemical engineer by education. The combination of chemistry and loudspeakers calculations is an unorthodox one. We must bear in mind, though, that the list is not free of errors (e.g. P.J. Bouma is erroneously listed as P.J. Bouman).
- 31 See the article by J.F. Schouten in the *Philips Technical Review*, Vol. 25, No 2, pp. 33-36.
- 32 Minutes of a meeting of directors and secretaries, March 6, 1928, referred to in Blanken (1992), p. 330.

- 33 Blanken (1992), p. 339.
- 34 Blanken (1992), p. 344.
- 35 Blanken (1992), p. 348.
- 36 Blanken (1992), p. 356.
- 37 *Philips Technical Review*, Vol. 3 (1938), nr. 10, pp. 298-305.
- 38 *Philips Technical Review*, Vol. 5 (1940), nr. 3, pp. 74-81.
- 39 Blanken (1997), p. 77.
- 40 Verheijen and Westmijze are not mentioned in the 'List of doctors and engineers' given in the PCA, so probably they were assistants. H.J. Vink, though (letter to M.J. de Vries, dated February 13, 2002), remembered having met Westmijze at the lab in 1942 as a graduated physicist from Leiden.
- 41 Garratt (1976), pp. 135-137.
- 42 Blanken (1997), p. 81.
- 43 Blanken (1997), p. 85.
- 44 In the 1970s a similar thing happened in the same field of recording: although the Video Long Play was largely a research project and failed commercially, the knowledge of (optical) recording that had been gained from the project was transferable to a more successful successor: the compact disc.
- 45 Groot, W. de (1951), 'Wetenschappelijk onderzoek bij de N.V. Philips' Gloeilampenfabrieken 1891-1951', *Philips Technical Review*, Vol. 13, No. 4/5, p. 99-143, see in particular p. 117.
- 46 Blanken (1997), p. 97.
- 47 Blanken (1992), p. 363; Garratt (1976), Vol. 1, p. 115 mentions a 'PTT group' that was located in the Nat.Lab. but which was not officially a part of it. It is not clear from his description whether this was a group of PTT people or a group of Philips people designated to work in co-operation with the PTT.
- 48 Solymar (1999), p. 257.
- 49 Blanken (1992), p. 331.
- 50 See the article by Rinia, Kleis and Van Tol in the *Philips Technical Review*, Vol. 10, No. 8, pp. 189-195.
- 51 Blanken (1992), p. 333, 334.
- 52 When Philips moved into radio the term 'systems' was not in common use as in Von Bertalanffy's 'General System Theory'. It meant little more than the sum of all the elements needed to broadcast and receive radio. In the period that is described in Part II of this report, a debate about the 'system theory' was continuing in the lab.
- 53 Groot, W. de (1951), 'Wetenschappelijk onderzoek bij de N.V. Philips' Gloeilampenfabrieken 1891-1951', *Philips Technical Review*, Vol. 13, No. 4/5, p. 99-143, see in particular p. 120.
- 54 In 1946 an article on this research was published in the *Philips Technical Review*, Vol. 8, No. 8, pp. 231-237.
- 55 This story was written by Garratt (1976), Vol. 2, p. 210, but it alas lacks further references.
- 56 See the article by H.J. Lemmens, M.J. Jansen and R. Loosjes in the *Philips Technical Review*, Vol. 11, No. 12, pp. 341-372.
- 57 Holst, G., Boer, J.H. de, Teves, M.C. and Veenemans, C.F. (1934), 'An apparatus for the transformation of light of long wavelength into light of short wavelength', *Physica*, Vol. 1, pp. 297-305.
- 58 See the article by Bruining in the *Philips Technical Review*, Vol. 3, No. 3, pp. 80-86.
- 59 According to the 'list of doctors and engineers' given in the PCA, all of them had studied mathematics and physics.
- 60 Blanken (1992), p. 335.
- 61 As a matter of fact, this was not necessarily an outcome of the 'Fremdkörper' character of this research.

- 62 Quoted from Blanken (1992), p. 263.
- 63 Proposals for the reorganisation of the physics laboratory, G. Holst, January 9, 1921, cited from Heerding (1988), p. 334, 335.
- 64 Heerding (1988), p. 283.
- 65 Garratt (1976), Vol. 1, p. 58.
- 66 Garratt (1976), Vol. 1, p. 113, refers to a remark made by Holst that was recorded in a document of June 21, 1922.
- 67 Garratt (1976), Vol. 1, p. 113, suggests the decision was taken between June and September of that year.
- 68 Garratt (1976), Vol. 1, p. 118.
- 69 Garratt (1976), Vol. 1, p. 127.
- 70 Interview with Jansen-Gratton, cited from Garratt (1976), p. 137.
- 71 Letter of appointment, dated April 22, 1926, and printed on page 160 of Garratt (1976); before that time there was no formal lab director yet.
- 72 Letter to the Personnel Department, November 21, 1934, cited from Garratt (1976), Vol. 1, p. 239.
- 73 Merton, R.K. (1942), 'The Normative Structure of Science', in: Storer, N.W. (1973), *The Sociology of Science*. Chicago: University of Chicago Press, pp. 267-278.
- 74 In that respect M.A. Dennis is correct in stating that the 'genotype' of Mertonian science did not exist in the industrial research labs. See: Dennis, M.A. (1987), 'Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science', *Social Studies of Science*, Vol. 17, pp. 479-518. It is equally questionable if it existed in university labs, as research in the sociology of science has shown.
- 75 In PCA-NL.
- 76 Lists of colloquia (dates, speakers and topics) in PCA-NL.
- 77 Berkel, K. van (1985), *In het voetspoor van Stevin. Geschiedenis van de natuurwetenschap in Nederland 1580-1940*. Amsterdam: Boom, p. 174.
- 78 Van Berkel (1985), p. 175.
- 79 Van Berkel (1985), p. 187.
- 80 Garratt (1976), Vol. 1, p. 167.
- 81 Van Berkel (1985), p. 206.
- 82 Heerding (1988), p. 333.
- 83 Heerding (1988), p. 283.
- 84 List in Garratt (1976), Vol. 1, p. 188. Probably the number of white cards for 1933 should be 350 rather than 150.
- 85 *Philips Technical Review*, Vol. 1, No. 1, Introduction.
- 86 Garratt (1976), Vol. 1, p. 231 mentions those.
- 87 Interviews with Mr. F. Philips on June 22, 1973 and F.H. Stieltjes in December 1972, PCA.
- 88 Memorandum 'Indeeling werkbesprekingsgroepen voor het jaar 1932 Natuurkundig laboratorium', dated December 12, 1931, PCA-NL.
- 89 This was the case for: Oosterhuis, Van der Pol, De Boer, and Van Arkel. The X-ray group is not included in this survey.
- 90 Picture in Garratt (1976), Vol. 1, p. 144.
- 91 Interview with Verff, cited from Garratt, Vol 1, p. 203.
- 92 Memorandum 'Bespreking betr. de organisatie der werkbesprekingen', dated January 14, 1936, PCA-NL.
- 93 Memorandum 'Betr. Werkbesprekingen', dated January 3, 1935, PCA-NL.
- 94 Blanken (1992), p. 330.
- 95 Blanken (1997), p. 22.
- 96 More details about them can be found in Blanken (1997).

- 97 Blanken (1992), p. 331.
- 98 Minutes Director's meeting September 16, 1929.
- 99 Minutes Director's meeting October 7, 1929.
- 100 This year is mentioned by Blanken (1997), p. 75. In a meeting minute dated July 6, 1953 (PCA) Van Walsem mentioned that the first meeting was held in 1931 and that the first minutes were written in 1932.
- 101 Blanken (1997), p. 75.
- 102 The January 9, 1933 Orco meeting was Holst's first meeting (minutes in PCA).
- 103 Minutes of the Orco meeting January 8, 1934, PCA.
- 104 Minutes of the Orco meeting June 3, 1960, PCA.
- 105 Memorandum P. v.d. Berg, J.A.J. Bouman, G. Oudemans, dated October 9, 1946, PCA.
- 106 Blanken (1997), p. 71.
- 107 Minutes of the Orco meeting September 4, 1934.
- 108 Minutes of the Lighting committee June 24, 1935 to January 13, 1937 are available in PCA-NL.
- 109 See e.g. the minutes of the Orco meeting held on March 5, 1934, PCA. Holst did, though, foresee possibilities in Germany.
- 110 Blanken (1997), p. 87.
- 111 Memorandum P.R. Dijksterhuis, May 9, 1935.
- 112 Blanken (1997), p. 96.
- 113 Minutes of the Orco meeting May 14, 1934, PCA.
- 114 Television would have some negative consequences for the company, but totally differently than Holst had predicted: once television was introduced, the sales of radio decreased, and in 1938 the company had to carry out a special campaign to promote the sales of radio. See Ende, J. van den, Ravesteijn, W. and Wit, D. de (1997/8), 'Shaping the Early Development of Television', *IEEE Technology and Society Magazine*, Vol. 16, No. 4, p. 23.
- 115 Blanken (1997), p. 93, 94.
- 116 Cited from Garratt (1976), Vol. 1, p. 102, 103.
- 117 Appendix 17 to minutes of the company's Directorate meeting of June 30, 1936, PCA.
- 118 Minutes of a meeting held in Schoonenberg's room, November 12, 1935, PCA-NL.
- 119 Memorandum 'Enkele Onderdelen Ontwikkeling vanaf 1928' by Ir. C de Lange, dated February 23, 1966. The author wants to thank Mr. A.R. Bos and Mrs. M.R. Bos-Prins, who gave him access to the personal archive of Dr. J.G. Bos in which this memorandum was found.
- 120 In the memorandum the name is spelled Klaassens, but this can only possibly refer to Dr. A. Claassens.
- 121 Memorandum dated December 4, 1933 by J.A.J. Bouman and B.D.H. Tellegen, PCA-NL.
- 122 Minutes of the Orco meeting January 22, 1934.
- 123 Minutes of the Laboratory meeting January 6, 1939, PCA. The minutes do not indicate which factory Hardenberg was from.
- 124 Garratt (1976), Vol. 1, p. 174.
- 125 Cited from Garratt (1976), Vol. 1, p. 177.
- 126 Letter in PCA-NL.
- 127 Letter De Fremery to Otten dated July 23, 1936, PCA-NL.

Chapter 4

- 1 This chapter has been written by F. Kees Boersma and Marc J. de Vries.
- 2 The data for this section have been derived from Hutter, H. (1988), *Toepassingsgericht onder-*

- zoek in de industrie. *De ontwikkeling van kwikdamplampen bij Philips 1900-1940*. Eindhoven: Technische Universiteit.
- 3 Hutter (1988), p. 123.
 - 4 Heerding (1988), *The history of N.V. Philips' Gloeilampenfabrieken, Vol. 2*. Cambridge: Cambridge University Press, p. 183.
 - 5 Heerding (1988), p. 333.
 - 6 Elenbaas, W., Kwikhuizen, Nat.Lab. verslag 854, December 21, 1933, PCA-NL.
 - 7 Hutter (1988), p. 223.
 - 8 The reasons for this are not clear, but maybe it has to do with the fact that the key players in this field left the Nat.Lab. The reason is certainly not a fading interest of the company in the gas discharge lamps, as they remain an important product after WWII.
 - 9 This section is a translated and edited version of: Boersma, F.K. (1999), 'De ontwikkeling van röntgentechnologie in de beginjaren van het Philips Natuurkundig Laboratorium'. *NEHA Jaarboek 1999*, pp. 291-318. References can be found there.
 - 10 Reich, L.S. (1985), *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926*. Cambridge: Cambridge University Press, Chapter 4.
 - 11 Blanken (1992), p. 253.
 - 12 Most data for this case study have been derived from Hoitzing, A. (1992), *Ferrietonderzoek op het Philips Natuurkundig Laboratorium. Materiaalonderzoek zonder vaste-stoffysica 1933-1950*.
 - 13 Reports Magnetic materials, PCA-NL.
 - 14 Reports Magnetic materials, survey 1935, PCA-NL.
 - 15 Interview with Druyvesteyin, done by Bruining, Spring 1973, PCA.
 - 16 Magnetic materials, Survey November 1943, PCA-NL.
 - 17 Interview with H.B.G. Casimir on February 3, 1988.
 - 18 Casimir, H.B.G. (1983), *Haphazard Reality – Half a Century of Physics*. New York: Harper & Row Publ., p. 237.

Intermezzo I

- 1 Blanken (1997), p. III. Apart from material that is cited in other footnotes the data for this section have been derived from this source.
- 2 Garratt (1976), Vol. 1, p. 258 has a list, but no reference is made to archive material.
- 3 Blanken (1997), p. 109.
- 4 The Dutch term for 'Verwaltung' was: 'onderbeheerstelling' (i.e. 'placing under control').
- 5 Blanken (1997), p. 266.
- 6 Interview with Dr. H. Vink on January 7, 1998. The interview transcript can be found in a special archive created for the historical research that led to this book. When the book was finished, this archive was kept at the Corporate Research Bureau of Philips Research, on the Nat.Lab. premises. From now on, any archive documents not quoted as part of the Philips Company Archives (PCA) in the notes are to be found in that special archive.
- 7 Interview with Dr. H.B.G. Casimir on February 3, 1998. In 'Haphazard reality' Casimir explains the relationship: Köhler was married to Casimir's sister, and Verwey was married to his wife's sister.
- 8 Interview with Dr. H.A. Klasens on January 9, 1988.
- 9 Interview with Verff cited in Garratt, Vol. 1, p. 309.
- 10 The plan is reproduced in Garratt, Vol. 1, p. 310.
- 11 Interview with Dr. H. Vink on January 7, 1998, who talked about his unexpected confrontation with the so-called 'acid oxides', that appeared to relate to one of his own fake reports.

- 12 Memorandum 'Het onderzoek in het Natuurkundig Laboratorium tijdens de bezetting', dated September 13, 1945, PCA-NL.
- 13 Garratt (1976), Vol. 2, p. 197.
- 14 Garratt (1976), Vol. 1, pp. 317-321 has the full text of Holst's presentation.
- 15 Of these people only Slooff was not a Nat.Lab. employee (he was a pediatrician), according to Garratt, Vo. 1, p. 325.
- 16 Hargreaves, C.M. (1989), 'H.B.G. Casimir's teachings in Leiden and Eindhoven', in: Sarlemijn, A. and Sparnaay, M.J. (eds.), *Physics in the making. Essays on the development in 20th century physics*. Amsterdam: North Holland, pp. 341-361.
- 17 Blanken (1997), p. 364.
- 18 Blanken (1997), p. 357.
- 19 In Dutch the name *Hoofdingstrieegroep* was used, which literally means Main Industry Groups; in much archive material we find this term used in English.
- 20 Blanken (1997), p. 357.
- 21 Royen, E.J.G. van (1991), *Philips en zijn toeleveranciers*, p. 169, provides a survey of the names of the PDs during the 1946-1991 period.
- 22 Blanken (1997), p. 359.
- 23 Dennis, M.A. (1997), 'Historiography of science: An American Perspective', in: Krige, J. and Pestre, D. (eds.), *Science in the Twentieth Century*. Amsterdam: Harwood Academic Publishers, p. 7.
- 24 Sparnaay, M.J. (1989), 'The historical background of the Casimir effect', in: Sarlemijn, A. and Sparnaay, M.J. (eds.), *Physics in the making. Essays on the development in 20th century physics*. Amsterdam: North Holland, pp. 235-246.

Chapter 5

- 1 Zanden, J.L. van and Griffiths, R.T. (1989), *Economische geschiedenis van Nederland in de 20e eeuw*. Utrecht: het Spektrum, p. 210.
- 2 Schot, J.W., Lintsen, H.W. and Rip, A. (1998), 'Inleiding: Technische ontwikkelingen in de twintigste eeuw', memorandum for the TIN-20 project, included in the reader for the *Fourth TIN-20 Workshop*, October 9, 1998 in Eindhoven.
- 3 Vannevar Bush (1945), *Science, the Endless Frontier*. Washington DC: United States Government Printing Office.
- 4 Riordan, M. and Hoddeson, L.E.G. (1997), *Crystal fire. The birth of the information age*. New York/London: W.W. Norton & Corporate.
- 5 Dickson, D. (1984), *The new politics of science*. New York: Pantheon Books, p. 7.
- 6 In Holst (1946), 'De beoefening der Natuurkunde en de toekomst van ons land', *Nederlands Tijdschrift voor Natuurkunde*, Vol. 12, pp. 123-136.
- 7 Blanken (1997), p. 372.
- 8 Minutes of the first meeting on 'Onze laboratoriumplannen' ('Our laboratory plans'), January 23, 1947, PCA-NL.
- 9 In Section 6.5 we will assess the use of the term 'fundamental' against the practice of research in the period 1946-1972 and conclude that it seems that the term was used for justifying an independent role of the lab within the company rather than for making decisions about the content of the research programme.
- 10 All Minutes in PCA-NL.
- 11 Minutes of the first meeting of the Comité for Technisch Beleid (CTB) (undated, but most probably in 1947), PCA-NL.
- 12 The name that is used in the written documents is Concern Research Conferences, but as

- the word Concern is not appropriate in English and Philips has replaced it now by the word Corporate, the term Corporate will be used in this text.
- 13 The main reason for starting these meetings and the Research Directors Conferences (RDCs) was the fact that foreign labs were added to the research organisation. These will be described later on in this section.
 - 14 In 1950 a similar meeting was held but it was totally dedicated to television and this is probably why it is not counted as a CRC.
 - 15 The RDCs were meetings of the international research managing directors. Members of the company's Board of Management and occasionally PD-representatives also participated in the CRC meetings.
 - 16 In the Minutes of the 1964 CRC we find the term 'product-oriented' research as opposed to 'fundamental' research.
 - 17 An example of this is the field of industrial control, where Casimir expected research output to lead to new commercial activities. See the Minutes of the 8th CRC from 1964.
 - 18 The report by R. van Beek is mentioned in the Minutes of the Directors contact meeting Nat.Lab.-RGT, April 22, 1970, PCA-NL.
 - 19 Letter G.T. de Kruijff (PIT) to H.J.G. Meijer (Nat.Lab.), dated October 31, 1968, PCA-NL.
 - 20 Minutes Directors' contact meeting Nat.Lab.-Elcoma, October 10, 1968, PCA-NL.
 - 21 See the article by A.H. Gomes de Mesquita in the *Philips Technical Review*, Vol. 30 (1969), No. 2, pp. 36-47.
 - 22 Haanstra, H.B. and Knippenberg, W.F. (1965), 'Crystal growth of silicon carbide (II)', *Philips Technical Review*, Vol. 26, No. 7, pp. 187-189.
 - 23 Interview with Dr. Knippenberg on April 1, 1997.
 - 24 According to Knippenberg (interview on April 1, 1997), the Lighting and Elcoma PDs expressed some interest, but it is not clear if there was a real transference of the research output to those PDs.
 - 25 Minutes of the Research and Development Co-operation Policy Convention, held in Eindhoven from June 28th – July 3rd, 1948. This shows that research in the field of facsimile was already at a stage in which a concrete product was perceived, thus making it different from stereophony research.
 - 26 Minutes of the 6th CRC of 1960.
 - 27 Minutes of the 7th CRC of 1962.
 - 28 The co-operation already existed before 1949.
 - 29 J.M. Waalwijk and N. Wiedenhof in the *Philips Technical Review*, Vol. 39 (1980), No. 11, pp. 286-289.
 - 30 Remark made by Casimir in the Minutes of the 4th CRC of 1956.
 - 31 Minutes of the 3rd CRC of 1954.
 - 32 Memorandum written by Casimir to the company's Board of Management, dated March 3, 1966, PCA-NL.
 - 33 Minutes of the 7th RC of 1962.
 - 34 Memorandum 'Beschouwingen over het wetenschappelijk werken in het Natuurkundig Laboratorium' (Considerations on the scientific work continued in the Physics laboratory; in Dutch) by Prof. H.B.G. Casimir.
 - 35 Minutes of the 11th CRC of 1970.
 - 36 In the Minutes of the 9th CRC of 1966, Tromp explicitly expressed this as the opinion of the Board of Management.
 - 37 In the minutes of the 10th directors' contact meeting with the PD PIT, there is a discussion about the Nat.Lab.'s support for developing ICs for the PD. It was agreed that this support should end with the first series of prototypes and then the PD should take over the initiative. This is an example of the Nat.Lab.'s view of its development task as mentioned in the text.

- 38 See the exchange of letters between Casimir and Kistemaker (FOM) from February and March 1969, PCA-NL. Casimir's remark was a response to Kistemaker's proposal for a joint institute for medical research and development.
- 39 Dr. A.E. Pannenburg (letter to M.J. de Vries, dated April 15, 2002) mentions the case of a solution for the problem of minimising third-degree aberration errors in television tubes that was developed at the Nat.Lab. and could only be transferred to the PD RGT by transferring Nat.Lab. researcher Kaashoek to the PD.
- 40 This feeling was expressed e.g. by H.J.G. Meijer with respect to the index tube (interview of September 16, 1997), and by R.J. Meijer with respect to the Stirling engine (interview August 4, 1998).
- 41 Pickup tubes can be found in a television studio camera. They realise the transition from the optical image to a video signal. The case study of the Plumbicon in the next chapter deals with these sorts of tubes.
- 42 Minutes of the Research and Development Co-operation Policy Convention, held in Eindhoven from June 28th – July 3rd, 1948.
- 43 Minutes of the 4th CRC of 1956. Already in a series of articles on the X-ray image convertor published in 1955 in the *Philips Technical Review* the trend towards systems-oriented research can be seen: the focus was on the way this X-ray image convertor was to be embedded in an X-ray system, rather than on the convertor itself.
- 44 F.L.J. Sangster in the *Philips Technical Review*, Vol. 31 (1970), No. 4, pp. 97-110, describes this device. The case studies on optical communication and on AD/DA conversion in Part III will give further information about this research field.
- 45 In 1950 L. von Bertalanffy published an article entitled 'An outline of General System Theory' in a philosophical journal. This marked a breakthrough in the debate. Strijbos, S. (1988), *Het technische wereldbeeld*. Amsterdam, Buijten & Schipperheijn, p. 34.
- 46 Interview with Dr. H.B.G. Casimir of February 3, 1998, and with Dr. K. Teer of September 9, 1997.
- 47 We find this general remark with respect to e.g. the PD Radio, Grammofoon en Televisie (RGT), see the minutes of the Directors' contact meeting Nat.Lab.-RGT, April 22, 1970, PCA-NL. At the same time, frustration is uttered because the PD was not interested in listening to the 'background information' that the Nat.Lab. offered. Evidently providing such background information, and not just doing some 'trouble-shooting', was seen by the Nat.Lab. as its task.
- 48 1946, 1954 and 1968 have been selected as reference years, mainly for reasons of data availability and to get a reasonable representation of the period.
- 49 Philips Annual Report of 1960.
- 50 Memorandum 'Research-activiteit 1946-1955' (undated). The research topics have been renamed and arranged according to later research programmes presentations (e.g. the 1954 and 1968 survey). This was done to enable comparison. The terms 'Materials', 'Devices' and 'Systems' for the three main research areas were not yet used at that time.
- 51 This is the first overall presentation of the research programme according to the main research fields. The Memorandum 'Research-activiteit 1946-1955' only contained a survey according to the names of the three directors, not according to the main research fields. The next presentation of this kind that was found was for 1968.
- 52 This can be seen from the turnovers reported in the Annual Reports in the 1960s.
- 53 There was no direct relationship between the three fields and the three directors (interview with H.B.G. Casimir of February 3, 1988). Each director could incorporate elements from each of the three fields.
- 54 For this, data on numbers of scientists in the research programmes have been used.
- 55 Blanken (1997), p. 347.

- 56 Philips Laboratories Briarcliff Manor, N.Y. Undated brochure, s.l.
- 57 Research and Philips. Brochure, s.l., 1967.
- 58 Interview with Dr. A.E. Pannenburg on January 5, 1998.
- 59 Memorandum van een bespreking ter bepaling van het verband tusschen het Eindhovensc Nat.Lab. en de verschillende in andere landen gevestigde of nog te vestigen research-laboratoria, op December 3, 1946 (Memorandum of a discussion to determine the relationships between the Nat.Lab. in Eindhoven and the other research laboratories established, or still to be established, in other countries).
- 60 Minutes of 2nd CRC of 1952.
- 61 Minutes of the Research and Development Cooperation Convention, held in Eindhoven from June 28th – July 3rd, 1948.
- 62 Blanken (1998), p. 351.
- 63 Memorandum December 3, 1946.
- 64 See the article by Ducot in a special 1969 issue of the *Philips Technical Review* (Vol. 30, No. 8/9/10), pp. 213-224.
- 65 Interview with Dr. A.E. Pannenburg on January 5, 1998; besides this problem Boutry also displayed recalcitrant behaviour on some occasions in his contacts with the Eindhoven management, as was told by Dr. E.F. de Haan on September 9, 1997, and by Dr. H.J. Vink on January 7, 1998, during interviews with them.
- 66 Interview with Dr. A.E. Pannenburg on January 5, 1998.
- 67 Memorandum ‘Buitenlandse researchlaboratoria’ by J.H. van Santen, dated 1.12.71.
- 68 Interview with Dr. A.E. Pannenburg on January 5, 1998.
- 69 Blanken (1992), p. 104.
- 70 The data that follow now in the text have been derived from a 1967 brochure ‘Philips and Research’.
- 71 The source of this information does not mention the number of people. Memorandum ‘The growth of research’. Data supplied by Mr. K. Woensdregt (Appendix to first RDC meeting of 1965), dated May 2, 1965.
- 72 Minutes of the 2nd CRC of 1952.
- 73 Communication No. 87/55 from the Board of Management, 24.2.1955.
- 74 Minutes of 8th CRC, 1964.
- 75 Memorandum ‘Buitenlandse researchlaboratoria’ by J.H. van Santen, dated 1.12.71.
- 76 Interview with Dr. S. van Houten of November 19, 1997
- 77 Letter by A.E. Pannenburg to M.J. de Vries, dated March 18, 2002.
- 78 Interview with Dr. S. van Houten dated November 19, 1997
- 79 Data have been derived from the memorandum ‘Ten years of concern research’ by K. Woensdrecht, dated May 2, 1965 (appendix to the agenda of the RDC meeting of May 6 and 7, 1965). The patent income was larger than was indicated by these numbers, but part of that was refunded to the PDs.
- 80 Interview with Dr. H.B.G. Casimir on February 3, 1998, and Rathenau, G.W. (1974), ‘Met z’n allen research doen voor de industrie’, *Nederlands Tijdschrift voor Natuurkunde*, Vol. 40, Nr. 13, p. V.
- 81 Memorandum ‘Verhoudingsgetal academici en overig personeel’, by H. Ritmeester, dated January 19, 1970.
- 82 This term was used in the memorandum quoted on the previous note. The term was used to indicate that the scientists were the main instigators of research work, while the assistants only carried out tasks that were defined by the scientists.
- 83 Schippers, H. (1989), *Van tusschenlieden tot ingenieurs. De geschiedenis van het Hoger Technisch Onderwijs in Nederland*. Hilversum: Uitgeverij Verloren, p. 127.
- 84 See e.g. the interview with Prof. D. Broer on January 12, 1998.

- 85 The WA building was completed in 1960 and had been used before 1963 to field-test the modular approach for organizing the buildings.
- 86 Minutes Directors-CoCo meeting of November 22, 1963.
- 87 This bus service continued to function for a long time, probably even until the early 1980s.
- 88 See the article by M.J. Jansen Gratton in the *Philips Technical Review*, Vol. 24 (1963), No. 11/12, pp. 385-395.
- 89 See the article by L.A. de Haas and S.S. Wadman in the *Philips Technical Review*, Vol. 31 (1970), Vol. 5/6, pp. 153-157. The article was written on the occasion of the opening of a new building, WY, in 1968.
- 90 The October 1963 issue of the *Philips Technical Review* was a special issue published on the occasion of the symposium.
- 91 Minutes Directors-CoCo meeting of December 19, 1962.
- 92 Minutes of the 5th CRC in 1958.
- 93 See the article by W. Nijenhuis in the *Philips Technical Review* of April 1961. To an article in the *Philips Technical Review* of October 1962 a 45 rpm record was added that contained sounds of the PASCAL during calculations. On one side of this record, the scientists had even got the PASCAL to produce a Mozart melody!
- 94 The full name was: Snel Tel En Vermenigvuldings INSTRUMENT (Fast Counting and Multiplication Instrument).
- 95 Data have been derived from a computer database that was set up especially for this historical research project from the collection of 'yellow cards' kept in the Nat.Lab. personnel department.
- 96 Tijdsopname. De Technische Hogeschool Eindhoven 1956-1966. Eindhoven: Stichting Eindhoven Hogeschoolfonds, 1967.
- 97 Data have been derived from the same computer database that was mentioned in note 91.
- 98 Interview with Dr. Valkenburg on January 13, 1998.
- 99 Minutes 1st Directors-CoCo meeting of August 10, 1945.
- 100 In the company the Kern had already existed since 1922. It was probably only after WWII that minutes of specific Nat.Lab. Kern meetings were kept.
- 101 E.g. Minutes of the Directors-CoCo meeting of February 16, 1962, December 12, 1962, and May 20, 1963.
- 102 Minutes of the 7th CRC in 1962.
- 103 Stumpers, F.L. (1966), 'Ir. H. Rinia neemt afscheid', *Tijdschrift van het Nederlands Elektronica- en Radiogenootschap*, Vol. 31, No. 12, p. 247.
- 104 Minutes of the Kern meeting of July 3, 1969.
- 105 Today the connection is still not there, which many people now regret.
- 106 Interviews with Dr. E.F. de Haan on September 9, 1997, and with Dr. H.J.G. Meijer on September 16, 1997.
- 107 Memorandum 'Enkele opmerkingen n.a.v. het door Ir. P.M. van den Avoort verspreide verslag d.d. 7-10-1971' by J. Hasker, dated October 12, 1971.
- 108 Memorandum 'Korte samenvatting punten bespreking d.d. 25.11.1971 betreffende index', dated December 6, 1971.
- 109 Memorandum 'Indexbuis' by Heessels, De Jong, Kaper and Tielens, December 1971.
- 110 Interview with Mr. B. Kaper on March 3, 1998.
- 111 Here too initially the term Concern was used instead of Corporate.
- 112 In fact the need to have such a representative on the Board of Management had already been identified in 1951, according to the Minutes of a meeting between the Board and the Nat.Lab. directors, dated January 23, 1951, PCA-NL.
- 113 Casimir, H.B.G. (1983), *Haphazard Reality. Half a Century of Physics*. New York: Harper & Row, p. 244.

- 114 Only in the Minutes of the Quo Vadis meeting on radar of January 27, 1954, do we find the remark that Holland Signaal, a military branch of the company, was allowed to talk first, 'which deviates from the normal practice of first giving the word to the Nat.Lab.', PCA-NL.
- 115 Minutes of the Quo Vadis meeting PD Lighting, dated June 30, 1953, PCA-NL.
- 116 Minutes of Quo Vadis meeting PD Icoma, dated June 22, 1955, PCA-NL.
- 117 Minutes of the June 14, 1963 Quo Vadis meeting, PCA-NL.
- 118 The data refer to the years 1965-1970. Minutes of the meetings only from those years have been found.
- 119 This was, of course, before the painful meetings that led to the abolishment of the index tube activities in the PD.
- 120 Minutes of the Directors' contact meeting Nat.Lab.-RGT, November 28, 1968, PCA-NL.
- 121 Minutes of the Directors' contact meeting Nat.Lab.-RGT, October 20, 1970, PCA-NL.
- 122 Minutes of the Directors' contact meeting Nat.Lab.-RGT, December 24, 1971, PCA-NL.
- 123 Minutes of the Directors' contact meeting of August 8, 1968, PCA-NL.
- 124 At a meeting on October 26, 1965, the PD passed on to Pannenberg a number of specific requests for research support. Pannenberg promised to discuss those with the Nat.Lab. scientists. In the minutes of the August 27, 1968 meeting we find that the PD asked the Nat.Lab. for advice on pattern recognition. In the minutes of June 9, 1970, we again find a list of specific research requests by the PD.
- 125 Minutes of the Directors' contact meeting of October 26, 1968, PCA-NL.
- 126 Minutes of the Directors' contact meeting of May 17, 1967, PCA-NL. As this was said with respect to PIT, and not Elcoma, Pannenberg must have referred to applications of semi-conductors and ferrites.
- 127 Minutes of the Directors' contact meeting of January 8, 1969, PCA-NL.
- 128 Letter dated February 26, 1971, PCA-NL.
- 129 Minutes of the Directors' contact meeting of October 26, 1971, PCA-NL.
- 130 Minutes of the Directors' contact meeting of June 28, 1972, PCA-NL.
- 131 Minutes of the Directors' contact meeting of September 18, 1969, PCA-NL.
- 132 In fact, Van Santen's complaint was weakened even further by the fact that in 1975 there was an Elcoma Bubble team (internal memorandum by H.P.J. Wijjn to H.J. Vink, dated July 29, 1975, PCA-NL), so apparently Elcoma did in the end take over the bubble idea.
- 133 Interview with Dr. K. Teer on September 9, 1997.
- 134 Minutes of the Directors' contact meeting of June 28, 1966, PCA-NL.
- 135 Minutes of the Directors' contact meeting of December 15, 1966, PCA-NL.
- 136 Letter written by A.M. Eben and W.K. Westmijze (Elcoma) to Teer (Nat.Lab.), 1968, PCA-NL.
- 137 Letter written by Van Gijn (Elcoma) to Klasens (Nat.Lab.), August 12, 1968, PCA-NL.
- 138 Minutes of the Directors' contact meeting of May 8, 1968, PCA-NL.
- 139 Minutes of the Directors' contact meeting of October 31, 1967, PCA-NL.
- 140 Minutes June 5, 1969, PCA-NL.
- 141 Minutes August 14, 1968, PCA-NL.
- 142 Survey of co-operation November 1970, PCA-NL.
- 143 Minutes of a meeting between Verwey and Vink (Nat.Lab. with Klein and Keverling Buisman (Duphar) on September 23, 1965, PCA-NL.
- 144 Memorandum J.A. Kuiper (Duphar) to H.J. Vink (Nat.Lab.), dated December 11, 1968, PCA-NL.
- 145 Letter by E.J. Verwey to H.J. Vink, dated November 14, 1970.
- 146 Interviews with Dr. Teer on September 9, 1997, with Dr. De Haan on September 9, 1997, and with Dr. H.J.G. Meyer on September 16, 1997. Casimir himself (interview February 3, 1998) could not recall any specific memories of the Directors' contact meetings, which probably means that he was not very involved in those meetings.

- 147 Or actually: Concern.
- 148 Minutes of the 1st CRC of 1948.
- 149 Minutes of the 8th CRC of 1964.
- 150 This number was mentioned during the 1st CRC in 1948.
- 151 Memorandum 'Verloop Academici Nat.Lab. 1.1.1946-1.7.1955', dated August 30, 1955.
- 152 Data have been derived from the same computer database that was mentioned in note 91.
- 153 In 1955 6 scientists were for example transferred to other parts of Philips, out of a population of 245 scientists. In 1950 this was 11 out of 171. The number of people transferred decreased in the years 1946-1955, according to the Memorandum 'Verloop Academici Nat.Lab. 1.1.1946-1.7.1955, dated August 30, 1955. Another memorandum, dated October 5, 1967, shows that the number of transferred assistants in those years was 18 out of 249 in 1950 and 17 out of 366 in 1955.
- 154 'Enquête Hoger Personeel 1969. De resultaten van het Natuurkundig Laboratorium'. Undated report Philips Research Laboratories.
- 155 Report of the Efficiency Contact Committee 1962/1963, PCA.
- 156 Evidently, the authors of the report presumed that the distribution of research efforts over various fields should be the same as the division of the total company turnover over the PDs. The correctness of this can of course be questioned. One could also defend the thesis that research effort should be large when turnover is still small in the hope that research output might increase productivity.
- 157 Interview with Dr. Knippenberg of April 1, 1997.
- 158 Memorandum 'Contacts Product Divisions-Research' written by Mr. De Kruijff for the interview of March 2, 1998.
- 159 Interview with Dr. J. van Nieuwland on November 20, 1997.
- 160 Interview Dr. P. Kramer on November 5, 1997.
- 161 Interview with Mr. L. Ottens on March 5, 1998.
- 162 Philips Annual Report 1958.
- 163 Interview with Mr. P. Broerse of March 10, 1998.
- 164 The 1970 brochure 'Facts about Philips' contains on page 19 a 'family tree' of Philips' products over the course of time (1891-1967). For most of the product 'leaves' related to the years 1914-1950 there had been Nat.Lab. involvement, but for many 'leaves' relating to later years there had been no such involvement.
- 165 Minutes of the 2nd CRC in 1952 (remark made by Casimir).
- 166 Minutes of the 9th CRC in 1966 (remark about the PDs' lack of activity in displays and recognition techniques for industrial control).
- 167 E.g. Minutes of the 2nd CRC in 1952.
- 168 E.g. in the field of computers, as mentioned in the minutes of the 8th CRC in 1964. In view of the fact that the Nat.Lab. also saw themselves as the source of the PD's long-term strategy, this is a strange remark.
- 169 Minutes of the 10th CRC in 1968 (remark about development of new light sources). A similar remark was made at the January 1968 RDC meeting.

Chapter 6

- 1 Buchanan (1984), in: *Energy in history*, ICOHTEC, Düsseldorf.
- 2 Van de Goor (1995) (M.A. thesis, State University of Limburg).
- 3 Interview on March 2, (1998).
- 4 Hargreaves, C.M. (1991), *The Philips Stirling Engine*. Amsterdam: Elsevier, p. 404.
- 5 Interview with Dr. H.J.G. Meijer on August 3, 1998.
- 6 Hargreaves, p. 30.

- 7 Hargreaves, p. 31.
- 8 Hargreaves, p. 32.
- 9 Report of Laboratory Meeting of November 15, 1940, PCA.
- 10 Hargreaves, p. 39.
- 11 Finkelstein, *The Engineer* December 12, 1947, p. 550.
- 12 Report concerning the hot air engine October 2, 1945, PCA.
- 13 Development Hot Air Engines for US Navy (report, November 30, 1945), PCA-NL. None of these areas had a clear relationship with the rest of the research programme.
- 14 Report 'Discussions activities in the engine field', Thursday, June 20, 1946, PCA-NL.
- 15 Technical Report February/March 1947, PCA-NL.
- 16 The Stirling cycle cannot only be used as an engine, but also as a refrigeration device. In that case the configuration is driven by an engine, and heat is pumped from one place to another. This is the reverse of the engine process: rather than converting heat into motion, motion is converted into heat (or cold). This application has been exploited parallel to the engine application in Philips, and for many years yielded small profits. Later a separate daughter company was founded to continue production of the Stirling-based refrigeration equipment.
- 17 Orco Meeting, April 18, 1945.
- 18 Orco Meeting on Hot Air Engines, March 23, 1945, PCA-NL.
- 19 Personnel Survey January 1948, PCA-NL.
- 20 Report on the combined meeting between the Engine Council and the Board of Management on January 6, 1947, PCA.
- 21 Confidential announcement made by Van Hornstra (CV&P), 15 January 1948, PCA.
- 22 Report concerning the characteristics and applications of hot air engines, May 14, 1949, PCA-NL.
- 23 Annual Report of the PD December 11, 1953, PCA.
- 24 R.J. Meijer, *The Philips Stirling Thermal Engine. Analysis of the Rhombic Drive mechanism and efficiency measurements*. Delft Polytechnic 1960.
- 25 Travel report by J.W.L. Köhler and R.J. Meijer, February 18, 1957, PCA.
- 26 Minutes Board of Management, February 27, 1958, appendix A (J.A.L. IJzer), PCA.
- 27 Minutes Corporate Orco, July 5, 1966, PCA.
- 28 Minutes Corporate Orco, September 2, 1958, PCA.
- 29 Minutes Corporate Orco, October 26, 1965, PCA.
- 30 Message from P.C. Breek to the Board of Management on December 13, 1964, PCA.
- 31 Minutes of the Corporate Research Conference, June 1968.
- 32 Letter from F. Seibezeder to J.J. Spijkerman, July 12, 1955, Minutes of Corporate Orco, July 19, 1955, and Situation report Stirling engines by P.J. Hagendoorn, January 6, 1971, all PCA.
- 33 Minutes Corporate Orco, October 27, 1964, PCA.
- 34 Report Feb-March 1961 (L. de Lange to Rinia, Köhler and Meijer), PCA.
- 35 Interview with Dr. R.J. Meijer on August 3, 1998.
- 36 Hargreaves, p. 404.
- 37 Meeting of Orco Stirling Engines, May 26, 1970, PCA.
- 38 Kern report January 25, 1970.
- 39 Letter from G.W. Rathenau to A.E. Pannenberg, April 21, 1970, PCA.
- 40 Report Orco Stirling Engines January 7, 1972, PCA.
- 41 Progress Report Stirling Engines, March-May 1970 (H.J.C. van Beukering), PCA-NL.
- 42 Minutes Orco Stirling Engines, November 26, 1970, PCA.
- 43 Memorandum from the Contact Committee 'Clean Engines', March 1971, PCA.
- 44 Meijer in the interview on August 3, 1998, explained that the contract with Ford was called the 'Sunrise Contract' by Pannenberg's wife, because it was finalised during a last-minute session that took a whole night.

- 45 Corporate Research Programme 1973. Not all groups were involved in Stirling technology.
- 46 R.J. Meijer, *The evolution of the Stirling Engine* (internal undated report on Stirling Thermal Motors)
- 47 On Dutch television for Teleac, broadcast on February 18, 1976.
- 48 Report Kern, March 20, 1979.
- 49 Although Meijer may not have been unique in doing this, there was certainly no general trend toward outsourcing in that time.
- 50 Ende, J. van den, Ravesteijn, W. and Wit, D. de (1997/8), 'Shaping the Early Development of Television', *IEEE Technology and Society Magazine*, Vol. 16, No. 4, 13-26.
- 51 See the article by F.H.J. van der Poel and J.J.P. Valetton in the *Philips Technical Review*, Vol. 15 (1953), pp. 221-232.
- 52 Van den Ende, Ravesteijn and De Wit (1997/8), pp. 19, 20.
- 53 Minutes of the Orco meeting on August 7, 1933, PCA.
- 54 See the article by Shagen, Bruining and Francken in the *Philips Technical Review*, Vol. 13 (1951).
- 55 This is what Casimir announced in the Quo Vadis meeting on January 13, 1956, according to the minutes of that meeting, PCA.
- 56 Memorandum 'Overzicht van de huidige stand van zaken op het gebied van de televisietechniek' ('Overview of the current state of affairs in the field of television technology'), dated November 20, 1935, PCA.
- 57 Balthasar van der Pol wrote an article in the *Eindhovens Dagblad* newspaper of December 21, 1935, that was entitled: Televisie thans een feit (Television now a fact).
- 58 Minutes of Televisiebespreking, December 29, 1938, PCA.
- 59 Minutes of Televisiebespreking, June 1, 1948, PCA.
- 60 Minutes of a meeting on iconoscopes and image iconoscopes that was held on Tuesday, December 20, 1949, PCA.
- 61 There was no name for it yet, so a question mark was used.
- 62 Memorandum H. Bruining dated June 8, 1950, PCA.
- 63 All minutes mentioned: PCA.
- 64 P.P.M. Schampers, interview and memorandum, February 11, 1999.
- 65 Interview with H.R. de Jager on October 25, 1990.
- 66 See the article by Schagen, Boerman, Maartens and Rijssel on the 'scenioscope' in the *Philips Technical Review*, Vol. 17 (1955), pp. 189-198.
- 67 Memorandum Ontwikkelactiviteit camerabuizen (by M. Wolff and J.R. Boerman), dated December 16, 1955, PCA-Components.
- 68 Minutes of the Bespreking vidicons voor kleurentelevisie, dated October 26, 1956, PCA-NL.
- 69 Memorandum Overwegingen van de HIG-EA betreffende opneembuizen, dated November 16, 1959, PCA.
- 70 Interview and memorandum Mr. P.P.M. Schampers, February 11, 1999.
- 71 Letter by H. Opdenberg (PD ELA) to Board of Management, dated July 3, 1959, PCA.
- 72 Minutes Quo Vadis meeting November 20, 1959, PCA.
- 73 Minutes Quo Vadis meeting Televisiecamerabuizen en -ketens, dated November 27, 1959, PCA.
- 74 Minutes Quo Vadis meeting Televisiecamerabuizen en -ketens, dated November 27, 1959; interview Mr. P. Broerse on March 10, 1998.
- 75 PCA-NL contains several fabrication prescripts by Van Esdonk and Schampers (dated December 19, 1960) and by Van der Drift and Schampers (same date).
- 76 Minutes of Plumbicon meeting, dated February 23, 1961, PCA-NL, and letter from Wolf to Brolsma (C.A. Electronica) on April 28, 1960, PCA.

- 77 Interview with Dr. P. Broerse on March 10, 1998.
- 78 Interviews with Dr. E.F. de Haan on September 9, 1997, and with Dr. P. Broerse on March 10, 1998. Broerse told the story of Mr Zeh, who instructed a new assistant by saying: 'Boy, do you know what a Plumbicon is? Well, it is a small tube of glass with a bit of lead in it. That's what you're going to make.'
- 79 Interview with Dr. E.F. de Haan on September 9, 1997. His remark is confirmed by EMI's researcher H.G. Lubszynski, who mentions as many as 36 different production methods being described in the Plumbicon patent (see Burns, R.W. (1998), *Television, an international history of the formative years*. London: The Institute of Electrical Engineers, p. 463).
- 80 Summary of Plumbicon meeting (by P.G. Kuipers, ELA), September 11, 1963, PCA.
- 81 Memorandum Hazeu (Board of Management) dated September 3, 1962, PCA.
- 82 Memorandum Board of Management (Hartong) to LeClerq, dated January 25, 1965, PCA.
- 83 Letter from the PD Electron Tubes to the Central Budget Committee, dated April 25, 1963 and letter from the Board of Management dated July 4, 1963; second request dated December 19, 1963, all documents PCA.
- 84 Letter from Emmerink, Noordhoff and De Vries (Elcoma) to Central Budget Committee, dated July 4, 1966, PCA-Components.
- 85 Memorandum J.R. Boerma (PD El. Tubes) and A.G. van Doorn (PD ELA), dated January 22, 1964, PCA.
- 86 Report of Troika meeting on January 8, 1964, PCA.
- 87 Report of the Plumbicon discussion on January 19, 1965, PCA-NL.
- 88 Letter of Tromp (Board of management) to Noordhoff, dated March 23, 1965, and report Plumbicon meeting on April 5, 1965, both PCA.
- 89 Letter of Hazeu to Board of Management, dated May 27, 1965, PCA-Components.
- 90 Report of Elcoma-ELA meeting on Plumbicons, dated October 6, 1967, PCA.
- 91 Interview with Dr. P. Broerse on March 10, 1998.
- 92 Corporate Research Programmes 1960, 1962, 1968, 1972. The group was apparently fairly constant in terms of size.
- 93 See the article by Broerse, Roosmalen and Tan in the *Philips Technical Review*, Vol. 28 (1968), pp. 325-335.
- 94 Corporate Research Programmes 1972-1978.
- 95 Corporate Research Programmes 1979-1985.
- 96 Report of the Committee 'New Plumbicon Generation', dated January 3, 1978. The objectives were to improve bandwidth and image quality.
- 97 According to E.F. de Haan (interview by H.R. de Jager on October 25, 1990) it was the Board of Management that had forced the early transfer to the factory, but no further evidence for this has been found.
- 98 Interview with Dr. E. Kooi, April 7, 1998.
- 99 The development of the transistor at Philips has been described in Verbong, G.P.J. (1981), *De ontwikkeling van de transistor bij Philips*. Eindhoven: Eindhoven University of Technology.
- 100 Riordan, M. and Hoddeson, L.E.G. (1997), *Crystal fire. The birth of the information age*. New York/London: W.W. Norton & Corporate; Ross, I.M. (1997), 'The Foundation of the Silicon Age', *Bell Labs Technical Journal*, Autumn 1997, pp. 3-14; Collet, J.P. (1997), 'The history of electronics', in: Krige, J. and Pestre, D. (Eds.), *Science in the Twentieth Century*. Amsterdam: Harwood Academic, pp. 253-274.
- 101 The precise agreement is described in Verbong (1981), *De ontwikkeling van de transistor bij Philips*. Eindhoven: Eindhoven University of Technology, chapters 5 and 6. In short, Western and Philips exchanged ferrite and semiconductor knowledge in a 'Main Agreement' on August 26, 1947, and on January 15, 1948, this agreement was extended by the 'Switching

- Agreement'. After the invention of the transistor there was a debate about whether or not transistors were part of what the agreements called 'resistors'. In the end Western and Philips agreed that Philips would pay the \$25,000 fee for the symposium, but be exempted from paying royalties for selling transistors in the devices that were mentioned in the Agreements. This resulted in a contract that was signed on March 31, 1952 (Report Orco meeting, PCA).
- 102 Interview with Dr. L.J. Tummers, April 2, 1997.
 - 103 Seitz, F. and Einspruch, N.G. (1998), *Electronic genie: the tangled history of silicon*. Chicago: Illinois University Press, p. 129.
 - 104 Interview with Dr. J.C. van Vessem on March 3, 1998.
 - 105 Interview with Dr. L.J. Tummers on April 2, 1997.
 - 106 Erker, P. (1993), 'Forschung und Entwicklung in der Transistortechnologie. Entscheidungszwänge und Handlungsspielräume am Beispiel Siemens und Phillips', 1947-1960. *Technikgeschichte* Vol. 60, No. 3, 267-284. English version in *History and Technology* (1994), Vol. 11, pp. 131-143.
 - 107 Blanken, I.J. (2002), *Geschiedenis van Koninklijke Philips Electronics N.V. V. Een industriële wereldfederatie*. Zaltbommel, Europese Bibliotheek, p. 143.
 - 108 Erker, Paul 1993, p. 274.
 - 109 Minutes of the 2nd CRC in 1952 and the 5th CRC in 1956, Corporate Research Programmes 1954 and 1956.
 - 110 Interview with Dr. E. Kooi on April 7, 1998.
 - 111 Report of a visit to Southampton by Van der Spek, Van Vessem and Van Wijlen, PCA-Components.
 - 112 Meilar-Smith, C.M., Haggan, D.E. and Troutman, W.W. (1997), 'Key steps to the Integrated Circuits', *Bell Labs Technical Journal*, Autumn 1997, pp. 15-28.
 - 113 Interview with Dr. F. Meijer on December 18, 1997.
 - 114 Minutes 7th CRC.
 - 115 Letter from Tromp and Hazeu, dated November 15, 1962, PCA-Components. The letter was quite critical about Philips achievements in the field of semiconducting techniques.
 - 116 PCA-Components.
 - 117 Blanken (2002), p. 377.
 - 118 Report of the Working Group Integrated Circuits, October 5, 1964, PCA.
 - 119 Minutes of the contact meeting Icoma-Nat.Lab., February 15, 1966, PCA-Components.
 - 120 Memorandum written by A.A. Opstelten, February 15, 1965, PCA-Components.
 - 121 Minutes of the meeting of the Raad van Bouwelementen, December 20, 1966, PCA.
 - 122 Memorandum from Mr. Willemsen to Mr. Emmerink and Mr. Noordhof, dated May 7, 1965, PCA-Components.
 - 123 Report of a visit to the USA by Van der Spek, Le Can and Tummers, 5-13 June 1965, letter by Opstelten dated January 28, 1966, both PCA.
 - 124 In his 1991 book on the invention of LOCOS, E. Kooi cited an article by Atalla et al.
 - 125 Progress Reports: L.J. Tummers, July-September 1966, PCA-NL.
 - 126 Progress Reports: L.J. Tummers, April-July 1966, PCA-NL.
 - 127 In his book on LOCOS, Kooi mentioned the id # 11.285. A. Wolters was made responsible for the application.
 - 128 Interview with Dr. J.C. van Vessem on March 3, 1998.
 - 129 Blanken (2002), p. 387.
 - 130 Interview on April 7, 1998.
 - 131 Progress Reports: J. Appels, June-August 1970, PCA-NL.
 - 132 Letter A. Willemsen dated July 8, 1966, PCA-Components. His opinion is based on an article in *Business Week* (July 2, 1966).

- 133 Memorandum The Integrated Circuit and its specific problems, May 1967, PCA-Components.
- 134 Minutes Technical Directors' meeting Elcoma, October 9, 1967 (report no. 67), PCA-Components.
- 135 Letter by P.H. Leclerq dated August 6, 1968, PCA-Components.
- 136 Letter by H.J. Vink dated July 23, 1969, PCA-Components.
- 137 Letter by A. Willemsen (Secretariat Corporate Analysis) dated September 10, 1968, PCA.
- 138 Interview with Dr. J.C. van Vessem on March 3, 1998.
- 139 Travel reports J.C. van Vessem to the USA, Spring 1971, PCA-Components.
- 140 Interview with Dr. E. Kooi on April 7, 1998.
- 141 Mentioned by L.J. Tummers in the Minutes of the 12th CRC in 1972.
- 142 Memorandum written by Le Can (Elcoma Nijmegen) dated September 3, 1971, PCA-NL, and Preliminary advice from Ockeloen, Hospel and Van Iersel to the Elcoma directors, dated November 1972, PCA-Components.
- 143 Product Policy Coordination Committee report, Nov. 19, 1975, PCA-Components.
- 144 Memorandum by Ockeloen to Stribos dated April 4, 1977, PCA-Components.
- 145 Minutes of the Solid State Policy Board Integrated Circuits, September 9, 1977, PCA-Components.
- 146 Letter by Stribos dated June 2, 1977, PCA-Components.
- 147 Letter dated February 11, 1971, PCA-Components.
- 148 Memorandum from De Haan and Rathenau to LeClerq dated February 23, 1971, PCA-Components.
- 149 Interview with Dr. E. Kooi on April 7, 1998.
- 150 Travel Report of trip to the USA by J.C. van Vessem, February 12-26, 1972, PCA.
- 151 Interview with Dr. E. Kooi on April 7, 1998.
- 152 Interview with Dr. E. Kooi on April 7, 1998.
- 153 Report of the discussions on the license policy semiconductors/ICs in Japan, dated February 3, 1981, PCA-Components.
- 154 The story of the VLP does not end in 1972, but most of it belongs to the 1946-1972 period. The transition between the periods described in Parts II and III is of course smooth.
- 155 Lang, J.K. (1996), *Das Compact Disc Digital Audio System. Ein Beispiel für die Entwicklung hochtechnologischer Konsumelektronik*. Aachen: Technische Hochschule, Diss.
- 156 Here we should mention that Philips is not the single inventor of the CD. The combination of necessary knowledge also involved the Sony company in Japan.
- 157 Travel report J.H. Wessels, October 4, 1957, and memorandum A./ Versnel (Patent Department) to Schuttevaer, Bijl, Meyer and Cluwen (Patent Department), PCA-NL 573 and PCA-NL 50.
- 158 'Rapport betreffende de uitvinding van een soort beeld-grammofoonplaat door de heer Rubbiani te Modena', PCA-NL 573.
- 159 Newspaper article *Het Parool* March 2, 1966.
- 160 Graham, M.B.W. (1986), *The Business of research: RCA and the VideoDisc*. Cambridge: Cambridge University Press.
- 161 Again there is a striking similarity to the Nat.Lab. situation at Philips.
- 162 Interviews with Dr. H.J.G. Meyer on September 16, 1997, and Dr. K. Bulthuis on November 18, 1997.
- 163 Report Nat.Lab.-ELA directors' meeting on September 5, 1969 (by J.J. Valetton, PCA-ELA 156637.
- 164 Interview on September 16, 1997.
- 165 1st symposium on audio-visual presentations September 1969, report in PCA-ELA 170515.
- 166 Memorandum from S.L. Tan dated January 27, 1970, PCA-ELA 156637.

- 167 Letter from H.J.G. Meijer to H.J. Gerritsen dated February 23, 1970, PCA-NL 174.
- 168 Interviews with Dr. H.J.G. Meyer on September 16, 1997, and Dr. P. Kramer on November 5, 1997.
- 169 We came across this name before in the Stirling Engine case.
- 170 In Progress Reports group Beek, April-July 1970 (mention of De Lang's publications PHN 787 and 4604), PCA-NL 787.
- 171 PCA-NL 228.
- 172 Report by J.C. Rovers, dated July 24, 1970, PCA-ELA 32814.
- 173 Report on discussion in the Nat.Lab.-ELA management meeting (J.J. Valeton, dated March 20, 1970), PCA-ELA 156637.
- 174 See the article by L.K.H. van Beek in the *Philips Technical Review*, Vol. 33 (1973), pp. 1-13.
- 175 Memorandum from G.W. Rathenau dated January 24, 1969, PCA-ELA 156618, and Progress Report April-September 1970, PCA-NL 787.
- 176 Progress report J.A. Rietdijk, October-December 1970, PCA-NL 787.
- 177 See the article by K. Compaan and P. Kramer in the *Philips Technical Review*, Vol. 33 (1973), pp. 178-180.
- 178 Corporate Research Programme 1972.
- 179 Interview with Dr. H.J.G. Meijer on September 16, 1997.
- 180 Minutes CRCs 1962 and 1964.
- 181 Interview with Dr. H.J.G. Meijer on September 16, 1997.
- 182 Progress Report K. Bulthuis 1970, PCA-NL 775.
- 183 Interview with Dr. P. Kramer on November 5, 1997.
- 184 Interview with Mr. L. Ottens on March 5, 1998.
- 185 Memorandum from PCA/BvG 2-6-92, PCA, and interview with L. Ottens on March 5, 1998.
- 186 Memorandum from P. Kramer and E.F. de Haan dated December 21, 1972, PCA-NL 50.
- 187 Interview with Dr. P. Kramer on November 5, 1997.
- 188 Memorandum from Board of Management 3/73, dated January 1973, PCA-NL 50.
- 189 Minutes meeting of the Policy Council on March 16, 1973, PCA-NL 50.
- 190 Minutes Policy Council 4/73, dated April 3, 1973, PCA-NL 50.
- 191 Travel Report R. Bom and H.J.G. Meyer to Japan and the USA, 4-15 June 1973, and Memorandum 'Beleid VLP' by R. Bom dated October 1973, both PCA-ELA 327815.
- 192 VLP Minutes of meetings (reports of weekly Monday morning meetings), August 11, 1975, PCA-NL 49.
- 193 Report of the MCA visit of October 25, 1976, PCA-NL 46.
- 194 Letter by K. Broadbent (MCA) to P. Kramer and P. Bögels dated February 3, 1975, PCA-NL 46.
- 195 PCA-NL 50.
- 196 Letter by T. v.d. Heer (Polygram) to the Policy Council, dated January 25, 1974, PCA-NL 50.
- 197 Memorandum J.P. Jorna and P.C. van Koningsbruggen, dated August 15, 1973, and Memorandum 'VLP and its software', dated May 1, 1974, both PCA-NL 50.
- 198 In the memorandum 'VLP and its software' (see previous footnote) this problem was ignored.
- 199 Minutes 13th CRC held in 1974.
- 200 Minutes RDC meeting June 1975.
- 201 Minutes meeting Technical Guidance Council dated August 8, 1974, PCA-ELA 327816.
- 202 Minutes RDC meeting May 1976.
- 203 Memorandum VLP discussions September 3, 1973, PCA-NL 50.
- 204 Minutes RDC, meeting May 1976.

- 205 Minutes RDC, meeting March 1979.
- 206 Minutes RDC, meeting November 1979.
- 207 S. van Houten (1991), *Why Industrial Research?* Lecture for the Royal Society of London (internal Philips publication). The issue of software is also suggested as an explanation for the failure of Philips activities in the video recorder market. The company had developed a machine called V2000, that from a technical point of view was better than Matsushita's VHS type of recorders, but the Philips company refused to produce video tapes with sex programmes. Although this explanation may be too simplistic, software does seem to have been a problem in several consumer electronics efforts that Philips made. In the case of the CD much attention was paid to software: Philips' Phonogram company was almost forced by the company's management to produce CDs.
- 208 Corporate Research Programmes 1977-1985.
- 209 Corporate Research Programme 1974.
- 210 Interviews with Mr. L. Ottens on March 5, 1998, and Dr. K. Bulthuis on November 18, 1997.
- 211 Lang (1997), p. 14.
- 212 Interview on September 16, 1997.
- 213 Lang (1996).
- 214 Blanken in the fifth volume of the book series on the history of the whole Philips company (Blanken, I.J. (2002), *Een industriële wereldfederatie*. Zaltbommel: Europese Bibliotheek) hardly pays attention to the aspect of 'fundamental' research when he describes the role of the Nat.Lab. in the period 1950-1970. Evidently, he also does not characterise this period as the period of fundamental research in the Nat.Lab., but rather as the period in which the Nat.Lab. preserved a rather independent role in the Philips company.
- 215 Minutes RDC meetings May 1965, November 1965 and May 1966.
- 216 Minutes December 1966 RDC meeting.

Intermezzo II

- 1 Hobsbawn, E. (1994), *Age of extremes. The short twentieth century 1914-1991*. London: Michael Joseph, p. 261.
- 2 Hobsbawn (1994), p. 261.
- 3 Hobsbawn (1994), p. 277.
- 4 Hobsbawn (1994), p. 265.
- 5 Hobsbawn (1994), p. 285.
- 6 Zanden, J.L. van and Griffiths (1989), *Economische geschiedenis van Nederland in de 20e eeuw*. Utrecht: Het Spectrum, p. 221.
- 7 Hobsbawn (1994), p. 444.
- 8 Bosscher, D. (1998), 'Diverse jaren zestig', *Spiegel Historiae*, Vol. 33, No. 5, pp. 184-185.
- 9 Smits, R. and Leyten, J. (1991), *Technology Assessment: waakhond of speurhond*. Zeist: Kerckebosch.
- 10 Arnold, E. and Balazs, K. (1998), *The Evaluation of Publicly Funded Basic Research*. Brighton: Technopolis, p. 10.
- 11 Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P. and Trow, M. (1994), *The New production of Knowledge. The Dynamics of Science and Research in Contemporary Societies*. London: Sage Publications. Also see Varma, R. (2000), 'Changing Research Cultures in U.S. Industry', *Science, Technology & Human Values*, Vol. 25, No. 4, pp. 395-416. He mentions the 1980s as the period in which this change took place, but in fact the transition started a decade earlier.

- 12 In 1973 Philips' Dr. A.E. Pannenberg became a member of this organisation.
- 13 Meadows, D.H., Meadows, D.L., Randers, J. and Behrens, W.W. (1972), *The limits to growth, a report for the Club of Rome's project on the predicament of mankind*. New York: Universe Book Publishers.
- 14 Tellegen, E. and Tommel, D.K.J. (1989), 'Milieuactie en milieubeleid', in: Boersema, J.J., Copius Peereboom, J.W. and Groot, W.T. de (eds.), *Basisboek Milieukunde*. Amsterdam: Boom, p. 58.
- 15 Halberstam, D. (1986), *The Reckoning*. New York: Morrow & Comp., p. 19.
- 16 Kaiser, W. (1992), 'Technisierung des Lebens', in: *Propyläen Technikgeschichte*, Vol. 5. Berlin: Propyläen Verlag, p. 383.
- 17 Abegglen, J.C. and Stalk Jr., G. (1985), *Kaisha, the Japanese Corporation*. New York: Basic Books, p. 110.
- 18 Kaiser 1992, p. 384.
- 19 After a pilot phase the programme expanded to include 226 projects in the years 1984-1986. The second phase followed in 1988-1992. In 1984 the First Framework Programme was initiated, and ESPRIT was embedded into it. The Framework Programme embraced more than the ESPRIT programme. It also comprised fields like new materials and energy research. The next phases were: the Second Framework Programme (1987-1991), the Third Framework Programme (1990-1994), the Fourth Framework Programme (1994-1998), and the Fifth Framework Programme (1998-2002). The budgets for the Framework Programmes varied from about 4,000 million ECU (First Framework programme) to 13,000 million ECU (Fourth Framework Programme).
- 20 Caracostas, P. and Muldur, U. (1998), *Society, The endless frontier. A European vision of research and innovation policies for the 21st century*. Brussels: European Commission.

Chapter 7

- 1 Zanden, J.L. van and Griffiths, R.T. (1989), *Economische geschiedenis van Nederland in de 20e eeuw*. Utrecht: Het Spektrum, p. 256.
- 2 Van Zanden and Griffiths 1989, p. 271.
- 3 Data have been derived from the Annual Reports of the company.
- 4 The pattern was: decrease in 1966, increase in 1967, decrease in 1970, increase in 1972, decrease in 1974, increase in 1976, decrease in 1979, increase in 1981, and decrease in 1985.
- 5 Blanken, I.J. (2002), *Geschiedenis van Koninklijke Philips Electronics N.V. Deel V. Een industriële wereldfederatie*. Zaltbommel: Europese Bibliotheek, p. 357. The early computer activities were also discussed in Chapter 5, section 5.1.
- 6 Chandler, A.D., Jr. (2002), *Inventing the Electronic Century*. New York: the Free Press, p. 186.
- 7 'Mijn opvattingen over de taak van de research-organisatie' by A.E. Pannenberg, August 11, 1972.
- 8 Memorandum 'computerresearch' by K. Teer, dated February 23, 1971.
- 9 Memorandum 'Home computer', dated September 8, 1977.
- 10 Memorandum K. Teer, dated September 7, 1979.
- 11 Internal memorandum 'Verslag PeCo discussie', H. Bosma, August 27, 1980.
- 12 Minutes 32nd RDC meeting, September 1975.
- 13 Minutes 15th CRC, 1978.
- 14 Interview with Dr. Pannenberg on January 5, 1998.
- 15 Minutes 18th CRC, 1984.
- 16 Minutes 13th CRC, 1974.
- 17 Minutes 30th RDC meeting, March 1975.

- 18 The lab participated in several European projects, and not just in the field of ICs. In 1989 Bulthuis estimated that the European grant incomes for the Nat.Lab. were NLG 17 million. In that year the Nat.Lab. took part in the programmes ESPRIT, RACE, and BRITE, and it also participated in European projects such as HDTV (High Definition Television), Jessi, Superconductivity and Prisma, for which grants were received from the Dutch government (about NLG 32 million in total).
- 19 Interview with Dr. Pannenburg on January 5, 1998.
- 20 Minutes 44th RDC meeting, November 1978.
- 21 Minutes 66th RDC meeting, November 1983.
- 22 Confidential memorandum Directiebureau, dated June 18, 1982.
- 23 Minutes of the 70th RDC meeting, May 1984.
- 24 Remark by Teer at the 72nd RDC meeting.
- 25 Minutes of the 73rd RDC meeting, November 1984. In 1985 Van Houten, Pannenburg's Board of Management successor, complained about a lack of interest for this in several of the research labs.
- 26 Minutes 89th RC meeting, June 1987.
- 27 Interview with Mr. N.J.P. Peters on January 20, 1998.
- 28 Jaarrede 1990 Dr. J.-P. Hurault, Nat.Lab. Report No. 6410.
- 29 Minutes 11th CRC, 1970.
- 30 'De positie van de researchorganisatie bij Philips', lecture presented by Pannenburg at the group leader meeting of June 11, 1974.
- 31 Minutes 14th CRC, 1976.
- 32 Corporate Research Programmes 1978, Minutes Kern, meeting March 20, 1979.
- 33 Minutes 91st RDC, January 1988.
- 34 Minutes 14th CRC, 1976.
- 35 In *Philips Research Reports* Vol. 15, 7, 1960.
- 36 See the article by J.A. Pistorius, J.M. Robertson and W.T. Stacey in the *Philips Technical Review* Vol. 35 (1975), 1-10.
- 37 Minutes of the 12th CRC in 1972.
- 38 Minutes of the 36th RDC in 1976.
- 39 Minutes 92nd RDC, April 1988.
- 40 Minutes of the 6th CRC, 1960.
- 41 Minutes 12th CRC, 1972.
- 42 Minutes 13th CRC in 1974.
- 43 This terminology is reminiscent of the Starnberger school in the philosophy of science.
- 44 It is striking that the terminology changed. The term 'fundamental' is used less often than before, and instead we find terms like 'long-term', 'exploratory', and 'risky' research being used. Although these terms do not necessarily have the same meaning as 'fundamental', they do seem to have replaced that term in the debates.
- 45 Of course this was supposed to be a contraction of Philips and fish.
- 46 The remaining one-third may seem like a lot compared with the 20% for 'exploratory' research that was mentioned as desirable earlier on, but one should bear in mind that this percentage includes more than 'exploratory' research. All research established by Philips Research as being desirable itself fell under this percentage.
- 47 Interview with Dr. J.M. van Nieuwland on November 20, 1997, and with Mr. L. Ottens on March 5, 1998.
- 48 Interview with Prof. D. Broer on January 12, 1998.
- 49 Memorandum Philips Research, June 10, 1994, F.P. Carrubba.
- 50 Robert Buder (in *Engines of Tomorrow*, New York: Simon & Schuster, p. 117) erroneously takes Carrubba to be the person responsible for the transition from central funding to con-

- tract-based funding for the Nat.Lab. This in fact was Van Houten, his predecessor. F.P. Car-
 rubba had previously spent 22 years at the famous IBM Corporation's Thomas J. Watson
 Research Laboratory in New York as a member of the technical staff and after that had
 moved to Hewlett-Packard to become Director of the Hewlett-Packard Laboratories. He
 did not stay with Philips for long. He left Philips in 1997 because he was dissatisfied with
 the new policy that was introduced by C. Boonstra, who had become Philips' CEO in 1996.
- 51 Interview with Dr. K. Bulthuis on November 18, 1997.
- 52 1991 Research Review Book.
- 53 'Operation Centurion' presentation by J.D. Timmer, Evoluon conference centre, March 3,
 1991
- 54 Declaration, March 7 1991, Corporate Research, CFT and CPT senior management.
- 55 Letter from K. Bulthuis to the participants of the Centurion III sessions, July 2, 1991.
- 56 Minutes of the Research Review Meeting on December 3, 1991.
- 57 Report Centurion III, 9-12 January 1992.
- 58 Roadmapping: Integrating Business and Technology, by Pieter Groenveld. Philips internal
 memorandum, November 6, 1996. Groenveld himself was not involved in the introduction
 of roadmaps in Research.
- 59 As in Part II, we find here the term Concern used rather than Corporate.
- 60 Minutes 12th CRC in 1972.
- 61 At the time this was written, the position was held by Dr. A. Huijser.
- 62 Interview with Kramer of November 5, 1997. After P. Kramer, only K. Bulthuis combined
 both functions.
- 63 This can be seen through, for instance, the emergence of the term 'sister' laboratories for
 the foreign labs.
- 64 Corporate Research Programme 1972.
- 65 Minutes 19th RDC meeting, February 1972.
- 66 Memorandum by H. Mooijweer, dated December 12, 1972, 'Microgolfresearch bij Philips'.
- 67 Minutes 25th RDC meeting, December 1973.
- 68 Minutes 43rd RDC meeting, May 1978.
- 69 Minutes 71st RDC meeting, June 1984.
- 70 Minutes 86th RDC meeting, October 1986.
- 71 Minutes 59th RDC meeting, January 1983.
- 72 Minutes 73nd RDC meeting, November 1984.
- 73 Minutes 82nd RDC meeting, January 1986.
- 74 Minutes 86th RDC meeting, October 1986.
- 75 This area was transferred to Aachen.
- 76 This area was transferred to the LEP.
- 77 Minutes 93th RDC meeting, June 1988.
- 78 Interview with S. Valkenburg on January 13, 1989.
- 79 Memorandum 'Loopbaanbegeleiding van het hoger personeel', by S. Valkenburg, dated
 March 1972, later included in the Nat.Lab. Technical Note No. 33/74.
- 80 As in Part II data have been derived from a computerized database of personnel data that
 was compiled especially for this historical research project.
- 81 Jaarrede J.-P. Hurault, Nat.Lab. Report No. 6410.
- 82 Jaarrede J.-P. Hurault, Nat.Lab. Report No. 6410.
- 83 Geschiedenis van Philips Research 1988-1994, memorandum by Dr. F. Meijer (undated,
 1998).
- 84 Interview with Dr. K. Bulthuis on November 18, 1997.
- 85 Interview with Dr. P. Kramer on November 5, 1997, Jaarrede J.-P. Hurault, Nat.Lab. Report
 no. 6410.

- 86 At this point it may be useful to explain what the CRB was. This bureau had been set up to support the work of the international research co-ordinator. The CRB published annual surveys of the Corporate Research Programme, that were known as the 'Blue Books' because of their blue covers. In the early 1980s the data were computerised and the name changed to 'Blue Base'. This database included contacts with the PDs. The CRB organised the R-PD meetings and thus had a supportive role in the contacts between Research and the PDs. The CRB also supported the organisation of the Corporate Research Exhibitions (see Chapter 5). In the 1990s the CRB was instrumental in compiling the Research Review Books that were used for discussing the research programme with the Philips Board of Management.
- 87 'Rapport van de commissie samenwerking Research – HIG'en', July 6, 1979.
- 88 Review Meeting of the Board of Management and Philips Corporate Research, September 2, 1983 (internal report).
- 89 Memorandum 'Bijeenkomst R.v.B. – Conc.Research op 13 maart 1979. Officieus commentaar Pannenburg op DB van 16 maart 1979', by H. Mooijweer, dated March 19, 1979.
- 90 Economising Plan Research, by K. Bulthuis, dated April 10, 1990.
- 91 Minutes 100th RDC meeting, April 1990.
- 92 Jaarrede 1989 K. Bulthuis, Nat.Lab. Report no. 6330.
- 93 Jaarrede 1990 J.-P. Hurault, Nat.Lab. Report no. 6410.
- 94 Operation Centurion. Movements for Change. J.D. Timmer, September 1990.
- 95 See also section 7.2.
- 96 Interview with Dr. F. Meijer on December 18, 1997.
- 97 Memorandum 'Geschiedenis van Philips Research 1988-1994' by F. Meijer, undated, 1998.
- 98 In the 1990 Review Meeting Book, on page 42, the example of Kodak was given. It was stated that this company was already unhappy with the dividing up of the research.
- 99 Review Meeting Book 1991, p. 22.
- 100 Minutes 20th CRC in 1988.
- 101 *Tijdschrift voor Wetenschap, Technologie en Samenleving*, No. 3, 1998, p. 104.
- 102 Review Meeting Book 1993, p. 96-99.
- 103 Interview on January 13, 1998.
- 104 Minutes Directors-CoCo meeting, January 27, 1971.
- 105 Molenaar, L. (1994), '*Wij kunnen het niet langer aan de politici overlaten...*'. *De geschiedenis van het Verbond van Wetenschappelijke Onderzoekers (VWO) 1946-1980*. Delft: Elmar.
- 106 Minutes Directors-CoCo meeting, June 21, 1974.
- 107 Minutes Directors-CoCo meeting, October 14, 1971.
- 108 Minutes Directors-CoCo meeting, April 24, 1979.
- 109 Interviews with Dr. S. Valkenburg on January 13, 1998, and Dr. H.J. Vink on January 7, 1998.
- 110 Cited in Molenaar 1994.
- 111 Molenaar 1994.
- 112 Minutes Directors-CoCo meeting, September 30, 1970.
- 113 Minutes Directors-CoCo meeting, October 3, 1978.
- 114 Minutes Directors-CoCo meeting, January 27, 1971.
- 115 Minutes Directors-CoCo meeting, September 5, 1984.
- 116 Minutes Directors-CoCo meeting, February 28, 1974.
- 117 Minutes Directors-CoCo meeting, May 16, 1977, May 9, 1978, November 10, 1981.
- 118 Minutes Directors-CoCo meeting, August 29, 1980, July 7, 1981.
- 119 Minutes Directors-CoCo meeting, January 27, 1971, November 17, 1980.
- 120 Minutes Directors-CoCo meeting, November 10, 1981, October 28, 1986.
- 121 Minutes Directors-CoCo meeting, June 29, 1982.

- 122 Minutes Directors-CoCo meeting, March 20, 1985.
- 123 Interview with Dr. S. Valkenburg on January 13, 1998.
- 124 'De bezetting van de assistent-functies in de research' (C.J. Gerritsma, F.C.M. van Gurp, J.M. van Nieuwland, L. Postma, A.S. Vreuls), dated November 1979.
- 125 'De problematiek van de medewerkers in de lagere functiegroepen op het Natuurkundig laboratorium' (G.A. Acket, P.M. van der Avoort, F.M.A. Carpay, F.C.M. van Gurp, J.H. Scholing, J.H. Veerman), dated March 1981.
- 126 According to the Technical Note No. 33/74, the CSB had started in 1966.
- 127 See interviews with Dr. E.F. de Haan on September 9, 1997, Dr. G. van Houten on November 19, 1997, Dr. F. Meijer on December 18, 1997 and Dr. K. Teer on September 9, 1997. H.J.G. Meyer (interview on September 16, 1997) expressed his opinion that it was mainly De Haan who realised the formalisation of the contacts between the Nat.Lab. and the PD.
- 128 See interviews with Dr. J.R. van Geuns on June 10, 1998, and Dr. J.C. van Vessem on March 3, 1998.
- 129 Report 'Bespreking Raad van Bestuur met Philips Concern Research', March 13, 1979. For some PDs [Lighting, Video, Small Domestic Appliances, Main Domestic Appliances, Electro Acoustics (ELA), Data Systems, Glass and the Nederlandse Kabel Fabrieken (NFK)] there was only one R-PD management committee. For Audio there were two: one presided over by Teer and one presided over by Valster. Two professional PDs had three R-PD management committees: Science & Industry and Medical Systems. Two PDs had a larger number of R-PD management committees. For the Telecommunications and Defence Systems PD six such committees existed, most of them focused on a specific area: radio, radar and fire control, transmission, switching, and infrared. One of them had a more general title: Advanced development. The largest number of R-PD management committees was for Elcoma. A list of titles gives an impression of the variation which existed in content: materials and components, ohmic resistance, magnetic materials, non-linear resistance, electrolytic capacitors, ceramic capacitors, professional sub-assemblies, discrete semiconductors, professional tubes, consumer tubes, and integrated circuits. Besides these there was a tripartite R-PD management committee TDS-Elcoma-Research. The number of committees related to Elcoma illustrates the importance of Elcoma as a research client'.
- 130 Minutes 15th CRC, 1978.
- 131 Interview on June 10, 1998.
- 132 Minutes 16th CRC, 1980.
- 133 A short description of the history of the Geldrop project centre up until 1990 is presented in the *Nat.Lab. Journaal* of August 24, 1990.
- 134 Interview with Mr. De Kruijff on March 2, 1998.
- 135 See the article by W. Bloemendal and C. Kramer in the *Philips Technical Review*, Vol. 33, No. 5, pp. 117-129.
- 136 Interview with Dr. Valster on November 24, 1997.
- 137 According to N.J.P. Peters (interview on January 20, 1998) who worked in Geldrop as an assistant and who later moved to Waalre, the whole atmosphere in Geldrop differed from the atmosphere in Waalre.
- 138 See the article by J. Cruq in the *Philips Technical Review*, Vol. 34, No. 4, pp. 106-111.
- 139 See the article by J.H.A. Melis in the *Philips Technical Review*, Vol. 40, No. 6/7, pp. 1231-1236.
- 140 Nat.Lab. Report No. 6330, Jaarrede 1989, F. Valster and Dr. K. Bulthuis.
- 141 See the article by J. van der Heijden in the *Philips Technical Review*, Vol. 41, No. 9, pp. 253-259.
- 142 See the article by J.R. Brandsma in the *Philips Technical Review*, Vol. 43, No. 1, pp. 10-21. In chapter 8 a case study in the field of optical communication with a lot of co-operation will be described.

- 143 Interview with Dr. F. Valster on November 24, 1997.
- 144 Minutes 90th RDC meeting, September 1987.
- 145 Research Review Book 1988.
- 146 Interview Dr. K. Bulthuis of November 18, 1997.
- 147 Minutes 91st RDC meeting, January 1988.
- 148 Minutes 93rd RDC meeting, June 1988.
- 149 From that list we can conclude that 4 projects had the status of 'Running, and document signed by managing directors'. All 4 were for the Consumer Electronics PD and had to be carried out by the Nat.Lab. In total, 133 Research people and 147 PD people were involved in those four projects. The topics were: CAD, High Definition Television (HDTV) and LCDs for television. Another 22 projects had the status of 'Running, document agreed and signed by (deputy) directors; ready for signing by managing directors'. Most of those were rather small projects (altogether about the same number of people from Research and from the PDs as for the four previously mentioned projects were involved). In this group of 22 projects we find that seven PDs were involved and all the research labs. Some of the larger ones (i.e. those involving more than 20 people) were: solid-state image sensors (with Elcoma), and media for magneto-optical recording (with PDO, the Philips Du Pont Optical joint venture). Furthermore, 9 projects had the status of 'Running, document agreed and signed by (deputy) directors; some slight updating was required for the specification of names of people, etc'. Those 9 again involved about the same number of people as the other groups of 4 and 22 projects, but it must be remarked that the Mega project is included here with 75 Research people and 57 PD (=Elcoma) people. The second-largest project in this category is concerned with long wavelength optical devices (again with Elcoma). A group of 14 projects had been given the status of 'Principle agreement to have a transfer project, but negotiations not yet concluded; or document being prepared'. Finally, there was a group of 15 projects which had the status 'No agreement, no transfer, or future transfer projects'.
- 150 Interview with Dr. R.P. Kramer on November 11, 1997. In this whole paragraph Kramer should be understood as meaning R.P. Kramer, not to be confused with P. Kramer.
- 151 This comment was also made by F. Valster in an interview on November 24, 1997.
- 152 Interviews with Dr. K. Bulthuis on November 18, 1997, and with Dr. R.P. Kamer on November 11, 1997.
- 153 1989 Research Review Book.
- 154 For example R.P. Kramer and J. van Nieuwland, who were interviewed on November 11, 1997, and November 20, 1997.
- 155 Interview Dr. S. van Houten on November 19, 1997. Indeed, later we find several examples of research directors who had worked in a PD for some time (e.g. A. Huijser and R. Harwig).
- 156 Interview with Dr. K. Bulthuis on November 18, 1997.
- 157 Interview with Dr. P. Broerse on March 10, 1988.
- 158 Review Meeting Book, November 29, 1989.
- 159 'De positie van de research-organisatie bij Philips', presentation of June 1, 1974, by Dr. A.E. Pannenberg.
- 160 The term is used e.g. in the Minutes of the 17th CRC.

Chapter 8

- 1 This case study description is based on a memorandum by P.G.I. van den Berg, M.Sc.
- 2 Kao, K.C. and Hockam, G.A., 'Dielectric fibre surface wave guides for optical frequencies', *Proc. I.E.E.E.*, Vol. 113, No. 7, July 1966, pp. 1151-1158.

- 3 Faltas, S. (1988), 'The invention of fibre-optic communications', *History and Technology*, Vol. 5, pp. 31-50.
- 4 Minutes Orcometing, September 16, 1971, PCA.
- 5 Minutes 11th CRC, 1970.
- 6 K. Mouthaan, Wideband Transmission by Glass-fibre Optical Cable. Technical Note 47/72.
- 7 According to Acket in an article published in the *Philips Technical Review*, Vol. 36, No. 7, pp. 198-208.
- 8 K. Mouthaan, 'Glasvezel Proefroute Helmond-Eindhoven', memo of lecture on October 6, 1980.
- 9 Solymar, L. (1999), *Getting the Message. A History of Communications*. Oxford: Oxford University Press, p. 227.
- 10 K. Mouthaan, 'Glasvezel Proefroute Helmond-Eindhoven', memo of lecture on October 6, 1980.
- 11 See the article by L.J.M. Bollen, J.J. Goedbloed and E.T.J.M. Smeets published in the *Philips Technical Review*, Vol. 36, No. 7, pp. 213-218.
- 12 See the article by H.M.J.M. van Ass, P. Geittner, R.G. Rossink, D. Küppers and P.J.W. Severin in the *Philips Technical Review*, Vol. 36, No. 7, pp. 190-197, the article by K. Mouthaan in the *Philips Technical Review*, Vol. 36, No. 7, pp. 186-189, and the article by J.W. Versluis, J.G.J. Peelen in the *Philips Telecommunication Review*, Vol. 37, No. 4, Sept. 1979, pp. 215-230.
- 13 According to Dr. A. Kats, during interview on October 1998, this method had been invented by D. Küppers.
- 14 Interview Dr. A. Kats on January 19, 1999.
- 15 Interview with Dr. G.A. Acket, November 3, 1998 and *Philips Koerier* September 20, 1979.
- 16 Minutes Orco meeting, April 10, 1951, PCA.
- 17 Article in the *Financieel Dagblad*, March 19, 1970, PCA.
- 18 Corporate Annual Reports 1969 and 1970.
- 19 Minutes of a meeting in Colone on October 3, 1969, and Minutes of the Orco meeting on May 26, 1970, both PCA.
- 20 Minutes Orco meeting, June 20, 1967, PCA.
- 21 Interview with Dr. K. Mouthaan on September 22, 1998.
- 22 Minutes Policy meeting, August 14, 1975, PCA-HIG Glass.
- 23 Minutes Orco meeting, April 23, 1974, PCA.
- 24 See the article by Mouthaan published in 1976 in the *Philips Technical Review*, Vol. 36, No. 7, pp. 186-189.
- 25 K. Mouthaan (1970), 'Telecommunication via glass-fibre cables', in: *Philips Telecommunications Review*, Vol. 37, No. 4, pp. 204-214.
- 26 Interview with Dr. K. Mouthaan on September 22, 1988.
- 27 F. Krahn and H. Thielmann (1970), '34 Mb/s optical cable system test in Berlin', in: *Philips Telecommunications Review*, Vol. 37, No. 4, pp. 257-269.
- 28 Minutes Orco meeting, September 16, 1975, PCA.
- 29 Interview with Dr. K. Mouthaan on September 22, 1998.
- 30 Minutes Orco meeting, June 24, 1975, PCA.
- 31 Policy meeting HIG Glas September 9, 1978 (dir. 78/231), PCA.
- 32 Interview Dr. K. Mouthaan on November 25, 1998, and interview with Dr. A. Kats on October 14, 1998.
- 33 Krahn, F., Olejak, G. and Weidhaas, W. (1979), 'The manufacture of optical cables', *Philips Telecommunications Review*, Vol. 37, No. 4, pp. 231-240.
- 34 The name contained a playful element in that it referred to the Royal Dutch Airlines which in Dutch is also abbreviated as KLM.

- 35 Interviews with Dr. A. Kats on October 14, 1998, and on January 19, 1999.
- 36 Minutes Orco meeting, February 27, 1979, PCA.
- 37 Minutes State of the TDS-Elcoma-Research Tripartite discussion on December 30, 1977, PCA-Components.
- 38 Minutes of the meeting Research-Elcoma Co-ordination Committee meeting on Discrete Semiconductor Devices, November 6, 1978, PCA-Components; Minutes Orco meeting on August 15, 1978, PCA; article 'Studiedag over glasvezel', *Philips Koerier*, November 13, 1980.
- 39 Minutes Orco meeting, February 27, 1979, PCA.
- 40 Minutes Orco meeting, February 27, 1979, PCA.
- 41 Article 'Glasvezelroute Helmond-Eindhoven in gebruik', *Philips Koerier*, October 9, 1980.
- 42 Minutes Orco meeting on May 3, 1977, PCA.
- 43 See e.g. Corporate Research Programmes 1975, 1976.
- 44 Minutes Orco meeting, March 25, 1975, PCA.
- 45 Article 'Glasvezel: de weg naar de informatie-maatschappij', *Philips Koerier*, May 6, 1982.
- 46 Interview with Dr. G.D. Khoe on October 29, 1998.
- 47 Solymar (1999), p. 205.
- 48 See the article by F.W. de Vrijer in the *Philips Technical Review*, Vol. 36 (1976), No. 11/12, p. 346.
- 49 PCM converts absolute signals, and DM converts relative signals. The feedback is used to obtain the relative signals.
- 50 See the article by L.D.J. Eggermont, M.H.H. Höfelt and R.H.W. Salters in the *Philips Technical Review*, Vol. 37 (1977), No. 11/12, pp. 331-348.
- 51 Documentation Van Kessel, Progress report Measurement and control group, January-March 1976. The reader should be aware that here 12-bit refers to the precision of the signal. That is different from the term 1-bit technology, in which 1-bit refers to the conversion technique. Thus, a 1-bit convertor can have 12-bit resolution.
- 52 Documentation Van Kessel, Progress report Measurement and control group, January-June 1977.
- 53 CD-players, Directorate meetings Nat.Lab.-Audio, January 29, 1975, PCA.
- 54 Directorate contact meeting Nat.Lab.-Audio, Minutes meeting, September 4, 1975, PCA-HIG Consumer Electronics.
- 55 Interview with Dr. L.D.J. Eggermont on March 18, 1999, interview with Dr. Th.J. van Kessel on March 5, 1999.
- 56 CD-players, Philips information (1982), Compact Disc Digital Audio, 8861N/mrt/82, PCA.
- 57 Directorate contact meeting Nat.Lab.-Audio, Minutes meeting, November 2, 1977, PCA-HIG Consumer Electronics, and Documentation Van Kessel, Measurement and Control Group, Progress report January-March 1978.
- 58 See the preface in the special issue of the *Philips Technical Review*, Vol. 40, No. 9, on the Compact Disc.
- 59 Interview with Dr. E.C. Dijkmans on September 24, 1998, interview with Mr. L. Boonstra on March 23, 1999.
- 60 Interview Dr. Th.J. van Kessel on March 5, 1999.
- 61 Interview Dr. R.J. van der Plassche on January 12, 1999, interview Dr. E.C. Dijkmans on September 24, 1998, interview with Mr. Th.J. van Kessel on March 5, 1999.
- 62 Documentation Van Kessel, Measurement and Control group, Progress report June-September 1978.
- 63 Documentation Van Kessel, Measurement and Control group, Progress report June-September 1978.
- 64 Documentation Van Kessel, Measurement and Control group, Progress report July-September 1979.

- 65 Explanations of these techniques can be found in Plassche, R.J. van de (1994), *Integrated analog-to-digital and digital-to-analog convertors*. Dordrecht: Kluwer Academic Publishers, pp. 368-370.
- 66 Documentation Van Kessel, Measurement and Control group, Progress report January-July 1980.
- 67 CD memorandum by C.J. van der Klugt (Board of Management), November 25, 1981, PCA.
- 68 Interview with Dr. Th.J. van Kessel on March 5, 1999.
- 69 Documentation Van Kessel, Measurement and Control group, Progress report January-July 1982.
- 70 Documentation Van Kessel, Measurement and Control group, Progress report October 1983-July 1984.
- 71 See the article by A.W.M. van den Enden and N.A.M. Verhoeckx in the *Philips Technical Review*, Vol. 42 (1985), pp. 120-156, in particular p. 152.
- 72 Documentation Van Kessel, Year plan 1981.
- 73 Documentation Van Kessel, Measurement and Control group, Progress report January-July 1982.
- 74 Documentation Van Kessel, Measurement and Control group, Progress report January-July 1982. The A/D converters for video applications used a totally different technique.
- 75 Again the reader should be aware of the difference in meaning between 1-bit and 14-bit or 16-bit converters (see note 49).
- 76 Documentation Van Kessel, Year plan, Measurement and control group, 1986.
- 77 This case study description is based on a memorandum by Mr. H.E.M.M. van Gastel.
- 78 See the article by D.A. Snel (1952) in the *Philips Technical Review*, Vol. 14, pp. 165-174.
- 79 Minutes Orco meeting, January 7, 1953, PCA.
- 80 Memorandum on the history of the Philips video recorder by B. van Gansewinkel, PCA; in Ketteringham, J.M. and Nayak, P.R. (1986), *Breakthroughs*, p. 25, we find that 1954 was the year in which Anderson, Dolby, Maxey, Henderson, Ginsburg and Pfof built this machine.
- 81 Minutes Orco meeting Ampex 309-6a.
- 82 Minutes Television meeting on February 11, 1958, PCA-NL.
- 83 Minutes Television meeting on September 12, 1958, PCA-NL.
- 84 Annual Report RGT 1961, PCA.
- 85 Minutes of meeting Magnetic recording heads (Magneetkoppen), April 16, 1962, PCA-Components.
- 86 Meeting Magneetkoppen, June 15, 1962, PCA-Components.
- 87 Memorandum Videorecorders, July 31, 1963, PCA-Components.
- 88 Survey recording heads by L. Hintzbergen, J.A. Otten and A.J. de Rooy, dated December 10, 1964, PCA-Components.
- 89 Memorandum on the history of videorecorders by B. van Gansewinkel, PCA.
- 90 Letter from A. Dros to A.J. de Rooy and W.K. Westmijze, dated October 11, 1965, PCA-Components.
- 91 Report visit Boelens and Dekker to Sony, February 7-11, 1966, PCA-ELA.
- 92 Internal memorandum by G. van Gijn (Elcoma) to A. Dros (Elcoma) dated April 21, 1966, in which a phone call with Pannenberg is alluded to, PCA-Components.
- 93 Minutes meeting Icoma/Elcoma-Nat.Lab., December 15, 1966, PCA-Components.
- 94 Memorandum 'IG professionele onderdelen en materialen', data for Annual Report 1968 by G. van Rijn (undated), PCA-Components.
- 95 Memorandum by G.M. Kruijmel (CV&P), dated May 14, 1969, PCA.
- 96 Minutes Directors' meeting Nat.Lab.-RGT, October 31, 1969, PCA-Consumer Electronics.
- 97 Minutes Directors' contact meeting, April 22, 1970, PCA-Consumer Electronics.

- 98 Ketteringham, J.M. and Nayak, P.R. (1986), *Breakthroughs*, p. 27.
- 99 'Overzicht situatie magneetkoppen', by M.H.M. Vrolijk, dated January 18, 1972, PCA-Components.
- 100 Report visit A.J. Stralen (ELMA Lab) to the Vienna factory, April 10-21, 1972, PCA-Consumer Electronics.
- 101 Minutes meeting 'Bespreking algemene situatie magneetkoppen', June 28, 1972, PCA-Components.
- 102 Minutes Directors' contact meeting, March 7, 1974, PCA-Components.
- 103 Minutes Directors' contact meeting, November 27, 1975, PCA-Components
- 104 Ketteringham, J.M. and Nayak, P.R. (1986), *Breakthroughs*, p. 32.
- 105 Directors' contact meeting, May 14, 1975, PCA-Consumer Electronics.
- 106 Minutes meeting, December 13, 1977, PCA-Components.
- 107 Minutes Directors contact meeting, September 12, 1978, PCA-Consumer Electronics.
- 108 Minutes Management meeting Research-Elcoma, October 27, 1982, PCA-Components.
- 109 Memorandum on the history of the Philips video recorder by B. van Gansewinkel, PCA.

Chapter 9

- 1 As Mowery [Mowery, D.C. (1981), *The emergence and growth of industrial research in American manufacturing, 1898-1945* (diss.). Stanford: Stanford University] pointed out, most historiographies of industrial research labs suffer from a lack of information about research output regarding their contribution to the company's productivity. In itself, that remark is true. It often appears to be difficult to obtain such data. In the case of Philips, too, getting organised data on patents and licences was problematic. It was possible to get more general information that allowed some general conclusions to be drawn on research output. Another problem when analysing this output was the difficulty of identifying exactly the contribution of the research activities to the whole process of technological developments in industrial companies. The final outcome is the result of effort on the part of many parties, and what exactly was the contribution made by each of those parties is hard to distinguish. It was possible to try to understand the research efforts and outcomes in the context of the circumstances in which the research activities took place.
- 2 Chandler, R. (1977), *The visible hand*. S.I.: Bellknap Press.
- 3 Kenneth Mees, C.E. (1920), *The Organization of Industrial Scientific Research*. New York: McGraw-Hill, p. 70.
- 4 Buderer (2000), *Engines of Tomorrow*, New York: Simon & Schuster, p. 20, mentions, for example, differences between the chemical and the electronics sector.
- 5 Reich, L.S. (1985), p. 255.
- 6 As Jasper Faber has pointed out in his dissertation, *Kennisverwerving in de Nederlandse industrie 1870-1970* (Aksant, 2001), in small and medium-sized industries, knowledge acquisition was often organised in ways that differed significantly from that in larger companies; in this section we will therefore not make any comparison with small and medium-sized companies.
- 7 See Boersma, K. (2003), 'Structural was to embed a research laboratory into a company: a comparison between Philips and General Electric 1900-1940', *History and Technology*, Vol. 19, No. 2, 1-18.
- 8 See Buderer (2000), p. 94, and Reich, L.S. (1985), *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926*. Cambridge: Cambridge University Press, p. 256.
- 9 See Buderer (2000), p. 105.

- 10 Reich (1985), p. 257.
- 11 For comparison: for the Philips Nat.Lab. the change was from 100% direct corporate funding to one-third corporate funding; for GE the change was from two-thirds corporate funding to one-quarter corporate funding; see Buderl (2000), p. 247.
- 12 Schottky's name is connected still today to several effects and devices: the Schottky defect, the Schottky barrier and the Schottky diode. He was one of the first to recognise the existence of electron 'holes' in the valence-band structure of semiconductors.
- 13 This account has been derived from Buderl (2000). He does not mention if contract research was established in the Siemens research organisation.
- 14 Hounshell, D.A. and Smith, J.K. (1985), *Science and Corporate Strategy. Du Pont R&D, 1902-1980*. Cambridge: Cambridge University Press, p. 7.
- 15 Hounshell and Smith (1985), p. 594.
- 16 Hounshell and Smith (1985), p. 595.
- 17 Hounshell and Smith (1985), p. 597. Due to lack of sources, for Philips, we have not found evidence that the choice for 'fundamental' research was the company management's preference. What we did find is that the term 'fundamental' research was used by the lab to protect themselves from division influence. Hounshell and Smith clearly ascribe the decision to invest in this type of research to the company's management. Also they seem to suggest that at Du Pont there really existed a special type of research that was called 'fundamental'. For Philips the content of the term 'fundamental' at least partially had a protective function, as we saw.
- 18 Hounshell and Smith (1985), p. 599.
- 19 Hounshell and Smith (1985), p. 1.

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