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*Friends,
you know who you are,
you know I'm grateful*

Small science on big machines

Small science on big machines

Politics and practices of synchrotron radiation laboratories

Olof Hallonsten

Lund Studies in Research Policy 1

Research Policy Institute
Lund 2009

Small science on big machines
Politics and practices of synchrotron radiation laboratories

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Research Policy Institute, Lund University, Sweden

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Summary

Synchrotron radiation laboratories are large scientific facilities where various scientific experiments are carried out by the use of radiation produced by particle accelerators. Research with synchrotron radiation emerged in the 1960s and 1970s as a peripheral activity at particle physics laboratories. It has since expanded and taken over facilities from particle physics, and developed specialized accelerators of its own, gradually becoming an important resource for a variety of scientific disciplines, foremost for different types of studies of materials on atomic and molecular level. This thesis is a study of the *institutionalization* of synchrotron radiation – its scientific and technological, but also political and sociological, development.

Three case studies, chosen to complement each other, highlight different aspects of this process. The *Stanford Synchrotron Radiation Lightsource* in Menlo Park, California, was a pioneering laboratory in the early days of synchrotron radiation. *MAX-lab* in Lund, Sweden, originated as a small scale university project and expanded gradually to become a national and international user facility. The *European Synchrotron Radiation Facility* in Grenoble, France, is a multinational collaborative European project and one of the world's largest synchrotron radiation laboratories.

The analysis is organized around three themes: the *changing dynamics of science*, *changes in science policy*, and the identification of *scientific entrepreneurs* – actors with particularly strong roles in the institutionalization. In recent decades, science has encountered increased demands for accountability and social and economic returns, resulting in disciplinary and organizational restructurings and internal sociological changes. These include the *collectivization* of scientific research and the *sophistication* of scientific instrumentation. The thesis identifies synchrotron radiation laboratories as manifestations of these trends; they are a new type of 'big science', sustaining small scale science in various fields – *small science on big machines*. It is argued that both the laboratories and the scientific activities they host are particularly well adapted to the new social and political conditions.

Sammanfattning på svenska

Synkrotronljuslaboratorier är stora vetenskapliga anläggningar vid vilka olika typer av vetenskapliga experiment bedrivs genom utnyttjandet av strålning som produceras av partikelacceleratorer. Forskning med synkrotronljus startade som perifer verksamhet vid partikelfysiklaboratorier på 1960- och 70-talen och har sedan dess kraftigt expanderat, tagit över laboratorieresurser från partikelfysiken, och även utvecklat egna specialiserade acceleratorer. Synkrotronljus har gradvis blivit en viktig resurs på många vetenskapliga ämnesområden, främst för olika typer av studier av material på atomär och molekylär nivå. Denna avhandling är en studie av synkrotronljusets *institutionalisering* – dess vetenskapliga och tekniska, men också politiska och sociologiska, utveckling.

Tre fallstudier, utvalda för att komplettera varandra, belyser olika aspekter av processen. *Stanford Synchrotron Radiation Lightsource* i Menlo Park, Kalifornien, var tidigt en föregångare inom forskning med synkrotronljus. *MAX-lab* i Lund, Sverige, började som ett småskaligt universitetsprojekt och har gradvis expanderat till en nationell och internationell användarfacilitet. *European Synchrotron Radiation Facility* i Grenoble, Frankrike, är ett multinationellt europeiskt samarbetsprojekt och ett av världens största synkrotronljuslaboratorier.

Analysen är organiserad kring tre huvudteman: *vetenskapens förändrade dynamik*, *forskningspolitiska förändringar*, och betydelsen av *vetenskapliga entreprenörer* – aktörer med särskilt tydliga och viktiga roller i institutionaliseringen. De senaste decennierna har allt större krav på produktivitet och samhällelig och ekonomisk nytta ställts på vetenskapen. Detta har medfört disciplinära och organisatoriska omstruktureringar och interna sociologiska förändringar, till exempel *kollektivering* av vetenskapligt arbete och så kallad *s sofistisering* av vetenskaplig instrumentation. Avhandlingen visar att synkrotronljuslaboratorier tydligt manifesterar dessa trender, genom att de utgör en ny typ av 'storforskning' som i själva verket understöder småskalig forskning på olika områden – avhandlingens titel översätts följaktligen med *småforskning på stora maskiner*. I avhandlingen framträder såväl laboratorierna som forskningen de står värd för som särskilt anpassade till vetenskapens nya sociala och politiska omständigheter.

Souhrn v češtině

Synchrotronové radiační laboratoře jsou velká vědecká zařízení, v nichž jsou prováděny různé vědecké experimenty s použitím radiace vydávané částicovými akcelerátory. Výzkum synchrotronového záření začal v 60. a 70. letech jako okrajová aktivita v laboratořích fyziky částic. Od té doby došlo k jeho rozmachu, převzetí zařízení dříve se zabývajících fyzikou částic a k vývoji vlastních akcelérátorů; postupně se stal významným zdrojem poznání pro řadu vědeckých disciplín, v první řadě pro různé typy výzkumu materiálů na atomové a molekulární úrovni. Tato práce se zabývá zkoumáním *institucionalizace* výzkumu synchrotronového záření – jeho vědeckého a technologického, ale také politického a sociologického vývoje.

Tři případové studie, vybrané tak, aby se navzájem doplňovaly, osvětlují různé aspekty tohoto procesu. Laboratoř *Stanford Synchrotron Radiation Lightsource* ve městě Menlo Park, Kalifornie, byla v počátcích výzkumu synchrotronového záření průkopnickou laboratoří. Laboratoř *MAX-lab* v Lundu, Švédsko, vznikla jako univerzitní projekt malých rozměrů a postupně se rozšiřovala, až se stala zařízením vyhledávaným švédskými i zahraničními uživateli. *European Synchrotron Radiation Facility* v Grenoblu, Francie, je mnohonárodnostní projekt evropské spolupráce a jedna z největších světových synchrotronových laboratoří.

Analýza je uspořádána do tří témat: *mění se dynamika vědy, změny ve vědecké politice* a identifikace *podnikatelů ve vědě* – hráčů, kteří v institucionalizaci mají zvláště významnou úlohu. V posledních dekadách věda čelí zvýšeným požadavkům na odpovědnost a sociální a ekonomickou návratnost, vedoucí k restrukturalizaci disciplín a organizace a k interním sociologickým změnám. K těm patří *kolektivizace* vědeckého výzkumu a *s sofistikovanosť* vědeckého vybavení. V této práci jsou identifikovány synchrotronové radiační laboratoře jako manifestace těchto trendů; jsou novým typem 'velké vědy' podporující v různých odvětvích vědu v malém měřítku – odtud název práce, který vysvětluje *malou vědu na velkých strojích*. Obhazuje myšlenku, že jak laboratoře, tak vědecké aktivity, které se v nich konají, jsou velmi dobře přizpůsobeny novým sociálním a politickým podmínkám.

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Margrethe Bohr: *Physics, yes? Physics.*

Niels Bohr: *This is physics.*

Margrethe Bohr: *It's also politics.*

Werner Heisenberg: *The two are sometimes painfully difficult to keep apart.*

(From the play *Copenhagen* by Michael Frayn)

Introduction

This is a book about science, but also about politics – not only because the two are difficult to keep apart, but also because the politics of science is intriguing, important, and particularly well-suited for sociological studies. Science is political because its conduct and content are influenced by political will, but also by having an internal politics. Science follows certain procedures, takes place at certain locations, has its results framed according to certain standards, and organizes itself according to certain patterns. All of these have an internal political logic and are influenced by external politics, and the understanding and problematization of these two political sociologies of science – as well as clarification of their separation and overlaps – are within the main aim of this thesis. The four-year study that is concluded by the publication of this thesis once departed in the realization of these essential features of modern science. It continued with the identification of a focus for the study: A particular kind of scientific laboratories – physical locations where science *is done*. The laboratories in question – *synchrotron radiation laboratories* – have proved to be substantial examples of the inseparability of science and politics and therefore very suitable for a study in political sociology of science.

Synchrotron radiation laboratories are large workplaces of science, and the technology put to use within their walls is sophisticated and expensive. The laboratories are the result of heavy investments and long-term financial commitments by governments, which make their existence political in a very tangible sense. Synchrotron radiation laboratories are, however, utilized for a variety of scientific activities, and their technological setups are multiple and interchangeable in that they can host several different experiments simultaneously and their experimental facilities can be partially substituted. Their prime function is to make available sophisticated instrumentation for research groups from ‘ordinary’ science institutions such as universities, research institutes and industrial R&D departments to utilize in temporary runs of experimental work. Therefore, despite their size and the comprehensive political commitments required, they are dynamic and receptive to changes in the surrounding scientific landscape. The combination – shaped by small and diversified science but enabled by large scale and centralized political initiative and command – makes these laboratories illustrative examples of *how* science is political.

Synchrotron radiation has been used in science for approximately fifty years and during this time it has expanded from an 'esoteric endeavor', practiced by a very small number of physicists, to a scientific 'mainstream activity' with applications in a wide range of sciences. About 40 synchrotron radiation laboratories are in operation worldwide, and their total number of users amounts to several tens of thousands.¹

Synchrotron radiation laboratories come in very different sizes and appearances. They may be as large as soccer fields or small enough to fit in a truck garage; they may be hidden in basements of university campus buildings or exposed with architectural majesty. What they all have in common is, however, the round-shaped accelerators in which particles rush around close to the speed of light and by doing so emits extremely bright and focused beams of radiation – *synchrotron radiation* – in the x-ray and ultraviolet ranges for the most part. The radiation is led through pipes to experimental stations where they are utilized for a variety of scientific experiments in subfields of physics and materials sciences, chemistry, biology and life sciences, medicine, earth and environmental sciences, engineering and cultural studies. Common to them all is that their experimental work benefits from access to this intense radiation that can not be produced by any other means.

Synchrotron radiation emerged as peripheral activity at particle physics laboratories and operated in the shadow of particle physics for a couple of decades, but nowadays accelerators and the surrounding infrastructures are purpose-built and optimized for synchrotron radiation. Nonetheless, synchrotron radiation laboratories worldwide are still frequently mistaken for particle physics labs, and newcomers often wonder what biologists are doing at large accelerator laboratories. The increased utilization of large scale research infrastructures and sophisticated instrument complexes in a wide variety of disciplines (including biology) is, however, merely one example of the general trends in the dynamic developments of science with which this thesis is also concerned.

Laboratories are established fundamentally because of their material qualities: their purpose is to provide material or technological arrangements attractive to researchers. Synchrotron radiation laboratories are *dynamic* and *contingent* in this respect – because they are scientifically multipurpose and technologically interchangeable. To maintain the ability to control and modify the material and the technology, and to adjust different activities and pur-

1. The number of labs refers to the number of *user facilities*, i.e. laboratories that have a clearly stated purpose of accepting external researchers and their projects, see appendix. The number of users is a rough estimate; in a 2004 brochure issued by MAX-lab in Lund, Sweden the global number of synchrotron radiation users is approximated to 20,000 (*MAX IV brochure 2004*, p 6). It is reasonable to suspect that the number has grown significantly since then.

poses to each other, social structuring is needed: institutionalized practices, organization, and politics – all are required but determined by the fundamental constraints of the laboratory's material and technological setting.

The range of possible interpretations is wide – with regard to the content and framing of the science, the design and function of the technology, the practice and sociology of the experimental work, the importance and influence of the political decision-making, and the perception and understanding of the relationships between all these instances that collectively shape the laboratories. At the same time, however, these laboratories are spatially, materially and organizationally constrained workplaces of science. Within their walls, they enclose all the variations and make up a single entity that can be conceived as one scientific laboratory, one technological construct, one sociological structure, and one political asset. By examining and analyzing this unifier of disunified activities through its politics and practices, the thesis aims at making a contribution to the understanding of the political sociology of science.

Shaping a laboratory

Aim and structure of the book

This book is about a modern scientific phenomenon – synchrotron radiation laboratories – and its contextual position in science and science policy. The aim of the thesis is to describe and analyze synchrotron radiation laboratories as *features* and *products* of modern science and science policy, but also as embedded *contributors* to the shaping of them. The analysis is grounded in an understanding of laboratories as *material and social institutions* in science, where certain scientific practices are developed and refined, as part of science at large and as part of a science policy context. Synchrotron radiation laboratories were chosen as study objects because they are large and expensive, multipurpose and dynamic, and because they have grown over time in number, size and areas of utilization – thus indicating their growth in relative importance in the sciences, a growth that is still underway. A significant part of the thesis is devoted to detailed accounts of the laboratories' historical roots, relative position in the sciences, and basic technological features, also through the examples provided by the three case studies. Only when the laboratory itself is described and examined can its politics be analyzed in a useful manner.

Synchrotron radiation emerged as a peripheral activity at particle physics laboratories in the 60s and 70s and grew gradually through the decades into an area of its own, with established user communities and laboratory complexes. This process has been described as synchrotron radiation going from 'esoteric endeavor' to 'mainstream activity'² and is in itself an interesting example of how a potentially multidisciplinary and generic experimental technique may emerge and become institutionalized among the scientific fields it serves in the shape of organized laboratories. But the process did not occur in isolation, and parallel changes in the dynamics of science and in the politics of science should therefore be taken into account in the analysis.

2. The expressions 'esoteric endeavor' and 'mainstream activity' are taken from a summary of synchrotron radiation and its utilization in a 1997 report (*Birgeneau-Shen Report 1997*) written by an expert panel on mission from the United States Department of Energy, sponsor and steward of four major synchrotron radiation facilities in the US. See chapters 3, 4, and 7 for further details.

The thesis aims to show that synchrotron radiation laboratories embody and manifest changes in science dynamics and science policy, and take part in shaping them.

The analysis of the institutionalization of synchrotron radiation is organized around three themes, which have been identified out of the body of theory in the field of science studies. They are main parts of the analysis and they connect the empirical findings with the theoretical framework and the eventual conclusions. The three themes are:

Changing dynamics of science, with regard to sociology, organization and practices of scientific undertakings.

Changes in science policy, especially the politics of scientific infrastructure and the 'big science' – 'little science' dichotomy.

The role of *scientific entrepreneurs*,³ i.e. particular actors in science to whom initiative and the role of enabling the pursuit of initiatives can be attributed.

These three themes of change are interpreted as cooperating and correlating in the creation and establishment of synchrotron radiation laboratories. Throughout the thesis, actors and processes are identified in relation to these themes and interrelated to create an analytical whole and a coherent, synthesized understanding of the laboratories and their context(s). More specifically, the analysis seeks to answer the following questions:

How did the institutionalization of synchrotron radiation in science and in science policy happen? What are the chief characteristics of synchrotron radiation laboratories and how can they be contextually explained? What actors (scientific entrepreneurs) can be identified especially important in the process of institutionalization and in the running and continuous renewal

3. The word 'entrepreneur' is generally ambiguous and requires some clarification. A classical meaning of the word is 'undertaker', and in some historical contexts the undertakings of entrepreneurs have been associated with some risk-taking; 'entrepreneur' being equal to 'adventurer' (Landström 2005, pp 8-11). By the incorporation of the entrepreneurship concept in economic theory, 'entrepreneur' became an occasional synonym for 'businessman'. Some economists however also identified 'entrepreneurs' as agents of innovation and change in the economy, risk takers, seekers and exploiters of opportunity, and coordinators and organizers of resources (Ibid., pp 13-20). The use of the word here and throughout the thesis seeks to emphasize all of these; 'scientific entrepreneurs' are identified as agents of change in science and particularly in laboratories, who show opportunism, some risk-taking and the ability to purposefully enroll and coordinate resources.

of the laboratories? To what overall trends in science and science policy – historical and contemporary – can the institutionalization be attributed? How do synchrotron radiation laboratories embody these trends?

In the present chapter, a theoretical framework for the study is developed. Concepts and theories are presented that will be used to contextualize the findings and aid in the answering of the questions above. The three analytical themes described are identified and defined on the basis of a description and interpretation of selected approaches and ‘schools’ in the fields of science studies and science policy studies. In chapter 2, a historical backdrop is established, including both the scientific and the political state of affairs in which synchrotron radiation emerged, and the specific political settings of the three cases. Chapter 3 answers to the ambition to understand synchrotron radiation laboratories also with respect to their science and technology and basic organizational features, and thus it contains general descriptions of the scientific and technological basics of synchrotron radiation and their development over time, as well as descriptions of fundamental organizational characteristics.

In chapters 4, 5 and 6, three case studies are presented. These chapters contain the major part of the empirical findings and constitute the basis for the analysis and the answering of the research questions. The cases are chosen and utilized to be *complementary* and *parallel* rather than *comparative* or *sequential*. All three chapters give inclusive and detailed insight to the particular laboratories they describe. They contribute to the overall analysis by focusing on specific observations and conclusions that complement each other to form a whole picture. These conclusions are *selective*, i.e. chosen and advanced for the purpose of contributing to the overall analysis. The conclusions of the three cases may therefore appear asymmetrical or incoherent, and with a comparative approach this would have been a weakness of the study. However, since the aim is rather complementarity, asymmetry is merely a sign of care and precision in the selection of particularly important and interesting contributions to the overall conclusion of the thesis. The concluding analysis and discussion in chapter 7 summarizes the selected findings, makes a few additional observations of interest for the conclusions, and relates back to the purpose of the study by further developing the three analytical themes and answering the research questions.

The stating of the aim of the study, the identification of the analytical themes, and the formulation of the research questions above have been done out of a need to structure and organize a rich body of collected material. Aim and research questions have been distilled from a rather chaotic general understanding of the subject and the significance it has for science and for science policy. This reinforces an already strong need for a thorough method

discussion, and such a discussion is found at the end of this chapter. For now, it suffices to state that the method used for this study is multifaceted and complementary; it consists of synthesized observation of laboratory environments, recorded interviews and discussions with scientists, technicians, policymakers and administrators, and the wading through technical documents, colorful brochures, and governmental reports. The ambition coupled with the choice of this versatile method approach has been to add richness and variety to the study.

Science and technology

The body of theories concerning *science* and *technology* and their interrelations and position in society is large. A classic view establishes a linear causal relationship between the two: Either science *drives* technology and scientific discovery of nature is the basis of all (material) development, or technology *enables* science and science is ultimately dependent on technological innovation to develop. One of the most popularized images of what roles science and technology occupy (or should occupy) in society and what their relationship is (or ought to be) is the classic notion that technology is *applied science*, that science provides readymade pieces of knowledge for engineers and industrialists to cultivate for practical ends, and that (basic) science is what should drive technological development for the benefit of society (e.g. Baird 2004, p 18; Stokes 1997, pp 10-11). This idea – the *linear model of technological innovation* – earned great appreciation in society and politics during the 20th century and had significance for the establishment of modern science policy (see chapter 2). Many authors have declared this model erroneous or oversimplified, among them Greenberg (1999/1967, p 29) in whose view “rather than a straight-line sequence from knowledge to utility, there has prevailed an interaction of such incredibly complex and intricate composition that it is rarely possible in examining any artifact or device to sift the science from the technology.”

Science and technology are thus inextricable parts of the same development or progress, and they are both ruled by a ‘principle of novelty’ (e.g. Weingart 2000, p 30), meaning that their purpose is development and advancement and that this purpose partly defines them. *Technoscience* has been suggested as an all-inclusive concept (see below), based on the view that the differences between *science* and *technology* or between *scientists* and *engineers* are merely institutional or disciplinary. As a profession or practice, technology is doubtless distinguishable from science in that its practitioners – we

might call them *engineers* or *inventors* – maintain and deploy their own body of knowledge and their own skills, cultures and modes of work which are quite separate from those of scientists and definitely not always based on the knowledge provided to them by science (Sismondo 2004, pp 76-77).

In sum, there is an interesting duality in the view on the relationship between science and technology, in that the two when treated or studied as human activities or cultures are distinct and separated, but when their relationship is discussed, they are said to overlap and mix in nearly every aspect. The tendency is also that attempted labeling is done primarily by scholars and outside observers rather than the actors or practitioners themselves: “Persons committed emotionally and intellectually to problem solving associated with systemic creation and development rarely take note of disciplinary boundaries, unless bureaucracy has taken command” (Hughes 1987, p 64).

Science and technology studies

Until the middle of the 20th century, scholarly study of science was virtually monopolized by philosophical epistemology, which treated science as unified, cumulative and rational, guided by one grand scientific method (although opinions differed about the details of this method) and uncontested in its objective claims about nature (Hacking 1983, pp 5-6). Philosophical epistemology held that theory guided all science, and that experiments were designed to test theory. The first major break with this tradition came with Thomas Kuhn (1996/1962), who turned the attention of science studies to organizational dynamics of science and laid emphasis on ‘consensus’ rather than ‘truth’ as the ‘product’ or ‘commodity’ in science. Kuhn’s model of the continuous renewal of the contents of science through succeeding periods of ‘normal science’ and ‘revolutions’ suggested that instead of ‘truths’ continuously and cumulatively being added to science through better and better descriptions of nature, it is rather the collected beliefs held by practitioners of a science at a given time in history, within a given ‘paradigm’, that counts as ‘truth’ (Kuhn 1996/1962, p 170).

Kuhn’s views “violated almost everybody’s ideas of the rationality and progress of science” (Sismondo 2004, p 19), and their significance is underscored by the occasional epithet the ‘post-Kuhnian paradigm’ for modern science studies. At least as important with regard to the sociological understanding of science was the sociologist Robert K. Merton’s works in the 40s and on. In Merton’s ‘school’ of science studies, academic science was a distinguished social system, coherent in its institutional structure, largely self-

organizing but ultimately governed by a set of norms.⁴ The quest for professional credit, sometimes aggregated to the level of eponymy but in the most cases simply the award of peer recognition, was according to Merton what kept scientists in the profession, in the community, and in compliance with the norms (Merton 1973, pp 267-278). With Kuhn having removed 'truth' as the principal guiding star in science and Merton acknowledging science's function as a social institution, the stage was set for a major change of the perspective and framework of scholarly studies of science.

And without doubt, change occurred. During the past four decades, scholarly approach to science as a philosophical, sociological and societal phenomenon has been in a prolonged, "major (and probably irreversible) rupture" (Pestre 2004, p 351). This rupture has taken the image of science far away from philosophical epistemology's old ideals. Perhaps most discontinuous in terms of general view on science was the 'sociology of scientific knowledge' ('SSK') movement that stripped science of its unique status, by claiming that scientific research was just another human undertaking among many others (Bimber and Guston 1995, p 554). The SSK movement brought enough controversy to science studies and its adjoining fields by its invoking of all kinds of postmodern and post-structuralism interpretations of things like 'knowledge' and 'facts' (e.g. Hacking 1999) to set off the so-called 'Science Wars' – the polarized debate between scientific realism and positivism on one side and relativism or social constructivism on the other (e.g. Labinger and Collins 2001). These 'Science Wars' not only showed a lack of self-criticism among the positivist partisans but also exposed certain inadequacies in the sociology of science. Flooded with all kinds of 'perspectives' and 'approaches' to science and its content, context, and practice the empirical investigations of these sociologists of science did not manage to deliver in accordance with their theoretical and methodological ambitions – they set out to deconstruct the construction of scientific facts, but ended up rather with ethnographic descriptions of scientists and their workplaces (Doing 2007; Hacking 1999; Lenoir 1997).

4. These norms are commonly lumped together under the acronym 'CUDOS', and are called *Communism* (a piece of scientific knowledge is common property and not the exclusive possession of its author), *Universalism* (scientific claims shall be viewed and evaluated regardless of its author's personal traits), *Disinterestedness* (scientists are supposed to leave out personal preferences of ideology or the like when making scientific claims), and *Organized Skepticism* (new scientific claims shall be met with reasonable doubt from the scientific community and undergo appropriate scrutinizing) (Merton 1973, pp 270-278). It is important to remember that neither these nor other 'norms' are claimed to be anything like 'rules' or undisputable characteristics of the scientific community (although Merton himself approached such an interpretation of their function) but just norms thought to *guide* scientific work, and from which exceptions naturally exists. See, for example, the discussion in Sismondo (2003, pp 20-32).

This transformation of science studies “did not happen in a social vacuum” but was of course reciprocal with dramatic societal changes and changes in the relationship between science and society (Pestre 2004, p 352). In the 60s and 70s, criticism of science as an institution in the service of the establishment was brandished in leftist, feminist, anti-war, and environmentalist movements. The established order was challenged, politically but also epistemologically. The SSK movement’s questioning of the “official epistemology of science” had great impact (Ibid.) but was also in turn inspired by the view of science and technology as “weapons in the hands of aggressive and repressive national states” (Giere 1993, p 105).

Science and technology change

“Over the twentieth century, *every aspect* of the shape and size of science has changed” (Cozzens 2003, p 127, emphasis added).

The 20th century saw a development and change of science and technology occurring with an unprecedented pace and with effects reaching further than anything before it. Derek J. de Solla Price (1986/1963) contributed to the establishment of modern science studies by the application of *bibliometrics* as a method for measuring science and scientific impact, and showed by measuring science according to three size parameters – money, manpower, and publications – that modern science had shown exponential growth patterns during its roughly three hundred years of existence: “[I]f any sufficiently large segment of science is measured in any reasonable way, the normal mode of growth is exponential” (Price 1986/1963, p 4).

Quantitative changes will doubtless induce and correlate with changes in qualitative aspects such as organization, modes of work, institutional dynamics, supply of infrastructure and use of instruments, career paths, and interfaces with society, politics and the public. Such qualitative changes are significant in the first of the three analytical themes – the changing dynamics of science. Quantitative and qualitative change in science has brought about a number of approaches, among which the *Mode 2* (Gibbons et al 1994; Nowotny et al 2001), *Systems of Innovation* (e.g. Lundvall 1992; Nelson 1993), and *Triple Helix* (e.g. Etzkowitz and Leydesdorff 1997, 2000) concepts are among the most widely used. Core to these theory constructs is the notion that science, technology and society – and their mutual relationships – have changed. Common to them is their focus on *innovation* as the motivation or rationale for science and technology, corresponding to the shift in policy

and public attitude towards science that has taken place during the past few decades and lead to a general demand for more visible returns from science (e.g. Freeman 1988, Guston 2000; Smith 1990; Pestre 2005). But innovation study is by definition curtailed in a very important sense because it “focuses only on the productive aspects of science” (Guston 1996, p 229) and leaves no space for the study of science as a societal and political phenomenon in its own right.

In connection with his identification of the unprecedented growth of science in the 20th century, Price (1986/1963, p 17) made the observation that the exponential growth in science was unsustainable by nature, at least with respect to its funding envelope. The starting point of analysis for John Ziman (1994, 2000) is the fulfillment of Price’s prophecy – the growth in expenditure on science has slowed down, and science now finds itself in a ‘steady state’ (Ziman 1994, pp 10-14) where it will be forced to “do with a fixed or slowly growing envelope of resources” (Ziman 2000, p 72). Simultaneously, science is changing internally, with old disciplinary constellations declining and new emerging and cross-disciplinary fields closer to commercial interests or fulfillment of immediate societal ends taking over (Ziman 1994, pp 3, 31-35; 2000, pp 70, 74). The inevitable effect of the internal restructuring and external financial pressure – public funding moving “from resource allocation to resource management” (Ziman 1994, p 120), i.e. the exercise of more control – is a painstaking renegotiation of science’s internal distribution of power, influence and prestige among its disciplines, institutions and professional positions (Ziman 1994, pp 75-82). A simultaneous ‘sophistication’ of technologies utilized by scientists – i.e. increase in complexity of instruments – partakes in forcing ‘collectivization’ upon the scientific enterprise. Together with resource tightening, the instrumental sophistication makes collaboration around expensive and scarce spearhead infrastructure and equipment necessary (Ziman 2000, p 69).

The ‘post-academic science’ that emerges out of all this is specialized and technologically sophisticated, collectivized, and signified by increasingly complex scientific problems and a growing need to utilize technically very advanced instrumentation and infrastructure (Ziman 1994, pp 43-49). The drive towards collaborations is also nurtured by the increased communications possibilities offered by information technology and the ‘research project’ as a prime organizational entity in science. While hardly existent some decades ago, the ‘project’ is nowadays the ruling form of scientific organization, and the ‘research grant’ – limited in time and scope – its principal form of funding (Ziman 1994, pp 122-123; 2000, pp 75-76). The limited time scope of projects and grants makes scientists more accountable, also with respect to ‘usefulness’ or commercial value of the science (Ziman 1994, p 141). As

a necessary follow-up of funding priorities, accountability was already an inevitable outcome of the transition to the 'steady state' (Ibid., p 253). This organization of science primarily in 'teams' and 'projects' and the overall collectivization has likely made much scientific activity more efficient, but it also entails the risk that grants are increasingly awarded to projects on specific societal problems of the politicians' or administrators' choice (Ziman 2000, p 76) which may threaten scientific breadth and scope or even scientific quality *per se*, if 'excellence' and returns are demanded without patience or comprehensive understanding of how it is really achieved (Ziman 1994, p 257).

However, science is also changing in culture. Different norms and ideals continuously emerge, compete and supersede each other and are applied momentarily on basis of opportunity and need. The 'steady state' is 'dynamic' in the sense that constant restructuring and rearranging give nerve and vigor: "[O]ne of the major characteristics of 'steady state' science is that all its outer and inner boundaries are open and indeterminate" (Ziman 1994, p 15). It echoes well the final sentence of Warren Hagstrom's (1965, p 296) book *The Scientific Community*: "it is not tension but the absence of tension that is symptomatic of the loss of values."

The 'science in a dynamic steady state' concept is central to the analysis of science's changing dynamics that make up the first of the three themes of the analytical pursuit of this thesis. Building on the Price legacy that science's growth cannot continue unhampered, it shows that slower growth or stagnation carries far reaching sociological changes – summarized in the concepts *collectivization* and *sophistication*. These two are fundamental for the understanding of the changes in science which places the scientific *collaboration* at the center and which will be further discussed below.

Disunified, social, and material

Philosophical epistemology attempted to separate science from non-science by referring to science's universal method (Hacking 1983, pp 5-6). According to Kuhn (1996/1962, pp 11, 163), science is separated from non-science by its adherence to *paradigms* (although the balance is occasionally tilted by scientific revolutions). Merton (1973, p 277) made attempts to use his norms (see above) for demarcation by referring to politically tainted science-like activity as "apparently scientific" and "unscientific". All three are unsatisfactory attempts to define science; philosophical epistemology because science does not adhere to any one universal method (e.g. Mulkay and Gilbert 1986), the paradigm theory because controversy is just as common to scientific prog-

ress as consensus (e.g. Dear 1995; Hacking 1996; Knorr Cetina 1981; Mulkey 1978), and the application of norms because norms are interpretable and possible to use differently in different contexts (Sismondo 2004, p 27; cf. also the previous section).

Thomas F. Gieryn (1983, p 781) has named the “debates over the possibility or desirability of demarcating science from non-science [...] ironic”; science obviously exists and has certain limits or boundaries regardless of conceptualizing attempts, and moreover, *scientists* are largely indifferent to such attempts (Palmer 1996, pp 57, 119; Thompson Klein 2000, p 13; cf. Hughes 1987, p 64, op. cit.). Attention should rather be paid to science’s “cultural space” (Gieryn 1999, p 5), a space that “acquires its authority precisely from and through episodic negotiations of its flexible and contextually contingent borders and territories” (Gieryn 1995, p 405). In other words, what science *is* depends on context and the actions of its individual practitioners. Rules most certainly exist, but it is with science as with any other game – “winning depends less upon the rules than on what is done within the space created by those rules” (Knorr Cetina 1981, p 128). The special *cultural* space science holds in society, suggested by Gieryn, is however important for the *authority* of science, and this point is further elaborated below.

The disunification of science has both theoretical and methodological consequences. Disunification indicates that there are a number of factors influencing scientific practice apart from the adherence to for example a theory paradigm, but it also suggests that there is a great deal to be learned from approaching scientists at work, with the ambition to discover what they really do and why. Some authors have developed the contingency argument further and established the view that scientific knowledge production is entirely a process of social construction (e.g. Latour and Woolgar 1986/1979, Mendelsohn 1977, Van den Daele 1977). The merit is perhaps that social impact is rightfully and forcefully inscribed into the procedures of science, but ‘social construction’ has unfortunately been mistaken for ‘fabrication’ and the whole effort misinterpreted as an attempt to do away with ‘reality’ through unhampered relativism. This is far from the objectives:

“While treating technoscience as socially constructed, I want to avoid the (to me nonsensical) claim that nature is simply an invented fabrication. A more pragmatically oriented realism emerges from consideration that the products of the sociotechnical systems we call ‘science’ and ‘technology’ work precisely because they are embedded in our practices and stabilized in our technologies for producing truth” (Lenoir 1997, p 47).

Scientific facts may or may not be corresponding to nature, but as claims they

would never become understandable, comparable, recognizable, deniable, functional, or successful had they not been established in a social context. *Social ordering* is one term used for this process by which scientific facts establish their trustworthiness in a social scientific environment by negotiation of their content and meaning – and hence validity – through social interaction (Law 1994). But the ordering is not merely ‘social’: “Rather, I argue, what we call the social is *materially heterogeneous*: talk, bodies, texts, machines, architectures, all of these and many more are implicated in and perform the ‘social’” (Law 1994, p 2, emphasis original).

Fortunately, then, science is not only social but also *material*, just like most other worldly things: “[T]he disposition of equipment and other accoutrements regulates human behavior in one way or another” (Livingstone 2003, p 18). And large parts of modern science are concerned with manipulation of artifacts rather than describing and depicting ‘nature’ (Knorr Cetina 1983, p 119). That is not to say that scientists can’t both manipulate the artificial and describe ‘nature’ – only that the *work* they do has a lot more to do with manipulation of invented material constructs than with reactively describing a world ‘out there’. Socially organized scientific activity, enabled and constrained by the material and technological, tends to happen in laboratories – spatially demarcated sites of scientific *work*. This leads to the next step in establishing the theoretical framework for this thesis; the notion that science to a large degree is made possible by material and social circumstances. The *disunification* of science and its idiosyncratic traits pointed out above are also important notions for the following discussion.

Laboratories

A significant share of scientific work *requires* specific material and social circumstances and places of its own, because it needs certain organizational constructions and technological setups. Although information, practices, instruments and people transcend all kinds of borders, scientific work does take place at identifiable geographic locations, for organizational reasons and for effective resource utilization (Henke and Gieryn 2007, p 355; Livingstone 2003). In cases where expensive or rare instrumentation is needed for scientists to do their work, *locality* becomes even more important, and thus one result of *sophistication* will be increasing concentration of scientific work to certain places, to counterbalance alleged *globalization* of science.

The ‘laboratory studies’ tradition (e.g. Knorr Cetina 1981, 1983, 1995, 1999; Lynch 1982, 1997/1993; Woolgar 1982, 1991) is grounded in the asser-

tion that the inquiry of what science *really is* must be an inquiry of ‘unfinished knowledge’ or ‘science in the making’, which refers the scholar to sites of scientific *action*, i.e. laboratories (Knorr Cetina 1981, p 20). However, the reasons for turning to the laboratory are also methodological, because first-hand studies of scientific action supposedly yields a more accurate or ‘undistorted’ picture (Woolgar 1982, p 484; Lynch 1997/1993, pp 270-273). Laboratory studies scholars made contributions to science studies that confirmed the *disunity* of science and its *social* and *material* aspects discussed in the last section. They concluded that scientific method is “context-impregnated, rather than context-free” and “rooted in a site of social action” (Knorr Cetina 1981, p 47). They found that the manipulation of nature in a crafted ideal laboratory environment was as much – if not more – a source of knowledge about nature as was plain observation (Hacking 1983, p 149). They found that nature in fact “is not to be found in the laboratory” (Knorr Cetina 1981, p 4), because laboratories “are based upon the premise that objects are not fixed entities that have to be taken ‘as they are’ or left by themselves” but rather can be manipulated and worked with through their representations in the form of “visual, auditory, or electrical traces” and through “their components, their extractions, and their ‘purified’ versions” (Knorr Cetina 1999, pp 26-27; similarly noted by Hacking 1983, p 226). They confirmed that “the quest for truth which is customarily ascribed to science” was hard to find in the laboratories they visited and that if laboratory activity is governed by any one principle, “it is the scientists’ concern with making things ‘work’, which points to a principle of success rather than one of truth” (Knorr Cetina 1981, p 4; identical observation in Mulkay 1981, p 164).

This last conclusion further elevates the *material* aspects of science, and is of great importance here. The notion that much scientific undertaking is rather focused on *making things work* than on reaching a particular answer is important for the understanding of the material aspects of science and will be further discussed below.

Another tenet of laboratory studies that will be returned to is that the fundamental reason for the existence of laboratories is their hosting of scientific equipment, and in the extension of that, the need for technologically skilled personnel to construct and maintain this equipment and act as interfaces between it and the scientists supposed to use it:

“The production of experimental facts is inescapably tied to the reproduction of equipment, with all the circularity inherent therein. In a fundamental sense, laboratory knowledge is local knowledge. It is bound up with particular practical know-how, with the on-site availability of appro-

priate bits of technology, and with knowing one's way around machines" (Livingstone 2003, p 142).

Technological know-how, scientific ambitions and agendas, material possibilities and constraints, and the social factors inscribed in each of these parts make up a heterogeneous laboratory environment that Peter Galison (1997) conceptualize in his model of the *material culture* of laboratories, that consists of (*material*) *subcultures* and the *trading zones* in which they meet and negotiate the progress of the laboratory and its scientific activities. Subcultures and their internal developments do not cohere by default but follow their own patterns, but according to Galison the continuous interaction between subcultures at various stages of their periodic developments is what ultimately drives scientific development. Subcultures are to be found within, between and across traditional disciplines or subdisciplines, and they are mostly material or instrumental to their nature. They tend to develop around experimental practices or instrument traditions, and they have their own traditions and complete set of practices – everything we perhaps would call *culture*. And they correspond to the material heterogeneity and complexity that is a salient feature of sophisticated and collectivized science done in laboratories.

Internal politics

The context-specificity of science and the 'blackboxing' power of laboratories was first pointed out by Bruno Latour (1983) and has since been discussed by several science studies scholars (e.g. Secord 1994, Livingstone 2003, Henke and Gieryn 2007) and not least the proponents of so-called Actor-Network Theory (ANT). The 'excluding mechanisms' that divide laboratory insiders from outsiders plays a role in the creation of solid validity and legitimacy around scientific claims, because credibility is built partly by the control of the social and material settings and the ability to display such control.

Actor-Network Theory treats the material and social settings of a laboratory and the activities they facilitate as a whole and denotes it *technoscience* (e.g. Latour 1987; Law and Hassard 1999). The *actors* are human (experimenters, technicians, laboratory assistants, and so on) and non-human (instruments, samples and other material objects), and they engage collectively in a (social) *network*, which as a whole undertakes the fact-constructing of practical scientific work in laboratories (Law 1994, p 95). The (techno)scientist with the ability to *enroll* actors in the strongest alliance (network) will build the stron-

gest case, and thus the most solid fact, and has the greatest chances to win a possible scientific controversy (Latour 1987, pp 58-62).

Two points are of interest here. First, Actor-Network Theory shows that *alliances* are important, that credibility can be established around specific scientific activities and facts by enrollment of resources to establish a strong case. This is of some significance for the *scientific entrepreneur* analytical theme, because the pursuit of the scientific entrepreneur to a large degree is concerned with enrolling and coordinating material and social resources for the sake of promoting her 'cause' or 'interest', also in cases when this cause or interest has an altruistic dimension.

The second point is that the aim of laboratory studies to study the practices of science and its material and social settings rather than its epistemological dimensions has contributed in creating a sociology of science that is not a sociology of knowledge (Fuller 2006, pp 28-29). When relieved of the burden of explaining what 'knowledge' *is* and how it is produced and disseminated, science studies can rightfully turn its attention to the politics of science, the policymaking around science, the institutional arrangements of science, and the organizational dynamics that flow from it (Frickel and Moore 2006b, pp 6-9). Such a change of perspective or repositioning of the field is desirable because science is deeply embedded in society and shares so many social and political features with it (e.g. Blume 1974; Frickel and Moore 2006a). The workplaces of science, where scientific activity is institutionalized, organized, and contextualized, are natural study objects also for studies of science policy and the dynamics of science that are shaped in the nexus of the social, material and political.

The details of the organization and institutionalization of science in laboratory environments have been likened with markets with trade commodities like 'credit' or 'credibility'⁵ which is the reward scientists seek and the ultimate rationale for their efforts (Latour and Woolgar 1986/1979, pp 187-201). Earlier scholars developed similar models, but did not overlook the fact that science is also a *job* and that material or economic reward is just as important (Hagstrom 1965, pp 54-55). There is frequent realigning of research topics, questions, and problems to standards and demands defined by funding agen-

5. In his studies of the trading of credit and credibility in science, Merton (1973, pp 443-459) identified a *cumulative advantage* that he dubbed the 'Matthew Effect' in science – "eminent scientists get disproportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions" (Ibid., p 443). The term 'Matthew Effect' alludes to the following passage in the gospel of Matthew (King James Bible, chapter 25 verse 29): "For unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath" (quoted in Merton 1973, p 445).

cies, department heads, and journal editors that might make them deviate away from the original wants of the scientist, but that are inevitable because of the multitude of inputs and their importance (Knorr Cetina 1981, p 82). But potential influence of the scientific community, the science policy system, and the surrounding society can also be interpreted as parts in a network or alliance *enrolled* by certain actors for the promotion of a scientific project, field, instrument, career, or laboratory. Such actors are exactly what is meant by *scientific entrepreneurs*, and their role in the shaping of scientific institutions is a vital part of the analysis in this study.

External politics

“Don’t underestimate the power of greed in the halls of science or the wholesome presence of altruism and self-respect. And don’t overlook shame and embarrassment as forces for good behavior in scientific affairs” (Greenberg 2007, p 1).

Though provocative, this quote fills the purpose of showing that science – when it comes to moral and integrity – may not be different at all from other professions or human activities. Both reputation and money are important motivating factors in science, and the demands for contribution to development and progress constantly placed upon science from various parts of society adds to its productivity orientation. Science is a profession, a public service, a means through which political goals may be reached, and a societal institution with persuasive powers (e.g. Blume 1974; Frickel and Moore 2006a). In short, science is *political*.

The second analytical theme is concerned with the political dimensions of science and the governing of science by politics, and changes they have undergone. The ‘principal-agent’ analytic interpretation of science policy (Guston 1996, 2000) holds that the government (the principal) is hiring the science (the agent) to carry out certain tasks desirable in society that it cannot do itself (Guston 2000, p 15). It is a conceptual and analytical extension of the so-called ‘social contract for science’, the general arrangement between science and the state that is put in place in most (western) democracies, relying on the governmental patronage of basic science. It is supposed to regulate the exchange of this patronage for general contribution to the wealth and welfare of society and the deployment of scientific expertise by the political powers to the achievement of their goals (Guston 2000, pp 37-45; Elzinga and Jamison 1995, p 574).

Built into this relationship is the potentially problematic issue of ‘information asymmetry’. The agent possesses knowledge and understanding about its own activities to a larger degree than the principal, otherwise there would be no reason for the arrangement in the first place. The information asymmetry skews the ideal relationship because it limits the principal’s ability to control the agent and hold it accountable, and it “presents the central problem of science policy” (Guston 1996, p 230). Another central problem in science policy, more explicitly concerned with the funding stream incorporated in the principal-agent relationship, is science’s apparently never-ending need for more money. Because of its nature – science is by default always engaged in ‘progress’ (e.g. Weingart 2000, p 30, op. cit.) – there can be no halt to its expansion ambitions, and so “more is never enough”; science will always expand to absorb as much money as is made available for it (Greenberg 2001, p 7; 2007, p 29).

Scientific activity is thus not funded solely on basis of a careful analysis of what it needs in order to fulfill a certain goal (whatever the nature of such a goal) but the sheer *availability of money* for it. The size of the funding envelope is determined by the considered opinion of a government or a society how much money it is willing to spend on science. At the core of science policy lies therefore the need to make *priorities* – between disciplines and fields, between institutions and groups, between possible strategies, and between different long term investments like the commitment to construction and operation of large scale scientific facilities or particularly expensive laboratory infrastructures. When society’s expectations or demands for returns for investment are intensified, the ability to make priorities becomes increasingly important. The ‘information asymmetry’ and the ‘more is never enough’ tendency challenges this ability on part of policymakers.

The ‘social contract for science’ was put in place largely as an outcome of the mighty demonstration of the potential political, economic and not least military power of basic science by the events of World War II, and this instigation of the social contract is briefly accounted for in chapter 2. The foundation for the social contract was the ‘linear model’ of technological innovation (see above) which prescribed that pure, curiosity-driven basic science should be the prime concern for the government to support because it will give rise to applied science, leading to technological applications, invention, and eventually societal benefit (Fuller 2000, p 118; Calvert 2002, p 46; Dennis 2003, p 7). ‘Basic science’ was thereby established as a concept and given an important societal function, and the ‘social contract’ for science was made imperative: The very meaning of ‘basic science’ presupposed an independent status, the only patron capable of promising such independence was the government, which therefore should pay but leave the governing of science to

the scientists (Guston 2000, p 59; Greenberg 1999/1967, pp 107, 112-114; Greenberg 2001, p 44).

The accuracy of the linear model has been questioned numerous times since its inception (Stokes 1997, pp 61-63), but it has nonetheless prevailed as a basic framework (Ziman 2000, pp 15, 54), as has the fundamental notion of governmental patronage of (basic) science. In most countries, science has been a policy area and a branch of government since World War II, and investments in science are made on basis of political considerations. In the 'dynamic steady state' described by Ziman (1994, 2000; see above), induced by a slowdown or halting of the growth of funding, strategic choices on political level are becoming even more important and the effects of them more far-reaching. One central clause of the social contract for science, that science is best governed by scientists, has been partly abolished by increased demands for accountability and a reorganization of the funding and governance of science into a kind of market economy. As mentioned, a thorough change of science's role in society and the 'terms' of the social contract has taken place, although its point in time is not unambiguously identified and its content and shape is interpreted very differently among analysts. It is however clear that the "move towards predominantly operational values" in science (Pestre 2005, p 38) has led to an abolishment of traditional cultural values of science as a vaguely defined 'common good' to a force for economic growth and societal utility (Guston and Keniston 1994b, p 22; Elzinga and Jamison 1995, p 594; Shapin 2008, pp 213-215).

Accordingly, the competition between *groups* for time-limited *grants* to cover for the carrying out of *projects* has almost entirely replaced tenured professors' long-term style of work. This is one of the most visible features of the changing dynamics of science that make up one of the analytical themes of this study. It is also intimately coupled with the changes in science policy detectable in one of the other themes: Increased demand for returns from the society whose support science is dependent on and increased competition due to limits of growth and the fundamental principle of priority in science policy has evident effects on the dynamics of science. Thus two central patterns of change – corresponding to two of the three analytical themes in this thesis – have been established.

Scientific collaboration

In the dynamic steady state, available funding for science is not only limited but also increasingly distributed among projects and teams rather than

through traditional, macrostructure, patronage channels like lump sums to broad academic programs over a long time (Ziman 2000, p 76). This inevitably leads to increased competition, which is also strengthened by sophistication that forces scientists to establish new connections with peers, other groups and institutions in new contexts and on basis of strategic choice, to optimize the conditions for their research and compete on a more or less globalized stage (Ziman 1994, pp 53-54). When expensive equipment is involved, the new institutional construction that has emerged as the prime contracting entity in which science is nowadays planned, carried out and reported is the *collaboration*.

As an organizational strategy, intended to enhance the collective capabilities of its participants through mutual exchange of expert knowledge and competence, collaboration is a logical response to increased competition for resources and rapid developments that threaten to leave individuals behind (Genuth et al 2000, p 312; Shrum et al 2007, pp 29-66; Swatez 1970, pp 56-57). While collaborations might be a generic strategy for increasing efficiency of resource utilization, it may also be grounded in a need for complementarities in knowledge, skills, material assets or access to immaterial resources like professional networks or the aforementioned, heavily sought-after 'credibility'. Scientific collaborations exist in a variety of forms and they are more likely to form around existing relationships than being instigated by external or superior authority (Shrum et al 2007, pp 29, 114), although for example allocation of funding may have a decisive role both in inciting and enabling collaboration.

Warren Hagstrom (1964, 1965) identified three partly overlapping factors that has created inescapable mutual interdependences between scientists and made collaboration a necessity in science:

"First, scientific facilities are becoming much more expensive, and access to them and to research grants has become a critical problem to many scientists. Second, modern scientific techniques and instruments require skills not possessed by a single individual, and scientists often require the technical assistance of professionally trained persons. Third, more research is done in interdisciplinary areas, where the skills of scientists from two or more disciplines are necessary" (Hagstrom 1964, p 251).

All three echoes of Ziman (1994), but Hagstrom (1964) identified them thirty years in advance. Collaboration *per se* may not have sharing of technology as its rationale, but when technological sophistication is furthered, collaboration does become the dominant organizational form in science. Mutual interdependence is created out of the need for division of labor and comple-

mentarities in knowledge and skills (Shrum et al 2007, p 119). When access to state-of-the-art instrumentation is becoming increasingly restricted, the skills required to operate such instrumentation goes beyond the capacity of a single person, and the scientific projects for which grants are awarded are increasingly interdisciplinary, the scientist will have no choice but to engage in a collaboration based on “task heterogeneity” (Duncker 1998, p 83). Initiatives to collaboration are also increasingly taken at the organizational level, for the very same reasons of interdependence and division of labor (Shrum et al 2007, p 119). The collaboration is perhaps the most visible manifestation of the changing dynamics of science that constitute one of the analytical themes in this thesis. The size and nature of collaboration and the details of institutionalization and organization of collaborations are important analytical tools for the study of multidisciplinary and large scientific laboratories. A brief detour over an extreme collaboration is however helpful for the perspective and will also provide some theoretical foundation for the historical and contextual positioning of synchrotron radiation laboratories.

Big science and big physics

An extreme form of scientific collaboration, to which a lot of attention has been devoted in science studies and the history of science, is so-called ‘big science’. Especially high energy physics has drawn considerable attention (Chompalov et al 2002, p 751), as a discipline where the instrumentation grew beyond ‘industrial’ sizes in the second half of the 20th century, and accordingly created collaboration groups of not only hundreds but thousands of researchers, engineers, administrators and technicians. In these fields, the sophistication of instruments and their growth in size pushed collectivization of scientific work towards an extreme, and created a scientific field where it is impossible to do experimental work except in a collaborative context (Galison 1997, p 578; Knorr Cetina 1999, p 167; Shrum et al 2007, pp 202–203; Ziman 2000, p 69).

It has been suggested that high energy physics after the construction of the major single-purpose laboratories in the 60s and 70s (see chapter 2) should be named ‘megascience’ to distinguish it from previous, comparably modest ‘big science’ (Hoddeson et al 2008, pp 281, 309–311). Authors have claimed that the extreme collectivization of high energy physics created entirely new self-understanding in the concerned fields (Knorr Cetina 1999, p 165), including a significant reducing of the individual’s status as epistemic subject and its replacement by the collaboration or even the *experiment* (Galison 1997, pp

635-636; Jungk 1968, p 144; Knorr Cetina 1999, pp 117-122, 167-168), a development perceived as threatening to science itself by both scientists within the field and outside it (Galison 1997, pp 370, 372-374, 417).

The term 'big science' was coined by one of its fiercest critics, Alvin Weinberg, who expressed concern that science would become increasingly bureaucratized and taken over by administrators and technicians at the expense of inspired individual scientists (Weinberg 1961, p 162; 1967, p 43; see also Hagstrom 1965, pp 153, 294; Shapin 2008, pp 170ff). The expression 'big science' has since then been used in a variety of attempts to conceptualize or popularize the new collectivized scientific endeavors that flock together around majestic pieces of infrastructure.

After Price's (1986/1963) and his followers' efforts to measure growth in science, little doubt remains over the fact that science is 'big' in one way or the other. There is, however, a difference between such an observation and a functional or helpful use of the term 'big science' in analysis, because as a term, 'big science' has become captive of all kinds of sociological and historical interpretation. The popularization of 'big science' as a term and as a concept increases its "analytical intractability" (Capshaw and Rader 1992, p 4), and so "even after hundreds of pages of text, 'big science' itself remains an elusive term" (Hevly 1992, p 355).

Big science and especially high energy physics has enjoyed an extraordinary position in science and in society, and with respect to its contents and culture high energy physics is a singularity. Funded by governments on basis of remote but apparently valid connectedness to nuclear energy and thus national defense, it is simultaneously argued to be one of the most 'fundamental', 'basic', or 'pure' branches of the modern natural sciences (Galison 1992, p 5; Greenberg 1999/1967, p 210; Greenberg 2001, p 404; Westwick 2003, p 165). The hubris associated with high energy physics is illustrated by many of the accelerator projects of the past decades, primarily perhaps by the failed Superconducting Super Collider (see chapter 2), but the hubris has got philosophical dimensions as well. High energy physics has been referred to as the spearhead of science's penetration into the unknown, along with claims that other scientific disciplines are subordinate in importance and in the capacity and *intelligence* of its practitioners, as noted by Traweek (1988, p 79; similar observation in Widmalm 1993, p 108).

Despite all elusiveness and ambiguity, big science exists and can be put in a historical, political and sociological context. According to Price (1986/1963, p 29), big science should perhaps be seen as an "uncomfortably brief interlude between the traditional centuries of Little Science and the impending period following transition", suggesting that not only must the expansion of

science in numbers sooner or later cease, but also as mode of work and organizational arrangement, big science is unsustainable.

The previously unhampered development towards larger and more expensive machines has come to a kind of end, and this will be discussed in chapter 2. If the new societal and political context for science entails an increased emphasis on strategic choices and accountability, as several authors (see above) have argued, perhaps Johnson (2004, p 226) is right in suggesting that high energy physics as a discipline belongs in the past time and in another science regime. That doesn't mean it will cease to exist completely, only that its dominance – politically, economically and culturally – is over.

Synchrotron radiation, emerging in the heyday of high energy physics and operating 'parasitically' on high energy physics machines in its early days, has been a successor of high energy physics in at least one aspect – the utilization of accelerator complexes. It has to a great extent developed in the shadow of high energy physics, as the coming chapters will show. For this reason – but also because big science functions as a point of reference when discussing scientific collaboration and the role of instruments and infrastructure in science – this brief theoretical discussion about big science and high energy physics is valuable for the following.

Experimental systems

In the previous discussions of the locality of scientific practice, one conclusion was reached regarding the impact of laboratories' material and social settings on scientific pursuits. As providers of opportunity, instruments occupy an instigator role in much scientific work (Van Helden and Hankins 1994, p 4). It is true for much of modern science that complete experimental arrangements are developed simultaneously with the outlining of the procedures and goals of the experiment, sometimes to the degree that it can be difficult to separate instrument from experiment (Smith and Tatarewicz 1994, p 101). This might lead to the dissolving of sharp boundaries between the professional identities 'instrument builder', 'experimentalist' and 'scientist' and the intensifying of negotiation or trading between needs and requests of scientists, technical capabilities and skills of technicians, physical and technological constraints of instruments and infrastructure, and the limitations of time and money (Ibid., p 108; cf. Galison 1997).

Partly in line with Actor-Network Theory (see above), authors have suggested that laboratories should be viewed as the agglomerations of extremely

specialized technologies that are utilized by groups of individuals with specific knowledge and skills to develop a kind of alliances necessary to produce scientific knowledge – a kind of *systems* of knowledge production. Rheinberger (1997) names them *experimental systems* – complete setups in which material and social resources in science and technology are combined to create a suitable and working experiment. They are chosen and refined by the scientist for her purposes just as theoretical frameworks are, and around or in them, experimental (sub)cultures evolve and optimize their intellectual and technical performance to make the experimental system *work* (Rheinberger 1997, pp 25–27; cf. Galison 1997). As mentioned, much science is about *making things work* rather than answering a specific question. Together this suggests that experimenters develop relationships to their experiments or instruments (experimental systems) that grow to become their primary professional identity. The scientific motivation of the experiment and the particular performance of the instrument converge and will ultimately overlap.

The general *disunity* of science (see above) is visible here; the mesh of variations of practice and conduct in the sciences does not neatly array itself according to traditional taxonomies of scientific disciplines. Rather, experimental practices *transcend* traditional disciplinary boundaries and create other more or less temporary entities, *subcultures* (Galison 1997) or “subcommittees with different knowledge-constitutive interests and experimental traditions, organized socially for access to different resources, and oriented around different repertoires of techniques and apparatus” (Lenoir 1997, p 26).

Authors have argued that what counters the disunity of science and the ephemeral character of its practices and culture is in fact instruments and technologies – because of their concrete physical character and their generic ability to serve many lords regardless of theory: “[I]t is not high-level theory that has stopped the innumerable branches of science from flying off in all directions, but the pervasiveness of a widely shared family of experimental practices and instrumentation” (Hacking 1996, p 69). Instruments are thus by their nature *transdisciplinary*, and other authors have claimed that they are entirely external to the theory/experiment divide and permeate the rest of science independently (e.g. Van Helden and Hankins 1994, p 6).

Instruments have historically not only been designed and developed entirely without having been ‘asked for’ by ‘non-instrumentalists’ but they have also produced scientific knowledge of great importance without being directed by theory (Baird 2004, p 187). Scientific change may be induced by the development and introduction of new instrumentation and experimental techniques. Synchrotron radiation laboratories are excellent examples of this, as the case studies and the analysis of the thesis will show. Technology

carries the ability to influence science in its institutions and organization, as illustrated by sophistication and collectivization, among other things. This is among the most important conclusion of this chapter, because it connects the emergence and establishment of particular types of laboratory infrastructures to the sociology of scientific undertakings and its politics.

Generic instruments and their promoters

The capability of instruments and other technologies to freely permeate disciplinary boundaries has given rise to the concept *generic instruments* – pieces of technology designed and developed for one purpose but subsequently used for other, or deliberately constructed to fill several, not predefined, needs. The concept is not restricted to (basic) science but applies to technology in a larger societal context, as Rosenberg (1992) has shown by pointing at the computer as the most spectacular example, a technology that has penetrated almost every part of society. Rosenberg also presents examples of inventions for experimental work in basic science that have been put to use for far more ‘applied’ purposes to show that instruments tend to “flow” between scientific disciplines and give rise to both interdisciplinary collaboration and forging of new disciplinary or semi-disciplinary entities (Rosenberg 1992, p 383; cf. Thompson Klein 2000, p 11).

Joerges and Shinn (2001b; 2001c; Shinn and Joerges 2002) attribute ‘generic instruments’ to certain *actors* who are the inventors and promoters of instruments and whose traveling with them through various contexts of science and technology can be traced. These actors are called “research technologists” and they “operate out of an interstitial arena that lies between the usual poles of interest and organization – university, firms, the state, military etc” (Shinn and Joerges 2002, p 207). They occupy a special position separate from but at the same time bridging the traditional realms and communities of ‘science’ and ‘technology’ (Joerges and Shinn 2001b, pp 3-8; Johnston 2001).

Generic instruments need not be *designed* to be flexible and open-ended, and their inventors need not be their sole promoters, also other actors can notice and exploit their potential. The main point is, however, that they can be made to fit in many contexts and serve many unrelated applications (Joerges and Shinn 2001b, p 3) and that the skills and capabilities of the promoter to facilitate their moving into new contexts is what enables the migration into entirely new areas of application. The generic instruments and their promoters constitute a *transverse* science and technology culture (Joerges and Shinn 2001c, p 244), transverse in the sense of crossing the boundaries of existing

disciplinary or cultural entities but not establishing their own. Generic instruments consequentially have the potential of serving a multitude of purposes *defined externally* to the instruments' own local context. The promoters of generic instruments are the 'practitioners' of a transverse science and technology culture, and operating in the 'interstitial' arenas make them turn up in different established scientific and technological environments, like for example different laboratories, or different parts of a laboratory.

The role of technology in shaping laboratory settings has been discussed in previous sections, as has the ability of the entrepreneurial individual to enroll material and social assets in the building of strong alliances with which to compete in a global (or local) quest for funding, recognition, or 'credibility'. The promotion of certain experiments, instruments, or scientific subfields may very well be areas of work for *scientific entrepreneurs*, who then will take on a significant role in the shaping of laboratory settings.

A political sociology of laboratories

The assorted excerpts from the state of the art of science studies presented in the previous sections constitute the theoretical framework for the thesis and the basis for the use of the three analytical themes. *Collectivization* and *sophistication* of science, used by Ziman (1994, 2000) as key features of the changes of science that amounts to the emergence of a dynamic steady state, have been identified as central concepts. They describe broad developments in science reaching deep into the sociology of scientific undertakings, and they have political explanations as well as political consequences.

Provoked by the inevitable halting of science's previous exponential growth (Price 1986/1963) and a general shift in society's expectations and demands on science (e.g. Freeman 1988; Guston and Keniston 1994b; Pestre 2005; Shapin 2008), sophistication and collectivization act out by forcing upon science a faster turnover of scientific projects and a greater demand for productivity and accountability, as well as indirectly a technical sophistication of instruments and infrastructure.

This development has a host of political and sociological dimensions. In order to comprehend it and to identify its effects in the general as well as specialized case, some fundamental features of modern science need to be acknowledged. Science is essentially *disunified* with respect to method, institutions, professional identities and values (e.g. Galison 1997; Gieryn 1983, 1995, 1999; Hacking 1983, 1996; Law 1994). The importance of *material and social settings* for scientific undertaking points out the important role of labo-

ratories as *workplaces of science* and manifestations of science's *cultural authority* and *localized* character (e.g. Galison 1997; Henke and Gieryn 2007; Knorr Cetina 1981, 1983, 1995, 1999; Livingstone 2003). If relieved from the task of explaining or unveiling the 'true nature' of scientific facts and how they are produced (cf. Fuller 2006, op. cit.), the sociology of science building on these observations might rightfully turn its attention to the material, social and political workings of science (cf. Blume 1974; Hagstrom 1965; Frickel and Moore 2006a). It is an essential part of the collectivization and sophistication concepts that the processes they describe entail material, social and political settings in constant interaction.

The basic sociological trend identified is the increasing *team-* and *project-*orientation of science. Collaborations built on effective resource utilization, *task heterogeneity* and *division of labor* become principal organizational units for scientific undertakings, and their structure is important both for performance and development of scientific fields (e.g. Genuth et al 2000; Shrum et al 2007; Swatez 1970). However, material and social settings of sophisticated instrumentation and research infrastructure have also been identified as organized (or ordered) in units – demarcated *experimental systems* where material and social settings are put to use for the fulfillment of scientific ambitions (e.g. Rheinberger 1997). Coupled with a dissolving of boundaries between *experiments* and *instruments* (e.g. Smith and Tatarewicz 1994) and the acknowledgement that much scientific activity is more about *making an experiment work* than answering a particular question (e.g. Knorr Cetina 1981; Mulkay 1981), experimental systems also become important *sociological* units in science and appropriate centers of attention for sociology of science.

The existence of instruments and infrastructure utilized for multiple purposes, so-called *generic technologies* (or *generic instruments*, the terms are used interchangeably) (e.g. Joerges and Shinn 2001b, 2001c; Shinn and Joerges 2002; Rosenberg 1992) and the role of instruments as *instigators* of scientific pursuits (e.g. Van Helden and Hankins 1994; Livingstone 2003; Galison 1997) are important concepts for the study of multidisciplinary laboratories and the identification of important actors in these laboratory environments. Experimental systems and generic technologies – if given wide definitions – show certain similarities with the alliances or networks of Actor-Network Theory (e.g. Latour 1987; Law 1994; Law and Hassard 1999). This is especially clear when acknowledging the importance of certain *actors* associated with the systems – *scientific entrepreneurs* running experiments, or promoting experiments or instruments to find new areas of utilization.

It is a basic premise of this study that all the processes described above are integral parts of a science that is *essentially political*, both in its internal workings and as an institution in society (e.g. Blume 1974; Frickel and

Moore 2006a; Greenberg 1999/1967, 2001; Guston 1996, 2000). Similar to the collectivization and sophistication processes for the changing dynamics of science, the acknowledgement of the essentially political nature of science is central to the analysis in this thesis. It provides a general analytical tool that builds on the distinction of the sociology of science as not a sociology of knowledge but an analytical study of the organizations, practices, and institutions in science.

Making sense of the laboratory

“Proceeding in this fashion guaranteed failure to achieve a definitive view of what’s going on out there. The subject is too big and too varied from university to university, and even within universities, to capture the whole story, which is rich in nuances, misleading appearances, hyper-polemics, self-delusions, deliberate evasions, and overlooked realities sitting in plain sight” (Greenberg 2007, p 8).

The stated aim of this thesis is both to understand synchrotron radiation laboratories as material and social institutions in science and to analyze their position in science and science policy systems. The introduction discloses that the aim and purpose, as well as the research questions, have been formulated out of a need to structure and organize a large body of collected material and a range of potentially useful theoretical concepts. The conduct of the study did, however, all along have the aim of understanding synchrotron radiation laboratories, through three case studies. The science and science policy contexts of the laboratories and the detailed sociology of the laboratories have thus been unveiled and mapped as the study has proceeded – therefore the three analytical themes of the thesis have been identified and concretized on basis of the material collected rather than the opposite.

Letting the empirical material guide the way in the analysis and presentation corresponds to the sociological and anthropological method *ethnography*, which is utilized when the purpose of research is to discover, describe and analyze how things are *done* (Grills 1998, p 7). Ethnographic study is a form of fieldwork study of environments and contexts with emphasis on the human subjects and their role in shaping the events that are studied (Vidich and Lyman 2000, p 40). Fieldwork studies are often done with a complementary set of method approaches, in order to broaden perspectives and maximize the level of detail – and in fear of simply missing important elements. Direct observation, document analysis and interviews are three main categories, and

a combination of various adaptations of the three is supposed to enable the fieldworker to crosscheck findings and let different sources validate and imply each other (Patton 2002, p 306). In this sense, the method used for this study resembles ethnography or *is* ethnography. With respect to purpose and attempted contribution, however, this thesis is not 'an ethnography', since the original meaning of that label rather points in the direction of cultural anthropology or naturalistic behavioral studies (Punch 1998, p 157).

The case studies were chosen successively in the course of the study, on basis of a wish to make them *complement* each other in terms of size and character of the laboratories, political context, and importance and interest to the general understanding of synchrotron radiation laboratories. Choosing case studies successively has some methodological implications, not least since the first case in question – MAX-lab, chapter 5 – came to be the 'entry point' to the field and at an early stage a 'test facility' for research questions and approaches to the topic. It became the main site for *participant observation*, one of the three major research methods used. Observation was required for the initial understanding of the laboratory environment that made the base for future investigation, as it provoked a range of questions that could be put to use in the interviews. MAX-lab also has a policy of openness towards visitors and granted almost unlimited access to the laboratory, which significantly aided in the initial studies. The second and third case studies were done under slightly different conditions, with limited access to the laboratory floor and shorter study visits to the laboratories. At the European Synchrotron Radiation Facility, located in the outskirts of Grenoble, France, the studies were done during two shorter visits of a few days each. An intensive interview schedule and only limited access to the laboratory floor due to a restricted visitors policy limited the opportunity for direct observation to a minimum. In the case of the synchrotron radiation laboratory at Stanford, California, the studies were done during a concentrated period of eight weeks. The laboratory was unfortunately closed to users during seven of these weeks,⁶ which severely restricted the ability to make useful observation of the activities on the laboratory floor. These differences in time and availability could nonetheless be balanced significantly by careful planning and scheduling. For both the ESRF case and the Stanford case, the limitations in time and access to interviewees could also be compensated by the availability of printed material, which is greater than in the MAX-lab case.

'Getting to know' the laboratory is important in order to understand it, and the researcher's increasing familiarity with her study objects is unavoid-

6. It is commonplace at synchrotron radiation laboratories to have so-called 'shutdown' periods for maintenance and upgrades of the accelerator and instruments. The stay at Stanford coincided with an annual shutdown period.

able and necessary but also associated with some risk. The so-called ‘observer effect’ – that the observer by her presence and act of observing inevitably effects the observed and thereby distorts their ‘naturalness’ (e.g. Angrosino and Mays de Pérez 2000, p 674) is a known methodological phenomenon and part of the explanation for the naming of the observation method as *participant* observation. Scholars in science studies and more specifically laboratory studies have acknowledged that one *has no choice* but to make contact with the study objects, and that this in fact makes ‘neutrality’ or ‘objectivity’ impossible (Knorr Cetina 1981, p 17). A larger issue on the same theme is personal interest, or *bias*, which traditionally has been denied in science,⁷ but which is inextricable from certain qualities of a researcher, such as devotion and interest, and which after all is essentially human. In this study, the ambition has therefore been to carefully and deliberately deploy bias, rather than to pretentiously try to hide it.

Personal interest can be a resource for gaining access to study objects and for creating a constructive atmosphere in interview situations (Marshall and Rossman 2006, p 74), but it can also be a *tool* in analysis, as it may mitigate discovery and understanding (Grills 1998, p 14). In this study, interest and fascination for the scientific and technological features of synchrotron radiation laboratories have in fact been deployed for both of these purposes – to establish a constructive relationship with interviewees and to make use of technical documents and reports which contain information of interest for the thesis but which are probably impenetrable for the complete newcomer. Herein lies also a crucial factor for gaining access to environments and interviewees. One way or the other, a scholar needs to prove knowledgeable and credible toward the interviewees and the people in charge of providing access to environments for study and important material – and her study needs to appear interesting and promising. A certain amount of devotion and interest is doubtlessly helpful, and possibly also unavoidable in the long run.

The notion of ‘insiderness’ is closely related to this, as possessing knowledge about the study objects and their context and enjoying a relationship of trust and respect can enhance the potential returns of both observation and interviews, but it can also be a liability (Labaree 2002, pp 99–100, 106–107). All these potential threats to clear-headed and conscious data collection and analysis will be further discussed in connection with interviews below.

7. Merton’s *disinterestedness* norm (see previous sections and note 4) prescribes such impartiality on behalf of scientists.

Interviews

A factor in the process of conducting studies for a PhD thesis that should not be underestimated is the amount of *time* available – time for fieldwork and theoretical study, for reflection and contemplation, and for trial of findings and conclusions in seminar work and continuous discussions with advisors. The comparably large amount of time available makes possible a partial *trial and error* approach, and it provides opportunity for extensive collection of material and theoretical study with undisturbed focus. It may therefore prove to be a luxury from method point of view since much of the risks associated with research methods can be significantly reduced by care, contemplation and continuous reevaluation. Concrete examples of advantages is foremost associated with the interviews – careful and complete transcription provides a more comprehensive insight to the message of the interviewee but is often time-consuming, and more available time also allows for returning to interviewees for follow-ups. In the present study, these factors have been decisive.

Interviews have been used extensively in this study, and though observation and document analysis also makes up important parts of the method, the absolutely largest part of the empirical material has been collected through interviews. In total, 66 interviews have been conducted with 57 people, over a total period of nearly three years.⁸ In eight cases, key persons (mostly people with especially deep knowledge and insight about the case study laboratories) have been identified and returned to for consecutive interviews or follow-up interviews after a period of transcription and analysis. All interviews have been recorded and transcribed, in most cases verbatim (see below). The interviews have been conducted according to the ‘semi-structured’ or in some cases ‘unstructured’ model (e.g. May 2001/1993, pp 121, 123) meaning that rather than a survey-like ‘question and answer’ strategy, interviewees have been encouraged to answer questions and comment on issues in their own terms or to talk freely about the subject within their own frames of reference.

The interviews have filled two primary purposes that correspond fairly well to their level of ‘structure’ and the choice of interviewees. One was to make use of the knowledge and pedagogical skills of the laboratory professionals, to gain *basic information*. The written documentation available does not suffice

8. The first interview was done in March 2006, and the last in December 2008. The interviews are listed on pages 305–308. Of the interviewees, 20 are associated with MAX-lab, 19 with Stanford and SLAC, and 14 with ESRF. One interviewee served double purposes as informant about both MAX-lab and Stanford, and three were interviewed for general purposes rather than in association with a specific case.

for this in the initial phase of the study, because of its advanced level, and so interviews were necessary to gain knowledge about scientific, technical and organizational features of synchrotron radiation laboratories, primarily in the beginning of the study but to some extent also later, for example at the initial confrontation with a new case. These interviews have been done with laboratory officials, such as directors, scientific directors, or inhouse scientists. The second purpose of interviews has been the collection of synthesized information about sociologically and politically interesting events, interactions and processes, in other words the empirical material that provides ground for the *analysis* and contains considered judgments of the actors in the study. The interviewees have been directors, inhouse scientists, external users, and science administrators, and they have been chosen with the ambition to achieve something approaching a representative sample (see below). From a methodological perspective, the first type of interviews is rather straightforward, structured or semi-structured with predetermined questions. It also contains less risk or uncertainty compared to the second type, the nearly 'unstructured', where interviewees rather have been asked to speak to certain issues or topics and in which all kinds of potential problems of *interpretation* and *subjectivity* are present.

In both cases, interviews are essential sources of information, and in some instances the *only possible* sources of information, because observation and documents both need clarifying in interviews and tend to inspire questions and areas of discussion for the interviews (Patton 2002, p 307). Interviews have the inherent quality of providing the researcher with insights that can hardly be obtained otherwise, because they are *personal*, but this is obviously also their problem. All information disclosed by humans is "a narration and a performance" (Law 2000, p 20), which means that it is shaped by the interviewee's *subjective* preferences, and therefore in some cases (entirely or in parts) consists of faulty interpretations or even deliberate disinformation.

Narratology or *narrative analysis* provides a method framework for the collection, handling and analysis of creative, non-fictional, recounted stories of human experiences over time (Patton 2002, p 116). It borrows from *phenomenological* study of human experience (e.g. Sokolowski 2000) and *hermeneutical* interpretation (May 2001/1993, p 15) and has its focus on the *content* and *performativity* of narrated experience. Narratology's sense-making or creation of meaning of things and events is a process that takes place in several instances: as the story is told by the informant, as the recording (and where appropriate, transcription) is done, when the story is presented in its finalized, packaged form in the writings of the researcher, and when it is finally read. The point is that *meaning* is "fluid and contextual" and added in every one of these instances, which means that a narrated testimony never

can achieve complete stringency with respect to meaning (Kohler Riessmann 1993, pp 4-5, 15). On basis of such realization, the *advantages* and *qualities* of the interview as research method can constructively be employed. Lived experience, narrated in an *anecdotal* fashion, may prove a particularly rich source of information. In the course of the interviews done for this study, one feature of oral testimony in anecdotal form proved especially useful, namely the freedom of the interviewees to frame and contextualize their answers, or plainly to *tell the story as they prefer to tell it*. It has been clear during the collection of material through interviews that information about *what really happened* in a particular situation is sometimes only obtainable when the informant is given full freedom to phrase and formulate her answers or to speak freely to a subject, as has been the case in the 'semi-structured' or 'unstructured' interviews that make up a major part of the empirical material for this thesis. The alternative has too often been no answer at all, and so criticism of the potential dangers of semi-structured interviews should be weighed against the richness of the information obtained and the empty alternative.

This leads to another important methodological issue in connection with interviews, namely that of choosing interviewees and the availability of informants. Access to both a laboratory environment (MAX-lab) and a knowledgeable informant associated with that environment, already at an early stage of the study, helped significantly in the process of making sense of the subject and plan the continuing work. Opposite situations have also arisen. The ambition has been to achieve a sample of interviewees as representative and complete as possible, but certain potential interviewees – sometimes judged particularly important for the study – have occasionally been unavailable, which has forced another route to be taken for the study and most certainly has led to the ignoring of a particular piece of information or interpretation of events and phenomena. This is as unavoidable as it is methodologically problematic.

The last important issue with regard to interviews is transcription and translation. As already mentioned, the study has benefited from a long time frame, which has made easier careful and meticulous handling of material, including verbatim transcription of interviews, which has been practiced in most cases. This has enabled continuous returning to interview transcripts during the processing and analysis of the material, and it has also allowed for extensive use of direct quotes in the final thesis, which strengthens the analysis and sends some potential problems of reliability back to the informants – quoted statements erases ambiguity in interpretation at least on one level. Some interviews have been done in Swedish, and direct quotes have then been translated to English in the final thesis text.

Rhetoric and disinformation

Related to the problem of bias is the risk of excess rhetoric, political agendas, and deliberate or accidental misinformation. There is an urgent risk – perhaps most obviously so in the interview situation but as we shall see also on other occasions – that part of the material obtained is skewed or politically tainted or that information is withheld for political purposes.

In the present study, dealing with science and science policy, and in which increased competition and scarcity of valuable resources in science is even part of the analytical framework, the study objects who are exposed to these realities may very well see the study and their participation as a potential advertising board for their activities and a channel for their message. Scientists and science policymakers have their own agendas and interests and they are hardly unaware that the study to which they are contributing may have some impact and is a potential forum for expressing their views and perspectives. But also leaving politics aside, people tend to have a general desire to make their personal interpretations and recollections shine through, if not for any other reason than the joy of having their name turning up frequently in a published text. The opposite problem is also occurring – especially policymakers may be reluctant to disclose some information on the record. Similar problems surround the use of printed secondary material. Reports and other material can be deliberately skewed in their perspective – apart from providing information about laboratories and science, they are also in many cases designed as advertisements for the particular laboratories or interest groups who have issued them. Remembering this in the analysis can however balance and nuance the message.

Synchrotron radiation laboratories and their activities are crowded with rhetoric that has evolved through the decades and established itself in the communities. Many rhetorically attractive terms and concepts are consciously or subconsciously passed on from interviewees to the interviewer: first, second, and third ‘generation’ laboratories, ‘parasites’, ‘user oriented’ laboratories, ‘oversubscription’ of beamlines, to name a few. Some are invented or advanced by the author: ‘x-ray jocks’ and ‘small science on big machines’. The existence and usage of such potentially deluding concepts cannot – and probably should not – be done away with. They are natural parts of the vocabulary of interviewees, they have an explanatory function, and they arguably add nuance and appeal to descriptions and analyses. Nonetheless, the existence of such rhetoric shall be born in mind, both by the author in order to achieve balance in the analysis, and by the reader in order to appreciate that balance.

Generally, a “modest skepticism” towards informants and the information

they disclose (Marshall and Rossman 2006, p 119) seem to be the reasonable antidote to these inevitable plagues of fields and arenas partly colonized by politics. Evaluating stories and claims in comparison with each other and attempting to retain a holistic perspective on the material is necessary. In an effort to somewhat reduce the risk of becoming the obedient spokesperson for particular interests, the strive in this study has been to conduct complementary interviews with different people about the same phenomena and events, while also contesting their claims with others' in the interview situation. The information in brochures, reports and other documents has been used as basis for interviews, and the sources have frequently been contrasted against each other.

Occasionally fragmented information and incomplete sources cripple the material and make the basis for solid statements and stringent analysis shaky, and that may lead both author and reader to give way to doubts about the relevance of parts of the study or the overall thesis. Such a stance, though not entirely nonsensical, is hardly constructive. But "guaranteed failure to achieve a definitive view" (Greenberg 2007, p 8, op. cit.) is not the same thing as guaranteed failure on all accounts, and if the author responsibly and consciously evaluates the strengths and weaknesses of the material and lets such evaluation guide the analysis and the development of the arguments, the statements and analysis will be trustworthy and hopefully contributive despite not always being completely solid and stringent. It has been a clear aim of this thesis to do exactly that.

The rise and fall of big physics

The marriage between science and the state

“The mobilization of American science during the Second World War – especially in the Manhattan Project and in the construction of radar, but spreading across much of the scientific landscape – propelled a generation of academic scientists into a world that was largely unfamiliar to them: the experience of large-scale organization; of teamwork; of interdisciplinary project-oriented research; of unlimited resources and severely limited time; of close contact with the sorts of people – especially the military and the commercial worlds – they had not known much about; and, after the end of the war and the beginning of the Cold War, the experience – for some of them – of political power” (Shapin 2008, p 64).

The historical foundation for the emergence and establishment of synchrotron radiation laboratories in the modern science and science policy systems is both old and new. The *science* done at synchrotron radiation laboratories can be identified as a recent branch of activities in the ordinary natural sciences that happen to use a specific type of instrumentation and therefore has a long history, while synchrotron radiation *laboratories* are physical and technological assets of a type that emerged in a recent historical period. The aim of this chapter is to contextualize synchrotron radiation *laboratories* historically, and to provide an overarching science policy framework for the thesis by explaining and analyzing the historical developments that can be identified as particularly decisive for the emergence and establishment of the laboratories. This is done largely on the basis of secondary material; the available body of literature on the history of science of the second half of the 20th century is enormous and provides an excellent ground for a selective account of events that is of interest and significance for a particular phenomenon.

The ‘social contract’ for science, prescribing governmental patronage of (basic) science and retained autonomy for the scientific institutions, was put in place in most Western democracies on a broader scale as a result of World War II, through the introduction of large-scale governmental funding programs for science and the establishment of governmentally run institutions

and organizations for scientific activities of basic societal interest but also with connection to more direct military and economic aims and purposes. The development was particularly visible and evident in the United States, whose detonation of the two atomic bombs over Japan in August 1945 dramatically demonstrated the potential power of science as military and political power. Their part in the 1945 victory was paired with almost no domestic material destruction and therefore they had a comparative advantage in economic and technological capacity.

For the most part, the (Western) research system had reached its modern form before World War II, including institutional division of labor between so-called basic and applied research and with well-established institutions such as universities and research institutes. Financing of the system was however for the most part private, and with rare exceptions governments were excluded from funding and control because scientists guarded their autonomy heavily (Greenberg 2007, pp 5-6; Smith 1990, pp 28, 30-31). To some extent, an international scientific community existed before World War II, especially in physics, a discipline with a particularly lively momentum of discovery and achievement in the interwar period (Greenberg 1999/1967, p 70). The Great Depression had caused governmental intervention in the economies of both the United States and several European countries, with investments in science and technology emerging as one possible arena for governments to influence the long-term development of the economies, but it took until the War before this opportunity was put to practice (Agrell 1989, p 19).

The potentials of governmental support for science and science's ability to contribute to society in a variety of ways was probably recognized by scientists and policymakers before the war, but the powers of this prospective partnership needed a strong and comprehensive demonstration (Agrell 1989, pp 20-21; Greenberg 1999/1967, pp 51-52). World War II gave that demonstration, most spectacularly through the achievements of the *Manhattan Project*⁹ and the atomic bomb, but in reality on a much broader scientific and technological arena, including early computer technologies, synthetic materials (e.g. nylon and synthetic rubber), the penicillin, cryptology, and a number of radio technologies such as the radar (Dahllöf 2001; Pickstone 2001, p 185). This wartime systematic and goal-oriented deployment of science and engineering – foremost in the United States, Japan, Germany and Great Britain – was extraordinary in historical perspective and laid the foundation for the postwar

9. The 'Manhattan Project' was the codename for the US atomic weapons program 1941-45. The project's organizational entity was formally named the *Manhattan Engineer District* (Kevles 1995/1977, p 324). For a comprehensive account on the project and its scientific and technological achievements, see Kevles (1995/1977, p 324-333), Groves (1983/1962), Hughes (2002), and Hoddeson (1992).

'marriage' between science and the state (Agrell 1989, p 20), a marriage that was conceptualized and arranged for by several actors in the systems long before the end of the war (Stokes 1997, p 47).

The postwar 'military-scientific complex'

Vannevar Bush (1890-1974), director of the United States federal Office of Scientific Research and Development (OSRD) during the war, has been canonized as the 'creator' or 'engineer' of the postwar American science system (Zachary 1997), and as the one who codified the social contract for science, as we know it. Bush's 1945 report *Science, the Endless Frontier* was the first and most famous document laying out the basic principles of the partnership between science and the state, and it was incorporated in "the mythos of American science policy" as the document setting the course for the United States' development into scientific superpower (Guston 2000, pp 52-59). Though the course was set already (Greenberg 2001, p 43-44) and other contemporary reports and investigations certainly also contributed (Guston 2000, pp 53, 57-58; Greenberg 2001, pp 47-49), the Bush report was the chief policy document of the time. It summarized the policies that was implemented in the postwar years and that has remained the backbone of Western Hemisphere science policy ever since (Stokes 1997, p 2). The 'social contract' (see chapter 1) was made imperative in the United States (and subsequently elsewhere) by the establishment of the 'linear model for technological innovation', because it was widely believed that science needed governmental support but also retained autonomy to be productive.

But science changed with World War II in several other aspects as well, and on international level. Scientific progress was brandished as a potential societal force and a military and economical source of power. Governmental money ensured continued growth, and changes within the scientific disciplines, reciprocal with the heavy support from the state, enabled strong and swift progress in many areas. Growth of scientific disciplines, growth in number of scientists and engineers, and plain growth of money and publications increased dramatically after the war (Nye 1996, p 226; Price 1986/1963, pp 1-13). Representatives of science became involved in military and government affairs (Needell 1992, pp 290-291). Especially in physics, the cultural content and meaning of science was altered, both internally, regarding mode of work and structure of workplaces, and externally, in the view of physics held by society and physics' place in the world (e.g. Galison 1997, pp 242, 310, see also chapter 1).

The term 'military-industrial complex', coined by President Eisenhower in a 1961 speech, has been used to denote the collected performance of military and economic interests in arms races in general and the Cold War specifically (Agrell 2002, pp 140-141). The term 'military-scientific complex' is perhaps appropriate to summarize the large and multifaceted institutional structure that emerged out of World War II and that was turned into a peacetime engine of military and economic development and power. For the first decade or so, there was no real separation between the military and civilian utilization of atomic energy, it was not until the Eisenhower proclamation of the 'Atoms-for-Peace'-program that weapons construction and peaceful uses of the nuclear were singled out as distinct entities of the pursuit of the atomic age. The international Geneva Conferences on peaceful utilization of nuclear energy and their accompanied policies of sharing and exchanging information on results on such utilization stands in sharp contrast to the restrictions and secrecy surrounding all the nuclear programs for military purposes in the various countries (Hewlett and Holl 1989, pp 209-270). This division between military and civilian utility, manifested in a division between policies of secrecy and exchange, was the immediate result of the Eisenhower administration's policies.

Nuclear research included foremost various branches of physics, but another natural area of studying the subatomic was biology and the subject areas that would later be jointly named the 'life sciences' (Hewlett and Duncan 1969, pp 244-245). Researching the atom and its nucleus, and the forces it could unleash, was only the 'crown' of the whole governmentally induced and controlled scientific initiative: All developmental goals – military and civilian, economic and societal – were joined together in a collective scientific and technological effort to boost the development of the postwar society (Hewlett and Duncan 1969, p 485; Hewlett and Holl 1989, p 515); in the US as well as the Soviet Union and in European countries whose postwar political and economical conditions allowed it. The institutionalization of the 'military-scientific complex' was the first step towards the emergence of large-scale scientific infrastructures as strategic and generic assets in the public science systems and a specific issue in science policy.

Big physics emerges

With science in general ascending to a crowned position in the postwar world, physics became especially important. The development of the atomic bomb was only one of several wartime scientific and technological achievements,

but it was indisputably the most spectacular and horrifying one. As a scientific undertaking, the Manhattan Project was thus also organizationally the most spectacular wartime effort, enormous also measured against late 20th century standards. With a total cost of \$2.2 billion (Hewlett and Anderson 1962, p 2) and at its height employing 130,000 people (Hughes 2002, p 9), the sociological lesson of the Manhattan Project was its demonstration that scientific projects could be large, hierarchically organized, multidisciplinary, and nonetheless achieve predefined goals. Other countries had had similar experiences; Germany and Japan had pursued their own bomb programs, as had to some extent the UK (Jungk 1956, pp 77, 108). In the immediate post-war years, fundamental physics programs centered on reactor and accelerator work would be organized in large scale, teamwork mode in several countries around the world.

Physics had however started to grow 'big' before the war. During the first decades of the 20th century, atomic and subatomic physics had developed slowly and gradually towards something resembling 'big science' (Seidel 1992, p 38), and in that tradition, the American physicist Ernest Lawrence had developed a nuclear physics program at the University of California, Berkeley. Lawrence was the first to construct and operate accelerators at a scale that motivated him to search financial support outside of the traditional university or research institute structure (*Ibid.*, p 28). His 1939 Nobel Prize in physics enabled for an expansion of the lab, and when the Manhattan Project was initiated in 1941, the Berkeley accelerators and laboratory assets were enrolled in the process of uranium enrichment (Heilbron et al 1981, pp 30-32; Kevles 1995/1977, p 325). As the end of the war drew closer, Lawrence started planning to make use of the accumulated wartime scientific and technological capacity for peaceful means (Heilbron et al 1981, p 46), and got a federal grant for continuing work in late 1945 (Greenberg 1999/1967, pp 212-213, Hughes 2002, p 106, Westwick 2003, p 36).

Simultaneously with Lawrence in Berkeley, accelerator programs for nuclear physics had emerged in Tokyo and Osaka (Sasaki 1997, pp 139-140). Just as in the Manhattan Project, Japanese accelerators became important parts of wartime military R&D (Hoddeson 1983, pp 4-7), but in 1945 all Japanese nuclear research was banned by the occupation force, and the existing accelerators were destroyed (Groves 1983/1962, pp 367-372; Sasaki 1997, p 140). When the ban was lifted one year before the 1952 peace treaty, a new nuclear and particle physics program including accelerators was started at the Institute for Nuclear Study, INS, in Tokyo (Hoddeson 1983, p 11).

In the Soviet Union, nuclear energy programs had been started immediately after the end of the war (Jungk 1956, pp 250-252) and culminated with the test explosion of the first Soviet atomic bomb in 1949 (Gaddis 2005, p 57;

Griffiths 1995, p 25). Soon after the war several European countries, including the UK, France and Sweden, initiated their own atomic research programs, none of them with any articulated separation between military and civilian utilization of atomic power. It was however in the United States that nuclear research could be developed at its most unlimited pace, due largely to the 1946 transformation of the Manhattan Engineer District and its assets into a system of *National Laboratories*.¹⁰ The Atomic Energy Act of 1946 had established the Atomic Energy Commission (AEC), which in 1947 inherited the Manhattan Engineer District's four major sites¹¹ and tens of thousands of employees (Johnson and Schaffer 1994, p 27). The Atomic Energy Commission saw as its purpose to join "men and machines" at places and institutional constructs that could ensure atomic energy work to be done in security (Westwick 2003, p 8; Hewlett and Duncan 1969, pp 223-227), not separating military and civilian purposes but rather deploying the sites, infrastructure and manpower of the Manhattan Engineer District in a comprehensive program to explore and utilize 'all things nuclear' and a host of other scientific or technological pursuits associated thereto¹² (Hewlett and Anderson 1962, pp 620-655, 714-722). Besides Berkeley, a nuclear research program was started at the second designated National Laboratory in Brookhaven in 1946 (Hewlett and Duncan 1969, pp 226, 237; Crease 1999, p 21). Scientifically, the postwar development of subatomic physics was a natural continuation of the progress made before and during the war, but the extreme change in the amount of money available for all things physics was something radically new and discontinuous:

"Right after the war we had a blank check from the military because we had been so successful. Had it been otherwise we would have been villains.

10. The use of capital letters in 'National Laboratories' and 'National Labs' is to emphasize the special character of these institutions, as described in this and coming sections, and to distinguish them from other national laboratories of various kinds that may be referred to elsewhere in this and other chapters. Most of the authors quoted do not capitalize the letters (probably because their works are focused exclusively on the US National Laboratories and thus there is no ambiguity), hence the confusion of non-capitalized letters in quotes. As an important part of the political context on one of the case studies in this thesis, the National Laboratories are discussed in a separate section at the end of this chapter.

11. Argonne, Illinois; Berkeley, California; Los Alamos, New Mexico; and Oak Ridge, Tennessee (Westwick 2003, p 27).

12. The expression 'all things nuclear' is taken from Krige (2003, p 901), who uses it to describe the privileged position of nuclear physicists in Europe after World War II: "Capitalizing on their newly achieved status and influence in high political circles, which were now particularly receptive to all things nuclear [...]" The *McMahon Bill*, also known as the *Atomic Energy Act of 1946*, regulated the mission of the AEC and prescribed that it should supervise and have the exclusive right to carry out research and development work on nuclear energy and all purposes associated with it (Hewlett and Anderson 1962, pp 714-722).

As it was we never had to worry about money” (Luis Alvarez, Berkeley lab physicist and Nobel laureate in 1968, quoted in Pais 1986, p 19).

The first major undertaking at the Brookhaven National Laboratory was the construction of the ‘Cosmotron’ accelerator, for which AEC had granted \$9.3 million (Hewlett and Duncan 1969, p 251). In 1949, Berkeley started construction of its \$9.7 million ‘Bevatron’ with AEC money (Greenberg 1999/1967, p 215; Hewlett and Duncan 1969, pp 234–235). The formal decision taken by the AEC in 1948 to officially let these accelerators into its program made the emerging field of high energy physics a natural inhabitant of the US National Laboratories (Westwick 2003, p 143) and provided the initial institutional hotbed for accelerator-based high energy physics.

The Marshall Plan for science

Though the purposeful and inclusive wartime harnessing of scientific talent and capabilities was not restricted to the United States but had its counterparts in the UK, Germany and Japan, its postwar sequel was largely an American product. The ‘social contract for science’ and the institutional structuring of the utilization of science for political, social, economical and military ends was for the most part designed by US scientists and policymakers and emulated in Europe by direct as well as indirect action. Similar to the United States, the United Kingdom’s transition from war to postwar included the deployment of wartime nuclear weapons work manpower and infrastructure to a nuclear research program for both fundamental physics research and the development of nuclear energy (Hughes 2002, pp 103, 111). The science policy strategy of exploring ‘all things nuclear’ was proliferated across the European continent after the war, and by 1958, every Western European country apart from Ireland had established governmental agencies to supervise and develop utilizations of atomic energy (Herman 1986, p 17).

Several strategic and political interests marked the developments in Western Europe after the war: the US interest in securing its fence against Soviet influence in Europe, the French need to tie down Germany in a politically subordinate position to Paris, the German willingness to renew its international standing and re-emerge as a country and nation, and the European agenda to establish a common order that would prevent future repetition of the catastrophes of the two wars. All was dependent on a rebuilding of (Western) European prosperity and economic stability (Anderson 1997, p 66; Gaddis 2005, p 17), and the economic efforts of the Marshall Plan were

complemented by cultural and social programs, including on areas of science and technology. The social contract and the institutional framework for science was purposefully and coherently exported from the United States to Europe, based on the Vannevar Bush doctrine and the notion that a strengthening of basic science was “essential to the long-term economic prosperity of the [European] Continent” as well as a preventive action against the spread of communism (Krige 2006, p 11).

The “Marshall Plan for science” (Krige 2003, p 902) implemented in the first decade of the Cold War had one particularly visible manifestation, the CERN project. CERN (Conseil Européen pour la Recherche Nucléaire, nowadays named the European Organization for Nuclear Research but still abbreviated ‘CERN’) was not only the first intergovernmental collaboration in Europe after the war and a remarkable European achievement, but also “a coproduced instrument of European and American political interests in the early Cold War” (Krige 2006, p 57) and a strategic asset. As a manifestation of US determination to readmit (West) Germany rapidly into the Western fold after the end of the war, the institutionalization of civilian nuclear research in CERN was the “perfect way” to let German scientists into the community¹³ (Krige 2003, p 903) and a way to consolidate US scientific and foreign policy interests in the region (Krige 2006, p 67). The influence of the US in this context was not only manifested in the direct policy measures taken but also in the existence of a subtler American scientific ‘hegemony’. In order to rebuild itself scientifically and technologically, and in order to counter ‘brain drain’,¹⁴ Europe had to compete with the US on US terms, by erecting its own contender in the enterprise of large-scale science (Nyberg and Zetterberg 1977, p 15).

The United States employment of scientific and technological policy-making for securing its own strategic interests was partly continued by the Eisenhower Atoms-for-Peace program of the late 50s. By the initiation of this policy, a sharp division between military and civilian utilization of ‘all things nuclear’ was introduced internationally for the first time. The American rationale was to let peaceful nuclear technology be shared internationally and charitably made available to all mankind, so as to legitimize the superpowers’ continuous monopoly on atomic weapons and supposedly increase the

13. Thus Werner Heisenberg also remarked that the German Federal Republic’s membership in CERN had “20 percent scientific and 80 percent political” rationale (Krige 2003, p 903).

14. ‘Brain drain’ is an expression used for the potential flow of people with talent and competence out of any entity or system (such as academia per se, an academic field, the private sector, a country, or a continent) because of a more attractive situation somewhere else. The fear of brain drain has been a repeatedly returning policy concern in many European countries during the postwar era. See for example Ziman (1994, pp 172, 244).

overall American policy control over ‘all things nuclear’, including weapons proliferation (Agrell 2002, p 35). Atoms-for-Peace was a “key element in Eisenhower’s grand design for Europe” (Hewlett and Holl 1989, p 430) and helped in forging even stronger ties between (Western) Europe and the USA.

Bigger and bigger physics

“To justify the value of research for the Administration’s Atoms-for-Peace program, the Commission had to rely on a few projects that seemed to push the frontiers of science into exotic realms that somehow captured the imagination of non-scientists. Ernest Lawrence had learned in the 1930s that probes into the submicroscopic world of the atomic nucleus with the cyclotron elicited that kind of response. The discovery of the synchrotron principle during World War II had sparked new enthusiasm for high-energy physics after the war, and it became the research area in basic physics most generously supported by the federal government” (Hewlett and Holl 1989, p 257).

‘High energy physics’ synonym with ‘particle physics’, the latter term referring to the aim of the discipline, discovery of fundamental particles, and the former to its pursuit: using accelerators to charge particles with high energy and smashing them into one another or into a target element and study the resulting bursts of smaller constituent particles. The discipline established itself slowly during the first decade after the war, by the construction and utilization of accelerators at labs in Europe and the USA, and soon the results started to show up in great numbers. The number of subatomic particles discovered exceeded all anticipation (Pickering 1984, pp 46-49) and was ‘proof enough that the investments in expensive accelerators produced results. The American National Labs were initially the only players on the world stage, until the Soviet Union, Japan and the collective European initiative entered in the mid to late 50s (Hewlett and Duncan 1969, p 237). The enormous expectations on what the subatomic world would offer mankind in terms of technologies for general wealth rendered its scientific and technological pursuit almost unlimited support.¹⁵

On the part of Atomic Energy Commission, however, there had been ini-

15. For an entertaining account on the most extreme praise and overconfidence in everything the ‘nuclear world’ could offer humanity according to contemporary beliefs, see Del Sesto (1986).

tial hesitation to enter the field. The first AEC director of research is reported to have had “qualms about funding these projects” but was convinced by the arguments of the field’s proponents that this was a peaceful branch of nuclear research and “something physical that could produce tangible results” (Holl 1997, pp 152, 155). One of the purposes of the National Laboratory system had become to construct large-scale scientific facilities and provide them to the nation’s researchers (Westwick 2003, p 8), and apart from reactors, the high energy physics accelerators were the largest and most expensive machines. In 1957, Congress decided to transfer federal responsibility for high energy physics entirely to the Atomic Energy Commission (Panofsky 2007, p 74), and by the end of the 50s the AEC budget was dominated by accelerators (Westwick 2003, p 151).

Although the Soviet Union had started governmentally funded programs in nuclear physics almost immediately after the war, it entered the field of high energy physics officially with the 1956 establishment of the Joint Institute for Nuclear Research (JINR) in Dubna, a collaborative effort with eleven other nations in the Eastern bloc¹⁶ (Jungk 1968, p 159). The institute and its surrounding infrastructure had been in operation but kept secret for several years when it officially ‘opened’, including the main accelerator that outdid all contenders in performance. Thus the Soviet Union entered the global competition in high energy physics with no prior warning, which caused some shock in the American physics community (Greenberg 1999/1967, p 216; Holl 1997, p 169; Jungk 1968, p 160).

The Soviet launch of Sputnik in 1957 however had even greater ramifications. It “suggested that the technological dominance that the United States had maintained since World War II was beginning to crumble” (Hewlett and Holl 1989, p 515), and hence it provided the government, the AEC, and scientists with renewed and extended justification for large and increased spending (Agrell 1989, pp 22-23; Holl 1997, p 191). The Eisenhower administration’s view on the new military-strategic situation was that the Soviet Union and its allies would outdo the United States with regard to conventional forces and military manpower, and that the only way to match them was with technological superiority (Gaddis 2005, pp 66-67). Thus together with the Korean War and the revealing of the Soviet high energy physics program in Dubna, the ‘Sputnik shock’ caused an intensifying of the federal physics programs, in nuclear reactor research but most of all in high energy physics (Westwick 2003, pp 160-161). An ‘advertisement science’, appealing to the general public as a quest for deeper knowledge about the fundamentals of nature (Hewlett

16. Albania, Bulgaria, the People’s Republic of China, Czechoslovakia, East Germany, Hungary, North Korea, Mongolia, Poland, Rumania and Vietnam (Jungk 1968, p 159).

and Holl 1989, p 257, op. cit.), high energy physics became a “key element” in the Atoms-for-Peace program (Hewlett and Holl 1989, p 522).

When the Soviet Union entered the field of high energy physics, a micro-version of the Cold War arms race begun; an ‘energy race’ between the United States and the Soviet Union with some involvement of Europe through CERN. The Cold War provoked the superpowers to seek ‘superiority’ in every demonstrable area, and high energy physics was indeed both prestigious and easily measurable (in size of accelerators and their energy) (Hewlett and Holl 1989, p 522). It thus became one of two major arenas for competition beside the purely military, the other one being the space race and the 60s quest to put a man on the moon (Agrell 1989, p 23). Hence United States federal operating costs for high energy physics grew from \$7.3 million in 1954 to \$33.2 million in 1960 (Greenberg 1999/1967, pp 216-218). When the Stanford linear accelerator project was approved for funding by the US Congress in 1961 (see chapter 4), it was the first single basic science project in the US to break the \$100 million ceiling (Ibid., pp 221-222), and it also inaugurated a new era for high energy physics in the US by being first single-purpose laboratory, built on green field solely for operating and utilizing a particle accelerator (Westwick 2003, p 183, Panofsky 1992, pp 134-135). This establishment was furthermore done *in addition to* the already vibrant high energy physics program at the US National Labs, with two new major machines opening in 1961 and 1963 (Pickering 1984, p 32).

For the rest of the Cold War, the developments of the Soviet accelerator programs kept roughly the same pace as Europe and the US, although not as many machines were constructed as in North America (Irvine and Martin 1985, p 303). In Serpukhov, Novosibirsk and Yerevan, Soviet scientists made “outstanding contributions” to accelerator physics development (Sessler and Wilson 2007, p 84).

Cyclotrons, synchrotrons, linacs and storage rings

‘Particle accelerators’ is the common name of machines that are built to accelerate elementary particles, i.e. those particles that make up the atom. For high energy physics purposes, bunches of either protons or electrons or any of their ‘anti-particles’¹⁷ are accelerated in these machines and collided with each other or smashed into a fixed target. In the resulting ‘explosion’ of the

17. The atom is made up of neutrons, protons and electrons. Protons and electrons are charged, positively and negatively, respectively, and they have rare counterparts that have the opposite charge but the same weight, ‘antiprotons’ and ‘positrons’ (Isaacs et al 2003/1984, p 47).

particles, several smaller constituent particles are released and can be detected (Sessler and Wilson 2007, p 110; Galison 1997, p 363). Because protons and electrons are charged, they are easy to manipulate with electric and magnetic fields. Electrons are very small in comparison with protons and neutrons (weighing about 1,800 times less), and are therefore easier to handle and manipulate in accelerators¹⁸ (Daintith 2005/1995, pp 151, 337, 407).

The development of accelerators for experimental study of atoms and their constituents started in the late 1920s, when it was showed experimentally that atoms could be penetrated by particles charged with energy. The first such man-made nuclear reaction in history was done in Cambridge, England, by the use of a so-called 'electrostatic accelerator' (Cathcart 2005, pp 223-234). In the 30s, a host of different accelerator concepts were developed, most notably among them the aforementioned 'cyclotron' invented by Ernest Lawrence, which had the unprecedented advantage of continuously adding energy to the particles as they turned several thousand times in the circular machine (Daintith 2005/1995, p 109; Hewlett and Duncan 1969, p 229). Lawrence's first functioning cyclotron was completed in 1931 and soon followed by similar machines of larger size, in Berkeley and Tokyo, each with increased maximum energy and thus with new capabilities for the experimental nuclear physics program (Heilbron et al 1981, pp 16-27).

Despite their groundbreaking performance of energy, cyclotrons had one major drawback, namely a proportional relationship between higher energy output and length of the circular path, in effect meaning that to double the energy, the circumference of the accelerator had to be doubled (Sessler and Wilson 2007, p 55). A new accelerator concept under the name 'synchrotron' solved this problem while maintaining the basic cyclotron concept¹⁹ (Daintith 2005/1995, p 517; Hewlett and Duncan 1969, p 230). Synchrotrons remained – in parallel with 'linacs', see below – the dominant machines for particle physics research until the late 60s and early 70s, when so-called 'storage rings' emerged (Sessler and Wilson 2007, p 79). Synchrotrons had the advantage of a strongly focused beam, but the drawback of a very short beam lifetime; it produced a particle beam and smashed it in a fraction of a second (Sessler and Wilson 2007, p 58). For high energy physics this meant that useful experiment 'moments' only arrived every three or four seconds.

Another accelerator concept was the linear accelerator or 'linac', proposed

18. A lighter particle requires less energy to gain velocity.

19. The main technical difference allowing synchrotrons to accelerate particles to higher energies without a simultaneous increase of the size of the accelerators is their gradual raising of the strength of the magnetic fields, i.e. its accelerating 'power', as the energy of the particle bunch increases. The accelerating frequency is thus *synchronized* with the particle energy, hence the name *synchrotron* (Sessler and Wilson 2007, p 55).

as early as 1924 but not possible to put to practice until the invention of the 'klystron' in the 30s (Sessler and Wilson 2007, pp 28-29, 36). The klystron allowed for efficient adding of energy to particle bunches, and gave linacs a new spring and paved way for the development at Stanford University in California that culminated with the construction of the three-km linac in the mid 60s (see chapter 4). The original linac idea, powered with the klystron, has proven to be one of the most enduring in accelerator history, as both the most recent radiation source accelerators 'free electron lasers' (see below) and state-of-the-art 'neutron spallation sources' nowadays are built on the very same basic linac design (Ibid., pp 29-30).

For experimental high energy physicists, the advantages of head-on collision of two particle beams instead of smashing one particle beam into a fixed target (the collision of two beams would yield twice the energy and thus double the probability of interesting observable particle 'events') were theoretically well known in the 40s and 50s. However, neither cyclotrons nor synchrotrons could produce beams stable enough for such collisions (Sessler and Wilson 2007, p 79), and so it wasn't until the realization of the 'storage ring' concept that collisions could be carried out in practice. Storage rings are designed to keep a bunch of particles at stable speed and it thereby allows for detailed modification and manipulation of the position and size of the beam. It also allows for continuous collisions if two accelerated beams are targeted towards each other in an interaction region (Ibid.), dramatically improving the so-called luminosity²⁰ in high energy physics experiments. For synchrotron radiation applications, the advent of the storage ring was a crucial step, as will be discussed in later chapters. From the mid 70s and on, 'circular'²¹ accelerators are almost without exception storage rings, both for high energy physics and synchrotron radiation purposes.

Hindsight and foresight in the 60s

The 'Sputnik shock' of 1957 brought with it a renewed rationale for government spending on science and technology, but it also changed the relationship between science and the state. The first postwar decade had been characterized by a buildup according to the Vannevar Bush doctrine – gov-

20. 'Luminosity' is the number of events per cross section of the beam in collision experiments, i.e. a measure of how 'efficient' the experiment is (Sessler and Wilson 2007, p 80).

21. Synchrotrons and storage rings are never really circle-shaped, but rather polygonal. The convenience of using the word 'circular' to distinguish them from linacs does however outweigh its incorrectness.

ernmental spending but retained scientific autonomy. In the late 50s, parallel with increased spending, the first attempts were made to 'renegotiate' the social contract for science. During its last three years in office (1957-59), the Eisenhower administration increased governmental control over governmentally funded science and technology (Hewlett and Holl 1989, p 516).

The exceptional growth of high energy physics, with major projects at several sites approved for funding more or less simultaneously, was bound to decline although it did not end with dramatic disruption. In 1962, President Kennedy made the comparably modest promise to fund one new major accelerator project every five years (Greenberg 1999/1967, p 240), and at about the same time, two investigations were initiated that would have some impact on the future policy directions of federally sponsored science in general and big physics in particular.

In 1963 the Department of Defense conducted a review under the name *Project Hindsight*, analyzing the scientific and technological origins of twenty major US weapon system programs (Guston 2000, p 78). The results showed that the contribution by so-called basic research (in the study called "recent undirected research in science") was very small (Greenberg 1999/1967, p 31), and for the first time, the linear model was seriously questioned. Though the Kennedy administration made attempts to strengthen the central governmental influence on federally sponsored science and technology (Stokes 1997, p 143) and had started to question the prevalent arrangements of the social contract, it did so only "inconsistently and uncertainly", leaving the fundamental critique to the Johnson and Nixon administrations to formulate (Smith 1990, p 59).

In 1963, a panel convened by the Atomic Energy Commission (usually referred to as the *Ramsey Panel*) was given the task of assessing the current situation and future needs for US high energy physics (Westfall 1989, p 191), and by the work of the panel it became clear that some restrictions would be laid on the federal expenses for the field (Greenberg 1999/1967, pp 243-244). The Ramsey Panel's choice to recommend a development towards higher energies rather than higher intensity²² in effect meant that the size increase of future machines would be even more rapid (Holl 1997, p 217). The failure of the Midwestern Universities Research Association (MURA), a consortium of several Midwestern universities, to get the next major federally funded high energy physics accelerator project located to an entirely new laboratory in the Midwest (Greenberg 1999/1967, pp 246-268) was the final break of the

22. In the beginning of the 60s, it was commonly agreed among high energy physicists that the field could move either towards higher energy or higher intensity of accelerators. A focus on higher intensity would make somewhat smaller accelerators suffice at least for some time, as size of particle accelerators is generally depending on their maximum energy (Holl 1997, p 217).

previous unlimited generosity shown by the federal government towards high energy physics: "Instead of duplicating machines across the lab system, scientists at a particular lab were forced to use the facilities of other sites in the system" (Westwick 2003, p 285). The growth in size of detectors for high energy physics experiments introduced them in the family of equipment that outgrew ordinary university-sized research groups and thus became part of the programs at the National Labs; detectors could be designed and built by groups at labs remote to where they would be used, a system of distribution of labor that has been employed ever since (Ibid.).

The Johnson administration's pursuit of both the Vietnam War and the 'Great Society' as well as the space race set economic limits on the expansion of the National Labs (Westwick 2003, p 269), and had a politically weakening effect on the United States, also in a military-strategic sense (Gaddis 2005, p 170). The military buildup and intensified Cold War mutual threat after the 1957 Sputnik shock had come to its climax in 1962 with the Cuban Missile Crisis, and the prospected outcome of a full-scale nuclear war – 'Mutual Assured Destruction' ('MAD') – was close and tangible enough to invoke a slow but steady softening of tensions between the two superpowers, leading ultimately to the 70s détente policy and a series of treaties limiting in different ways the development and proliferation of nuclear weapons (Ibid., pp 78, 81). The nuclear threat, the 'Great Society' policy and the Vietnam War complicated matters in the military-scientific complex that had operated according to a fairly simple logic during the late 50s and early 60s. President Johnson's question "what science can do for Grandma" (Kevles 1995/1977, p 411) is but one of several rhetorical indications that the relationship between science and the government was changing, at least on government's part. Furthermore, the 60s saw a general "anti-science trend" due to demands of return for investments of tax money (Westwick 2003, p 296) and the association of science with environmental problems and not least the Vietnam War (Elzinga and Jamison 1995, pp 584-587). The National Labs were "a natural target for protest", as they comprised a tangible institutional intersection of weapons work and large scale scientific and technological undertaking by the government, two major reasons of counterculture and protest in the 60s (Westwick 2003, p 276). Consequently, the budget increases were halted and some years even turned into decreases. Between 1953 and 1967, the US federal government had increased its expenditures in R&D with over 400%, but from 1968 to 1976, it fell 15% (Smith 1990, p 80).

Some criticism was targeted also towards high energy physics, both by politicians and prominent scientists in other fields. In congress, the chairman of the Joint Committee for Atomic Energy said in 1964 that though "[t]here is no end to scientific ambitions to explore, [...] there is an end to the public

purse" (Kevles 1995/1977, p 416). In his famous 1967 book *Reflections on Big Science*, Alvin Weinberg wrote: "I would be bold enough to argue that, at least by the criteria which I have set forth – relevance to the science in which it is embedded, relevance to human affairs, and relevance to technology – high energy physics rates poorly" (Weinberg 1967, p 78).

Interestingly though, the energy race was not slowing down but on the contrary intensified throughout the 60s. The general slowdown and reassessment of United States federal expenses on science only affected high energy physics in relative terms, i.e. in comparison with the previous spending. With the 1972 opening of the second single-purpose laboratory for high energy physics in the US, the Fermi National Accelerator Laboratory ('Fermilab'), a major step was taken in the development that was replicated in the USSR, at CERN (Westfall 1989, pp 194-200; Greenberg 1999/1967, pp 246-268), and not least in Japan, at the National Laboratory for High Energy Physics ('Ko Enerugii Butsurigaku Kenkyusho', 'KEK') (Hoddeson 1983, p 33; Traweek 1992, p 106). Authors have argued that Fermilab was the first step in high energy physics' transition from big science to 'megascience' – with single experiments running over several years and engaging hundreds of researchers – and that this change was partly induced by the sociological changes of science due to slowdown in spending increases (Hoddeson et al 2008, pp 3, 281).

Continuous renegotiation of the social contract

Several analyses have been made of overall postwar science policy periods and shifts (e.g. Elzinga and Jamison 1995, Guston 2000; Smith 1990; Stokes 1997), and most of them make the case that the first major shift in policy took place some time in the mid to late 60s or early 70s. Freeman (1988, pp 114-115) consequently identifies two periods, signified by 'supply side' and 'demand side' focus, respectively. They correspond to the overconfidence in (basic) science of the first postwar decades that allowed for the expansion of activities like high energy physics beyond what was measurably beneficial to society, and the introduction of accountability and priorities in governmental science policy and a weighing of scientific and technological development against social, environmental and economic interests²³ (Ibid.; Elzinga

23. The Freeman framework encompasses a third period, starting in the 80s, when the shift towards innovation policy enables a restoration of basic science as important and valid part of the ability of science and technology to contribute to societal and economic progress through innovation (Freeman 1988, p 115). This partial restoration of the confidence in basic science is discussed below.

and Jamison 1995, pp 578-590). An exact time of the shift is not identified, and the analysis has no ambitions beyond that of a general overview, but it corresponds strikingly well to the policy changes discussed above. As high energy physics accelerator projects increased in size and their costs soared accordingly, previously unheard criticism emerged that invoked questions of utility and return for investment, and appealed to societal and human needs and desires. In Sweden and other European countries, the debate over CERN II (a major upgrade of CERN, see below) was concentrated around the polarized debate between on one hand the importance of 'basic' or 'fundamental' research – often invoking arguments of cultural value to society – and on the other a mistrust on the usefulness of such ventures and the opinion that the money should be spent on fields closer to application and thus measurable societal benefit (Hadenius 1972, pp 24-26; Widmalm 1993, pp 118-119, 126). The arrival of such debate can be found in the American context as well, albeit somewhat later. The discussion surrounding the Superconducting Super Collider in the late 80s and early 90s (see below) was the height of this sharply polarized discussion and arguably the final blow for the uninhibited supply side doctrine that had allowed for big science to become a matter of course in governmental expenditure. But it did not happen until the early 90s, and until then, high energy physics was allowed to retain a remarkably privileged position.

The concern for the 'innovation problem' – the questioning of the productivity and benefit of basic science – grew stronger in the late 70s and early 80s (Guston 2000, p 113) and became an enemy of big physics. The worsening of the economic situation in the US made policymakers demand "more economic bang for their research buck" and that "American scientific superiority needed to translate into economic performance" (Johnson 2004, p 219). Measures were taken federally in the 80s to increase technology transfer in federal research laboratories and create innovation incentives for federally employed scientists (the 1980 Stevenson-Wydler and Bayh-Dole acts and the 1986 Federal Technology Transfer Act) (Greenberg 2001, p 15; Guston 2000, pp 120, 124-125), in all amounting to a federal strategy of enabling and inciting *innovation* – attempted satisfying of the demand side – in the science system.

The general decrease in federal spending on science in the 70s²⁴ caused

24. In constant (1982) dollars, overall annual federal expenditure on R&D increased every year from the war until 1968. During the years 1969-71, federal spending was decreased with nearly 10%. Its temporary turning up with 0.5% in 1972 was more or less just a leveling and it was followed by a lowering in 1973-75 with approximately 10%. Since then and until the late 80s, it only increased (Smith 1990, p 80). The decreases may seem relatively miniscule and occasional but in the light of the steady growth of federal R&D spending from World War II until the late 80s

concern that high energy physics would “monopolize the increasingly limited resources” and damage the prospects of development in other fields, especially other areas of physics (Kevles 1995/1977, p 422), a fear proven well-founded by the heavy investments made at Fermilab and SLAC that were prioritized at the expense of others (Holl 1997, p 328). At the end of the decade, warnings were voiced that the cutbacks seriously threatened the military technology lead of the United States, and opinions started to shift back towards support for defense research and development (Kevles 1995/1977, p 423). High energy physics machines were still being built with multimillion dollar budgets, and the restoring of the military-scientific complex in the Reagan era (see below) caused a re-establishment of big physics as a popular measure of technological strength in the Cold War context. The ‘megascience’ character of the contemporary high energy physics programs (Hoddeson et al 2008, p 3) seem to have been irrevocable and made it impossible to pursue high energy physics programs without spending on the level of the largest labs.²⁵

But other uses of particle accelerators had started to emerge, and the further competition for funds was stretched, the less the hunt for new particles could be justified. The lesson learned from the US history of high energy physics is thus most of all how remarkably intact this branch of big science could be kept despite all changes in the surrounding political, economical and social climates. Later events, that will be discussed below, indicates that this was largely because no change of the same magnitude occurred in the military strategic situation, and that the nuclear warheads of two superpowers remained pointed at each other for some more time to come.

CERN monopolizing European resources

In the general cooperative climate emerging in Western Europe in the late 40s and early 50s, the United Nations Educational, Scientific and Cultural Organization, UNESCO, increasingly took on the role as foundation for speculative plans for European scientific collaboration (Nyberg and Zetterberg 1977, p 11). In 1950 UNESCO took the resolution to make invitations to European governments to create a collaborative nuclear physics lab (Krige

(overall, in constant dollars, it doubled two times), they are discontinuous.

25. This is further indicated by the virtual inexistence of high energy physics programs outside the largest labs, in the US as well as Europe, by the late 70s. In the United States, Argonne, Berkeley, Harvard, Cornell and Princeton dismantled their high energy physics accelerator programs during the 70s when federal funding concentrated to Brookhaven, Fermilab and Stanford (Martin and Irvine 1984, p 188). A similar development occurred in Europe, see note 27.

and Pestre 1987, p 524; Krige 2003, p 901). The realized inadequacy of single European countries' financial and human resources compared to the joint capabilities (with perhaps one exception, the UK) strengthened the prospects for the idea (Pestre and Krige 1992, p 81), and as noted above, the United States was already pushing for such developments in Europe. The attitude among the nations' delegates at the planning and negotiations meetings seem to have been one of urgency, provoked by fear of being left behind in the rapid scientific development in the United States and pointing in the direction of a large scale project (Pestre 1987a, p 117).

Initially, high energy physics was only one alternative among others for the activities of the lab (astrophysics, computing, and a general physics lab were other ideas), but the high and growing international status of the nuclear eventually made the choice easier (Krige and Pestre 1987, pp 527-528). The joining of European governments in a collaborative effort of this kind was, however, not an easy process – not all prospective member states were convinced of the benefits to the degree that they would make such big investments in a project with an uncertain future (Pestre 1987b, p 199). Politically, it was the fact that the laboratory mission was so rigidly restricted to fundamental research – entirely separated from all applications, especially the military – that made CERN possible at all in the sensitive political context of Europe during the immediate postwar period (Pestre and Krige 1992, pp 83-84). The “organizational philosophy” of CERN, codified in several documents drafted in 1951-52 laying the foundation for the laboratory, prescribed its purely scientific mission and stated that it should not compete with research facilities in its member states (Krige 1987, pp 228-229). The convention was ratified in 1953 and entered into force in the fall of 1954²⁶ (Martin and Irvine 1984, p 183; Pestre and Krige 1992, p 80). CERN's first big machine, the ‘Proton Synchrotron’ (‘PS’) was completed in late 1959 and it immediately rose to the throne of accelerator performance globally, where it stayed for a few years (Martin and Irvine 1984, p 185; Krige 1990, p 15).

While in the US voices in Washington (including President Johnson, see above) had started to question the use of high energy physics (albeit only cautiously), the impression of CERN in the 60s is that it was largely isolated from such political drifts. In the mid 60s, CERN was “cruising along and apparently growing steadily”, very much leaning on the general high status position of high energy physics and big science and the “universally euphoric state of the European economies at the time” (Pestre 1990, p 785).

26. The founding states were Belgium, Denmark, France, the Federal Republic of Germany, Greece, Italy, the Netherlands, Norway, Sweden, Switzerland, the UK, and Yugoslavia (Martin and Irvine 1984, p 183). Yugoslavia withdrew from the collaboration in 1961 (Krige and Pestre 1990, p 861).

As long as nuclear physics was reasonably 'small scale', several European countries pursued domestic programs, often complementary to CERN. By 1960, accelerator-based high energy physics programs were under way in Britain, France, Italy, Germany and Sweden (Irvine and Martin 1984, p 185). The 60s radical increase in the average size (and thus also cost) of the machines, however, led CERN to make major upgrade plans, including a new accelerator, the 'Super Proton Synchrotron' ('SPS'). This machine was large enough to be initially separated from the existing CERN organization (Hadenius 1972, p 10), and it caused political turbulence among the member states that threatened to terminate the whole project (see below). Once it was agreed upon and built, however, (the go ahead was given in 1971 and it was taken into operation in 1976), it changed the institutional landscape of high energy physics in Europe by concentrating virtually all resources to CERN and the West German laboratory DESY ('Deutsches Elektronen-Synchrotron', 'German Electron Synchrotron') in Hamburg.²⁷

For the countries that had once joined CERN on basis of the estimation that international collaboration would be a cheaper alternative²⁸ to pursuing a domestic high energy physics program, this 'inevitable' development of CERN into a large and very expensive lab (the CERN II) was discontinuous, since they now faced the reality of having to dismantle all domestic activity in the field and redirect that expenditure to CERN (Martin and Irvine 1984, p 186). However, since the mission of CERN was "to be as good as the best American institutions" (Pestre 1990, p 788), there was not much choice in the mid and late 60s than to join the energy race for real, although it in effect meant that CERN ceased to be the "apex of a pyramid whose base comprised the national laboratories" and became "the locus around which European high-energy physics turned" (Krige 1996c, p 199, emphasis original).

Continuous investments in CERN during the decades, in total resulting in the construction of two more major accelerators after the 70s SPS, has made the laboratory a survivor despite the dwindling reputation of high energy physics globally and the enormous amount of money it still costs.²⁹ While other high energy physics accelerator projects have been closed down one by

27. Whereas in 1970 nine national accelerator programs in high energy physics existed in Europe, in the beginning of the 80s only two centers provided facilities for experimental high energy physics, CERN and DESY (Martin and Irvine 1984, p 188; Herman 1986, p 132). Cf. note 24.

28. And though it was initially, the growth in expenditure probably outdid most expectations: CERN increased its total annual expenditure with a factor of 25 from 1954 to 1975, from below 25 million French franc in 1954 to over 600 million in 1975, in constant 1974 prices (Krige 1996b, p 11).

29. For example, the total CERN expenditures 2007 amounted to 986,9 million Swiss Franc, which is approximately 650 million Euro (*CERN Annual Report 2007*, p 13).

one during the past years without being replaced by new and larger ones,³⁰ CERN have only recently inaugurated its latest major achievement, the 'Large Hadron Collider' ('LHC'). This machine is the result of investments not only from the CERN member countries but also from Japan and the US (*CERN Annual Report 1995*; p 5, 1997, p 5; Cho 2008a, pp 1287-1289), who instead of developing their own contenders have 'joined' CERN by their contributions and 'observer status' in the collaboration. Thus on the global scene, CERN could probably be crowned as the most successful high energy physics laboratory in the world, since it has in fact survived through the decades while other projects have failed and programs have been abandoned. In the 70s, CERN monopolized the resources of European high energy physics. In the 90s and 2000s, it has done something similar on the global scale.

The Superconducting Super Collider and the fall of big physics

The gradual but all-embracing shift in science policy, beginning in the 60s, altered governmental policies towards science, technology and innovation and complicated the relationship between science and society. It occurred in cohesion with the transformations of society of the 60s and 70s, including the emergence and growth of environmentalist and anti-colonialist movements and the anti-war activism, with the Vietnam War protests at its center. The public view on science and technology was dramatically changed through a number of 60s, 70s and 80s events and issues, such as the Vietnam War, the Three Mile Island and Chernobyl accidents, the emergence of the ozone hole, the Challenger disaster, smog warnings and oil-soaked shores. All these disastrous or dystopian world events had clear and signaling connections to the 'military-scientific complex' that had grown in size and scope with steady governmental patronage since World War II (Guston and Keniston 1994b, pp 23-24). Perhaps most importantly though, the cycles of the Cold War and the changes in the global military-strategic situation changed the status of the military-scientific complex in society and in politics, complicating the previous straightforward connections between military strength and scientific-technological superiority and limiting the legitimacy of 'all things nuclear'.

The Cuban Missile Crisis had a loosening long-term effect on Cold War dynamics, eventually leading to the Nuclear Non-Proliferation Treaty in 1968, the SALT I (Strategic Arms Limitation Talks) treaty of 1972, and the

30. DESY dismantled its last high energy physics program on the accelerator HERA in 2007 (Harris 2007, p 18). At Stanford, the last high energy physics experiment has been turned off (see chapter 4).

Helsinki Agreement of 1975 (Gaddis 2005, pp 188, 199-200), in all making the 70s a period of Cold War *détente*. The process was propelled by the attempts of a SALT II agreement and the 1976 election of Jimmy Carter to the US presidency, who “had pledged during the 1976 campaign not simply to freeze strategic arsenals but to seek deep cuts in them” (Ibid., p 201), but the Soviet invasion of Afghanistan, the Iran hostage crisis, and the election of a president with a dramatically different view on the role of nuclear forces in Cold War ‘realpolitik’ would soon turn the tables irreversibly (Ibid., pp 210, 212, 222-228; Gusterson 1996, p 194).

Seemingly unaffected by both societal change and Cold War *détente*, high energy physics had continued to consume vast resources, and the temporary but significant dips in US federal spending on R&D in the late 60s and 70s did not bring any significant decline in the enormous investments in accelerator projects. The global ‘energy race’ continued, with new accelerators costing several hundreds of million dollars opening also in Europe, Japan, and the Soviet Union in the 70s and 80s.

The dwindling of the general societal status of science and technology was somewhat restored in the beginning of the 80s, when the Reagan administration “reasserted a strong belief in research and basic science as the central pillar of the nation’s scientific enterprise” and expressed a desire to “encourage a more optimistic view of science’s contribution to national defense, health, and welfare” (Smith 1990, p 108). Ronald Reagan had barely been inaugurated before embarking on a massive defense buildup and thereby in effect starting a “Second Cold War” (Judt 2005, p 592). His policies included a doubling of the US federal expenses on national security (Gaddis 2005/1982, p 394), the burying of the SALT II negotiations, and the initiation of a new major defensive weapons program, the Strategic Defense Initiative (SDI), that would drastically alter the power balance between the US and the Soviet Union and that reportedly had an effect on the officials in Moscow that “approached panic” (Gaddis 2005, p 227). The steering of federal resources away from social and educational programs and over to the military had started already during Carter’s last years and was continued by Reagan (Gusterson 1996, pp 194, 221), causing a partial revival of the 50s ‘military-scientific complex’, both in terms of financial supply and societal and political status. Though high energy physics had not suffered much from the 70s decline, its standing was nevertheless improved by Reagan with some significant increases in federal funding in the beginning of the 80s (Smith 1990, p 131), and the initiation of the largest infrastructural project ever in the field.

The Superconducting Super Collider³¹ began as a “romantic vision” of

31. For a comprehensive account on the scientific content and context of the Superconducting

a next-generation powerful American accelerator at a high energy physics conference in Snowmass, Colorado, in 1982 and was formally launched as a project in 1983 (Hoddeson and Kolb 2000, pp 275-276). Motivated just as much by threats of European world dominance in high energy physics as by the prospects of producing new exciting physics (Holl 1997, p 462), this enormous project comprised of an elliptical accelerator long enough to encircle Manhattan Island (Hoddeson and Kolb 2000, pp 271, 275). In 1986, “politics’ leading supporter of scientific megaprojects”, President Reagan, formally endorsed the project at its initial cost estimate of just above \$4 billion. The supervising agency, the Department of Energy, had falsely reported that foreign countries would be willing to pay as much as half of this sum – “[i]n fact, no other nation had pledged a penny” (Greenberg 2001, p 405).

A high-pitched debate accompanied the journey of the SSC through the scientific and political processes that lead to start of construction in 1989 (Hoddeson and Kolb 2000, p 307). In his book *Dreams of a Final Theory*, physicist and Nobel laureate Steven Weinberg argued for the necessity of making the SSC reality, referring to “the great intellectual adventure of discovering the final laws of nature” that the project hopefully would enable (Weinberg 1994/1992, p 274). Sidney Drell, deputy director of SLAC, argued that a society that didn’t support the fundamental search for answers regarding the nature of matter (i.e. particle physics) was a “suffering society” (Kevles 1995/1977, p xiv). Nobel laureates Leon Lederman and Sheldon Glashow made the case that a decision not to build the SSC would be to “terminate the 3,000 year-old quest for a comprehension of the architecture of the sub-nuclear world” (Ibid., p xix). The voices against were just as resolute. The materials scientist Rustum Roy called high energy physicists “spoiled brats”, and Nobel laureate and solid state physicist Philip Anderson argued that “Dollar for dollar, we in condensed-matter physics have spun off a lot more billions than the particle physicists ... and we can honestly promise to continue to do so” (Hughes 2002, pp 139-140). As time passed, support for the project shrunk steadily in Washington, and when the site selection process favored a location in Texas, far away from any existing federal high energy physics laboratory, the project lost more ground, among policymakers as well as scientists, and even some National Laboratories collaborators redrew from the project (Kevles 1995/1977, p xxii; Hoddeson and Kolb 2000, pp 305-309). Construction was however started in 1989, and the project lived on for another four years.

In October 1993, after a series of attempts in the House of Representatives to cancel the SSC had been reversed by the Senate, Congress finally termi-

nated the project³² (Kevles 1995/1977, pp xxx-xl). The main reason was most certainly that the \$4 billion price tag estimated at the time of Reagan's strong endorsement in 1986 had risen to \$11 billion in 1993 (Sharp and Kleppner 1994, p 154). Construction had already accomplished the completion of 14 miles (approximately 22 km) of accelerator tunnel at the site in Texas, which meant that a total of \$2 billion had been virtually wasted (Greenberg 2001, p 410).

The failure of the SSC was largely due to its size. The inability of both the political and scientific systems to handle such a big project showed in the reluctance of the Department of Energy to give the physicists full responsibility for the project, which it had done with all previous federally funded accelerators. The attitude of the proponents among scientists that this was only another natural step in the developments of high energy physics³³ showed a neglect on the verge of hubris of the unprecedented size and scope of the project. All this created a turbulence that grew into fierce opposition in Washington when the price tag multiplied (Hoddeson and Kolb 2000, p 308). It seems however that the end of the Cold War had decisive impact on the discontinuation to the high energy physics development, a logic conclusion since the Cold War was what originally allowed for its expansion to start:

“As the Cold War wound down, Capitol Hill could hardly remember the physicists' glorious contributions to the Second World War. Washington no longer saw the relevance of expensive science to the American people” (Hoddeson and Kolb 2000, p 309; also discussed by Guston and Keniston 1994b, pp 23-24).

The collapse of the Soviet Union and the end of the Cold War³⁴ changed the logic of the position of the USA as superpower and took away much of the

32. The cancelling vote showed the true nature of pork barrel politics. The Texas delegation to the House of Representatives voted 26-1, while the delegations from the home states of the other two big high energy physics labs in the US, Illinois and California, voted against with 18-2 and 34-18, respectively (Kevles 1995/1977, p xxxvii).

33. “High-energy physicists had seen their budgets grow from thousands to millions of dollars during the 50s, and to hundreds of millions by the 70s. In proposing a multibillion-dollar machine in the early 80s, physicists thought they were simply advancing by another order of magnitude” (Hoddeson and Kolb 2000, p 308).

34. The high energy physics programs in the Soviet states were kept alive after the fall of the Iron Curtain, but no new accelerators were built, and the resources were instead targeted towards collaborative efforts. In 1991, the ‘Commonwealth of the Independent States’ (a number of former Soviet republics including Russia) was granted ‘observer status’ in CERN (*CERN Annual Report 1992*, p 45).

rationale for activities that had been arenas for competition between the East and West for forty years, including fundamental science. Big physics had enjoyed an almost uninterrupted growth since the wartime accelerator development in Berkeley, and spread itself over the Northern Hemisphere as the self-evident crown of modern society's scientific progress. The cancelling of the Superconducting Super Collider did not end this development entirely. By choosing to cancel a project that arguably had exceeded reasonable economic limits and that lacked sufficient political and scientific backing, the United States Congress signaled that high energy physics would have to adjust its ambitions to the general needs of society, just like every other governmentally sponsored scientific activity had done during the past decades renegotiation of the social contract (Hoddeson et al 2008, p 340). The search for answers to fundamental questions of the origin of the universe was not entirely external to society's interests and it would continue to receive governmental support, but it would have to be weighed against other priorities, and priorities would most certainly have to be given to so-called 'strategic' areas of research – fields perceived as more likely to produce results for practical purposes (Greenberg 2001, pp 407-408; Kevles 1995/1977, p xli). Having so far enjoyed a seemingly never-ending development of larger and larger machines with dramatically enhanced capabilities each time a new project was completed, it became clear inside and outside the field of high energy physics that the envisioned next accelerator projects would have to compete on radically different terms and probably be subject to global cooperative efforts.³⁵ The closing down of the Superconducting Super Collider project in 1993 was symbolic and important in this respect and came to symbolize the fall of big physics.

Political setting one: Big politics at the US National Laboratories

As institutional constructs for governmentally sponsored science, the US National Laboratories stand out. Created out of the remnants of the Manhattan Engineer District, the National Labs possessed enormous infrastructural and human resources right from the start, and were the targets of a stream of governmental funding for science and technology programs on a

35. At the time of writing, the linear collider concept 'ILC' – International Linear Collider is under development through a joint effort between the European, Japanese and US high energy physics communities. So far, no decisions whatsoever have been reached regarding the future of the concept, which in its present design is a linear accelerator between 30 and 50 km long (Clements 2006, pp 11-12).

large number of areas.³⁶ The real purpose or mission of the labs was however more elusive. No precise definition of 'National Laboratory' was ever released by the Atomic Energy Commission, other than that they were to be the "back-bone" of the commission's research activities (Westwick 2003, pp 223, 226; Johnson and Schaffer 1994, p 52), and a resource for scientific and technological undertakings extraordinary in size or scope (Hewlett and Holl 1989, p 253). Their prime function, especially in the 40s and early 50s when 'atomic energy' was still not divided between military and civilian utility but was perceived a general concern of society in all its possible functions (see above), was to provide solid ground for work on 'all things nuclear' (Westwick 2003, p 8; Hewlett and Anderson 1962, pp 620-655, 714-722; Hewlett and Duncan 1969, pp 223-227). A multitude of activities associated thereto were however also contained within the National Laboratory system to guarantee the deployment of its vast resources for the benefit of society.

The National Laboratories grew to become both the largest and most distinct federally sponsored research institutions in the United States. They were multidisciplinary and multipurpose, between each other but also within, and their variety with respect to activities and managerial details was guaranteed by the 'GOCO' principle ('Governmentally Owned, Corporately Operated'³⁷) according to which they were run (Holl 1997, p 49). They had the liberty to seek funding for programs and projects outside the AEC, which improved the balance of independence and fulfillment of overarching goals (Teich and Lambright 1976, p 449; Westwick 2003, pp 49, 55). Most of all, however, the 'GOCO' principle made the system of National Laboratories both a giant science policy tool for the United States federal government and an independent and self-sustaining shaper of events and doctrines in science and science policy (Westwick 2003, p 3).

All of the five original Labs (Argonne, Berkeley, Brookhaven, Los Alamos and Oak Ridge) kept a variety of basic science research programs in most fields of the natural sciences (Johnson and Schaffer 1994, p 29; Westwick 2003, p 154). The breadth and richness of resources made scientists and managers recognize interdisciplinary research "as a particular strength of the national labs and a feature that distinguished them from academic research", and it also provided solid ground for future pursuit of new areas as they

36. In its first six years of existence (1947-1952), the Atomic Energy Commission spent approximately \$4.8 billion on the National Laboratories (Hewlett and Duncan 1969, pp 676-677), which is more than twice the total cost of the Manhattan Project.

37. This meant that the Labs were run by a multitude of private actors under the supervision of the AEC. 'Corporation' has a widened definition; some operation contracts were given to universities (Johnson and Schaffer 1994, p 49).

emerged, for example solid-state science and molecular biology (Westwick 2003, p 307). The labs' pronounced rationale for their extensive programs in chemistry, physics (other than high energy), biology, computer science and earth science (Westwick 2003, pp 227-238) as articulated towards the AEC varied on the theme of 'scientists on tap'³⁸ and the necessity of a broad base of activities in order to be able to mount spearhead competence and capability when called for (Westwick 2003, p 154). Great liberties were taken by mid and top-level managers in defining and pursuing their missions (Ibid., p 227), as exemplified by the first director of Los Alamos National Laboratory, Norris Bradbury, who explained his strategy for the laboratory with referral to the need for a broad base of scientific programs in order to maintain the core mission:

"I can't keep on doing this unless I have a big active basic research program and all the fields which are relevant to nuclear weaponry ... And, boy, I can sure stretch relevant" (quoted in Westwick 2003, p 227).

This power to shape the terrain has remained strong in the National Laboratory system through its sixty-year history. Missions have sometimes changed drastically and new areas have been sought out at the expense of others. Political realities have been altered, and the economic justifications for different scientific pursuits have varied widely. As institutions, however, the National Labs have proven remarkable survivors. All of the original five are still operating, and a number of others have been added.³⁹ Mostly because of the investments made in the Labs and the enormous buildup of competence and infrastructural assets, they have been permitted continued existence even though they have deviated far away from their original missions and purposes (Greenberg 2001, p 15). The rationale for maintaining all of the National Laboratories has constantly moved away from the original postwar formulation of the need to control atomic energy and keep a supporting structure for that need in the form of broad basic science programs.

38. 'Scientists on tap' is the expression used for one idea of the role of science and scientists in society, arguing that science shall be seen as a potential resource for society. The contrasting idea is summarized as 'scientists on top', i.e. that science or scientists should be society's rulers (e.g. Shapin 1999, p xvii; Greenberg 2001, pp 282-283).

39. Today, 10 National Laboratories are run under the stewardship of the Department of Energy: Ames Laboratory, Argonne National Laboratory, Brookhaven National Laboratory, Fermi National Accelerator Laboratory (Fermilab), Thomas Jefferson National Accelerator Facility (JLab), Lawrence Berkeley National Laboratory, Oak Ridge National Laboratory, Pacific Northwest National Laboratory, Princeton Plasma Physics Laboratory, and SLAC National Accelerator Laboratory (*DOE National Labs Brochure 2008*).

Institutional constructs have a general tendency to “outlive the purposes for which they are created”, by and large because the “social and political consequences” of shutting billion-dollar enterprises with thousands of employees is a very unattractive option (Teich and Lambright 1976, p 447). Similar policies to keep large-scale programs alive, motivated by the unwillingness to see previous investments go to waste and having to take other associated responsibilities for their phasing out (such as the large amounts of people left to unemployment), are common to many large scale, long term governmental commitments (e.g. Agrell 2002, p 158; Kurth 1973, pp 139-145). The history of the National Labs through the cycles of governmental spending on science and technology described in previous sections suggests that the Labs themselves tend to prevail no matter the changes in the surroundings, and that single programs are more vulnerable to political shifts (e.g. Gusterson 1996, pp 227, 288*n*). It has been suggested that there is a ‘strategy’ on behalf of the steward agency⁴⁰ for the National Laboratories to keep them alive by the ‘parceling out’ of new projects among them (*Arthur 1, Galayda, Lindau 2 interviews*), and the perhaps most compelling ‘evidence’ for such a strategy is the fact that individual Laboratories’ missions have been altered several times, but none has ever been closed.

It is in this context, the multifaceted National Laboratory system, that the first of the three cases of this study is situated. The synchrotron radiation program at SLAC National Accelerator Laboratory (former Stanford Linear Accelerator Center) is an illustrative example both of the institutional capacity of the National Labs to absorb, host and cultivate cross- or transdisciplinary ventures and of the capability of such ventures to in fact alter the dynamics and on longer term the mission and purpose of a whole laboratory. Growing from a role as ‘parasitic’ (see below) on the prestigious high energy physics program at SLAC, the synchrotron radiation program developed into a vital part of its host institution, getting free access to the accelerator (which indeed was both a prerequisite and greatly beneficial, see chapter 4) and could eventually deliver back to the organization the credibility that came with association with the development of synchrotron radiation. The unique combination of institutional autonomy and large, publicly funded infrastructural and human resources made the National Laboratory a natural harbor for one of the first trial efforts to exploit synchrotron radiation. SLAC had the liberty to grant the synchrotron radiation program access to the accelerator and the knowledge and resources to run the basic infrastructure required for its ex-

40. Nowadays the Department of Energy (DOE). In 1974 the Atomic Energy Commission was replaced by the Energy Research and Development Administration (ERDA), which was reshaped into a Department of Energy in 1977 (Smith 1990, p 92).

istence. Once this synchrotron radiation program and its eventual siblings elsewhere started to grow and establish themselves, they became natural inhabitants of the National Laboratory system, as they both served a multitude of disciplines and required extraordinary initiatives beyond the capabilities of single research institutions. The case study presented in chapter 4 is the story of the phenomenon of synchrotron radiation incarnated in this particular context.

Political setting two:

The politics of European scientific collaboration

CERN constituted a completely new form of scientific organization, by the joining of forces of a number of European countries through multilateral agreements as an attempt to utilize the collective capacity and achieve mutual benefit. It exploited a new and potentially very powerful funding source and was heavily influenced by the dynamics and cycles of international politics (Krige 2003, p 897). As the first intergovernmental collaboration in Europe after World War II, CERN was partly provoked by the threat of European scientific and technological development being left far behind, and partly the result of a comprehensive American strategy to secure interests in Western Europe. Thus the overarching scientific and political framework of CERN was a nexus of European 'big science' aspirations, scientific Marshall Aid, European cooperative ambitions, and the threat of brain drain. But the scientific collaborations in Europe also became institutionalizations of the essential European tension between national sovereignty and the common good, and they straightforwardly illustrate typical European strengths and weaknesses in science policy and general politics:

"Collaboration in a European scientific organization always involves a loss of, or at least a dilution of, national sovereignty. This loss is accepted but not taken for granted. Its scope is limited, carefully monitored and constantly re-evaluated" (Krige 2003, p 900).

As noted in previous sections, CERN was in its first decade of existence characterized by very little controversy and a striking unanimity in decisions over development and the laboratory's mission (Pestre 1990, pp 786-787; Krige 1996b, p 29), but the inevitable development towards larger and larger machines ended this honeymoon abruptly in the mid 60s. The multiplied costs

required to partake in the continued 'energy race' seem to be what woke nationalist stubbornness, and the site-selection and financing controversy over CERN II came thus to be the first outburst of the political plague of European scientific collaboration.

Initially, the major upgrade project was not contested by the member states (Pestre 1996, p 67), but when it became clear that the project was too large to be contained within the existing CERN collaboration and the project was turned into a new laboratory concept and a new collaboration, 'CERN II' (Ibid., p 68), a new location outside Switzerland, was suggested (Hadenius 1972, p 10). This seems to be what lifted the lid: considering the prospective benefits of hosting CERN II, participating but *not* hosting seemed an expensive and comparably unprofitable option, and so nearly all member countries made site proposals and started lobbying (Pestre 1996, p 68). Threats of withdrawal from the Federal Republic of Germany and the United Kingdom put the whole project in danger (Krige 2003, p 905; Martin and Irvine 1984, p 187), and when the suggestion was made to build CERN II at the existing CERN site in Geneva, it was welcomed with relief by the member countries (except for Germany which was forced to compel, Krige 1996b, p 33). Not only did this settle the site question, it also meant a substantial reducing of construction and running costs, which helped to ease the tensions considerably (Martin and Irvine 1984, p 187).

Once the CERN II debate had settled, it seems European collaboration in science got a somewhat restored confidence, with the establishment of the European Science Foundation (ESF)⁴¹ in 1973, the European Molecular Biology Laboratory (EMBL) in 1973, the neutron spallation laboratory Institute Laue Langevin (ILL) in 1976, the fusion research center Joint European Torus (JET) in 1977 and the European Synchrotron Radiation Facility (ESRF) in 1984 (Herman 1986, pp 150-159; Krige 2003, p 899). But most importantly, all of these collaborations, and especially the ESRF, came into being during a period of renewed 'Europeanism', centered around the Franco-German entente that became the backbone of the united (Western) Europe and enabled a successive deepening of the European Economic Community (EEC) cooperation, eventually leading up to the 1992 Maastricht Treaty (Judt 2005, p 529; Middlemas 1995, pp 100, 324).

Postwar European collaboration had taken its first stumbling steps already in the 50s, with the creation of CERN, the establishment of the European Coal and Steel Community (ECSC) in 1951, followed by the European Economic Community (EEC) and the European Atomic Energy Community

41. The European Science Foundation however cost practically nothing in comparison with CERN and was furthermore not a intergovernmental collaboration but rather a supranational agency.

(Euratom) in 1957 (Sharp and Shearman 1987, p 25), but took full speed in the 70s. The postwar recovery and reinventing of (West) Germany as a country and nation, as an economy and as an accepted member of the (Western) international community was a crucial part in the establishment of a new European order that would be lasting and that would prevent future aggression. Normalization of Franco-German relations was also a key in the process of European 'recovery':

"Spanning the EU's entire history, prevalent throughout and sometimes decisive in its impact, the Franco-German understanding (link, or entente, or axis) stands out above all others. It has been described both as a marriage of convenience and the 'motor of Europe', neither of which adequately explains its protean nature, its variation over time, and the interplay of personality, of institutions and separate national identities" (Middlemas 1995, p 323).

Emerging out of the economic downturn of the early 70s, the Franco-German entente was a comprehensive attempt – initiated on top political level – to create or achieve the "real Europe" (Middlemas 1995, p 75). With the UK rather glancing over the Atlantic Ocean than the English Channel,⁴² with Italy experiencing a continuing downward spiral in economy and politics through the late 70s and the 80s and falling behind the other big European countries, and with several other countries "on the defensive", the reinventing of (Western) European unity was placed in the hands of the partnering France and Germany (Ibid., pp 74-75, 80, 89-90). West Germany became – as a result of its history – the most prompt and at the same time obedient member of European collaboration, with practically no domestic political resistance against the EC and the continuously deepened collaboration associated with it, and repeatedly accepting to be the largest contributor to the EC budget (Ibid., p 117). The Federal Republic of Germany "seemed willing, even eager, to play second fiddle to the French", and France was just as eager to play the first (Cruise O'Brien 1997, p 80). The 1985 Single European Act, establishing the internal market, was the product of the Franco-German entente and paved the way for both the Exchange-Rate Mechanism (the first step towards a common European currency) and the Maastricht Treaty of 1992 (Anderson 1997, pp 66-67; Keegan 1997, p 88).

42. The UK attitude towards European collaboration (including the ECSC and the EEC) had been slightly skeptical all the way (Sharp and Shearman 1987, p 79), while deemed crucial by the other member countries (Peterson and Sharp 1998, p 29). UK membership in CERN was "against the trend of her foreign policy objectives at the time" and the involvement of the British was doubtful in the early stages of the CERN collaboration (see below) (Krige 2003, p 902).

The argument put forward by several authors, that practically nothing of the European Union we see today would have been put in place without the establishment of this strong alliance, is easily convertible to the third case study, presented in chapter 6. Though attempted to be settled by a careful evaluation of several optional locations, the site selection process and its attached basic negotiations over funding for the European Synchrotron Radiation Facility (ESRF) was on the contrary resolved by invoking of the Franco-German entente and a secret agreement between the two countries, achieved in late 1984. At this time, the 70s 'real Europe' project had started to materialize: one year later the 'Single European Act' would be signed. On the science and technology arena, a new institutional context for collaborative European R&D project had been built up within the EC on the initiative of the Belgian commissioner Étienne Davignon, giving birth to a number of specialized cooperative programs and coordination efforts⁴³ (Middlemas 1995, pp 112, 136, 251). It is no coincidence that the ESRF was established (with comprehensive and powerful investments, see chapter 6) at the height of the 'Davignon period'.

Now the existence of this basic foundation for prospective collaborations to build on and fall back on has certainly not erased the old 'CERN II' problems entirely, and the politics of European scientific collaboration are still largely centered on the balance between national sovereignty and the common good (Sharp and Shearman 1987, p 82). CERN nowadays enjoys the comparably success-laden position as the world's leading (or in one interpretation, only⁴⁴) high energy physics institute, and the investments in its two latest major accelerator projects were made largely without stirring controversy or debate. But site selection and the negotiations over shares are still complicated and potentially stalemating procedures, as among others the European X-ray Free Electron Laser (XFEL) project and the European Spallation Source (ESS) project recently have shown.⁴⁵

43. Such as ESPRIT (European Strategic Program on Research in Information Technology), started in 1983, and its successor RACE (Research and Development in Advanced Communications Technologies in Europe), initiated in 1988. Also EUREKA, a funding and coordination organization, established in 1985, and not least the three- to four-year 'Framework Programmes for Research and Technological Development', the first of which started in 1983, were products of this period (Middlemas 1995, p 251; Papon 2004, p 69).

44. With the investments from the United States and Japan, the LHC became the "first truly global experimental undertaking" (Brumfiel 2008, p 862) and the only experimental high energy physics program of its kind in the world. Thus CERN is no longer one of a few contenders in the high energy physics development (cf. the 'energy race', see above) but rather a global solo player.

45. Despite nearly a decade of concrete scientific, technical and political planning, the ESS has, at the time of writing, not been given any go-ahead, as no agreements have been reached on either

At the European Synchrotron Radiation Facility, the political difficulties of European scientific collaboration make themselves felt in the procedures for allocation of experimental time among research groups from the member states, and a gridlock situation regarding financial shares caused by the reluctance to reopen negotiations and risk a complete collapse of the collaboration. At the same time, the facility enjoys strong and comprehensive funding, a product of the potentially very powerful financial source that the assemblage of European countries may collectively form if the time is right and the political ramifications are beneficial. The case study presented in chapter 6 is the story of synchrotron radiation incarnated in the European collaboration context; the one that united around CERN in the 50s, and that battled over CERN II in the 60s and 70s, and the one that was imposed on the continent by the Franco-German entente in the 70s and on, and in which a lot of money and political prestige have been invested without managing to eliminate nationalist stubbornness.

Political setting three: Big science in a small country

“Sweden’s road to CERN II was tortuous. [...] To the Swedish physics community, the CERN II affair was a bewildering, almost traumatic, experience. It brought to light an inability to deal with decisions with financial implications of the magnitude associated with international high energy physics” (Widmalm 1993, p 110).

In Sweden, like in most member countries, CERN had been politically uncontroversial in its first decade of existence (Widmalm 1993, p 111), but CERN II meant financial commitments of entirely new magnitudes, and the political inability to handle the issue (Widmalm 1993, *op. cit.*) brought to light a seemingly general systemic shortcoming, due partly to Sweden being a small country but also with other clear historical, political and institutional reasons.

Enjoying a development similar to most Western countries in the immedi-

site selection or the details of the financing. The XFEL project was previously subject to similar delays but was decided upon in 2008. Ironically, this European political imperfection seems to hinder infrastructure projects for which political support would be comparably easy to mount (as both the XFEL and the ESS would serve a variety of scientific disciplines and are arguably closer to a range of utilities than high energy physics), while it has left high energy physics largely unaffected for the past three decades and let CERN become the global leader at the expense of the United States.

ate postwar decades, with strong growth in most parts of the scientific research enterprise and an intensified relationship between science and the government, Sweden developed a public science system dominated by universities and with limited coherence in policy and planning, which has been in place since, with little changes. Governmental science policy measures taken in the 50s established a permanent structural division between the research councils as funders of research and the universities as performers (Schilling 2005, p 53). The councils, formed in the 40s, were largely ruled by representatives of the scientific community (Premfors 1986, p 13), and the universities were given an almost monopolistic standing as performers of basic research, at the expense of other possible solutions such as the establishing of an institute sector (Nybm 1997, p 101). Expansion of public higher education during the decades has been coupled with a focusing of governmental policy for the university system on education policy rather than research policy in the 60s and on, and the government has continued to show unwillingness to take initiatives in research policy (Premfors 1986, pp 15-41).

The dominating role of the universities as performers⁴⁶ may lead to the erroneous conclusion that the *power* over the science policy in Sweden lies with the universities. However, central university management is weak, mostly because of limited power over financial resources. Such a research funding system, with a large portion of governmental money going directly to the universities in large block grants already distributed among the main research areas (the faculties), tends to consolidate research management on departmental and disciplinary level at the expense of central university management and multidisciplinary governmental science policy agencies (Benner and Sandström 2000, p 444; Melander 2006, p 133). Since individual scientists act on basis of their own special interests, a science policy system governed by scientists will be pluralistic and weak. It will be decentralized and in some sense 'democratic', but it will show a certain inability to make strategic priorities or embark on large scale commitments. Swedish research policy in the natural sciences is thus decentralized and governed mostly bottom-up, lacking "aggregation mechanisms", i.e. institutional means of mobilizing resources or support, apart from the ones developed by the scientists and research groups themselves (Benner 2008, p 222). The political attempts of the recent two decades to turn Sweden into a 'knowledge society' or 'knowledge economy' has led to some changes intended to strengthen initiative and coherence, but their practical outcomes have not corresponded to these in-

46. In 2005, approximately 72% of the governmental R&D expenditure (in total 21,810 MSEK, approximately 2,000 MEuro) was absorbed by the university sector (*VR Research Financing Report 2006*, p 11).

tentions. Efforts to increase autonomy and revitalize Swedish public research through increased financing have been undone by economic downturns and accompanying retrenchment actions, and redefinitions of the role of universities through partial replacement of the traditionally academic with a more effective university organization and a broader societal mandate have further weakened the universities as actors in the science policy system (Benner 2001, pp 162-165).

In recent years, attempts have been made at the universities to counter their traditionally strong bottom-up governance (Benner 2008, p 391), but the real conditions for governance are naturally still determined by the structure and channeling of funding. The merging of the four research councils⁴⁷ and the functions of the National Council for Planning and Coordination of Research (Forskningsrådsnämnden, FRN) into a single Swedish Research Council (Vetenskapsrådet, VR) in 2001 was a sign of a “discursive” shift of Swedish research policy towards ‘excellence’ and ‘strategic choice’ (Melander 2006, p 138), but it remained largely ‘discursive’. The council is still governed by scientists and its annual budget is still defined by the government and channeled to the three scientific councils created in the merger,⁴⁸ with very little room for occasional increase due to larger strategic commitments or priorities within the overarching research council organization (Benner 2008, pp 298-299, 382).

Two special efforts have been made on council level with regard to research infrastructure. The first one was the 1977 creation of the National Council for Planning and Coordination of Research with the task of handling “collaborative projects considered to be of special societal importance” (Premfors 1986, p 28). The planning and coordination council’s lack of ties to specific scientific areas gave it a slightly more unrestricted role in the system (Benner 2008, p 361) and could therefore commit to funding of trans- or interdisciplinary projects, such as research infrastructure (see chapter 5) (Forkman 2001, p 180; Premfors 1986, p 28). After the merger of the research councils in 2001, FRN ceased to exist as separate entity, but in 2005, the council created the Committee for Research Infrastructures (Kommittén för Forskningens Infrastrukturer, KFI) to coordinate matters of infrastructure within the council. Its primary function has been the publication of a Swedish ‘roadmap’ for research infrastructures, *The Swedish Research Council’s Guide*

47. The Humanities and Social Sciences Research Council (Humanistisk-Samhällsvetenskapliga Forskningsrådet HSFR), the Medical Sciences Research Council (Medicinska Forskningsrådet, MFR), the Natural Sciences Research Council (Naturvetenskapliga Forskningsrådet, NFR) and the Technical Sciences Research Council (Teknikvetenskapliga Forskningsrådet, TFR).

48. For medicine, natural and technical sciences, and humanities and social sciences.

to *Infrastructure*, issued for the first time in 2006. Though the committee has probably managed to create coherence and increase coordination with respect to Swedish research infrastructure, its budget is still contained within the council structure with no change in the prospects of having the government add to it on basis of strategic commitment⁴⁹ (*Karlsson interview*).

Against this background, it is possible to conclude that Sweden's resolving of the general small country's dilemma in research policy – what priorities to make, at the expense of what – has been to avoid taking any stand. Sweden maintains a broad and ambitious spectrum of research activities and does hardly ever, on national level, make active priorities between different fields. Sweden participates in most European intergovernmental research collaborations⁵⁰ and the importance of high return for investment on those arenas is continuously emphasized by the research council and other governmental bodies.

Seen in historical perspective, the key result of the CERN II debate was Prime Minister Palme's referral of the whole issue back to the scientific community, urging the scientists to take responsibility for their own priorities, also when coupled with enormous financial commitments (Widmalm 1993, p 121). This decision on part of the prime minister merely reflected an already built-in mechanism in Swedish science policy, but it was a comprehensive summary of the official Swedish line with regard to large scale initiatives, including research infrastructure: Either it concerns domestic projects or participation in international collaborations, investments must be made on basis of priorities within the existing system.⁵¹ In reality, all such priorities are set within the research council, not primarily by strategic evaluation and

49. The budgets of the Swedish national facilities and all Swedish memberships in international collaborations (see note 50) thus continuously need to be contained in the zero-sum game of the KFI budget (*Karlsson interview*).

50. VR represents Sweden in the following international research organizations: the European Organization for Nuclear Research (CERN), the European Southern Observatory (ESO), the European Synchrotron Radiation Facility (ESRF), the European Molecular Biology Laboratory (EMBL), the European Molecular Biology Conference (EMBC), the International Agency for Research on Cancer (IARC), the European University Institute (EUI), the European Incoherent Scatter Scientific Association (EISCAT), the Global Biodiversity Information Facility (GBIF), the IceCube Neutrino Observatory, the Institut Laue-Langevin (ILL), the International Neuroinformatics Coordinating Facility (INCF), the Integrated Ocean Drilling Program (IODP), the Joint European Torus (JET), the Nordic Data Grid Facility (NDGF), and the Nordic Optical Telescope (NOT). (*VR Annual Report 2007*, p 76) The 2007 total amount of contributions paid to international projects by VR was 299,6 MSEK (*Ibid*, p 32).

51. This is exactly what happened as a result of Sweden's commitment to CERN II. Not only was the experimental high energy physics program in Lund terminated (see chapter 5), resources were also drained from other scientific areas along the whole spectrum of the natural sciences (Widmalm 1993, pp 123, 126).

decision-making or on basis of political will, but as part of ordinary budgetary considerations.⁵² There seems to be both a lack of mechanisms for taking large-scale strategic initiatives on governmental level – a lack that is due to the structure of the science system as described above – and a lack of political will to make such commitments.⁵³

It is in this political context that the second case study is situated. Since there is no established structure through which the government can take hold of and execute management over for example large scale initiatives like a new facility or a major upgrade of an existing, initiatives must grow elsewhere and gain support and momentum in an ‘organic’ way to establish themselves as serious contenders for either a piece of the VR cake or, as in exceptional cases, direct government support. MAX-lab is one example of such an initiative that grew ‘organically’ and that came into being despite the political circumstances rather than because of them. The case study presented in chapter 5 is the story of synchrotron radiation incarnated in this political context.

Conclusions

A fact as simple as it is fundamental ties synchrotron radiation to high energy physics and makes the history of high energy physics a prehistory of synchrotron radiation – or perhaps synchrotron radiation the sequel of high energy physics – namely the fact that they make use of the same basic technology: particle accelerators. The growth of high energy physics was enabled by and could arguably not have occurred without the establishment of the social contract for science, unleashing of a large stream of funding from governments to basic research, and the ‘military-scientific complex’, making ‘all things nuclear’ a top priority. The scientific exploration of the atom and its

52. To contain all infrastructures, regardless of their form, function, or organizational character, in the same budget is somewhat risky, given the nature of some certain international commitments. This is mentioned in the KFI Guide to the Infrastructure from 2008 with an example: Infrastructures operating under international conventions, such as CERN, often determine the level of the members’ contributions to the GDP of the member countries and adjust it as the GDP changes. In the case of Sweden, this means that growth in the domestic economy increases the membership fees automatically, without any increase in the governmental funds for VR, which of course causes some problem within the council. “This is an illogical way to finance infrastructures and leads to problems in long-range budgetary planning” (*KFI Guide 2008*, pp 22–23).

53. With possible exception for the Swedish declared candidacy for hosting the European Spallation Source (ESS), an initiative taken entirely on governmental level, short-cutting the research council’s attempts of creating coherence and coordination in matters of research infrastructure through its Committee for Research Infrastructures.

smaller and smaller constituent particles was furthered in complete reciprocity with the technological development of accelerators. Therefore it was a basic premise of high energy physics – from scientific as well as political (and thus funding) perspective – that the next step in its development always entailed the construction of a new accelerator.

From the science policy view, the political transparency and simplicity of big physics is extraordinary: there was always a next step in the development, a next machine bigger and costlier than the previous one, and its funding and construction was close to imperative in the Cold War context. Scientists formulated a goal, and politics responded. Though this formulated goal was perhaps not comprehensible in all its details, it was easily measurable in terms of accelerator performance. And though politics not always gave a positive response, their choice was simple because it could always be referred back to the fulfillment or abandonment of a measurable goal.

Small science was never swallowed nor completely outmaneuvered by big science. It was in fact quite the opposite – when the Cold War wound down, small science made a comeback of sorts, with biology and life sciences taking the leading role as the spearhead in scientific benefit to society. But sophistication and collectivization of science and the increasing dominance of ‘projects’ and ‘teams’ (e.g. Ziman 1994, 2000), together with a general shift towards results orientation, productivity and the innovation aspects of science (Freeman 1988, p 115; Guston 2000, pp 113, 116-117; Pestre 2005, p 38; see also chapter 1 and above) has made also small science increasingly dependent on centralized organizational constructs and technically advanced infrastructure.

Though theoretically predicted long before any particle accelerator was even on design stage, synchrotron radiation needed a certain level of accelerator development to occur in reality, and though theoretical predictions had been made about its potential usefulness, several things stood between the idea and its practical implementation (see chapter 3). Thus when concrete plans emerged to make use of synchrotron radiation, it was in the 60s and big physics still ruled, especially when it came to accelerator construction and utilization. Thus the only reasonable path to its realization was as an activity ‘parasitic’ to high energy physics.⁵⁴ This ‘parasitic’ relationship was developed over the decades and can be said to still exist in some sense, not least when considering the ‘fall’ of big physics and the ‘vacant space’ high energy phys-

54. It shall be noted that the word ‘parasitic’ as used in this section and throughout the rest of the thesis is commonplace in the whole synchrotron radiation community. High energy physicists however prefer to call the relationship between their field and synchrotron radiation ‘symbiotic’ rather than ‘parasitic’, which probably is due to the words’ different semantic connotations: ‘symbiotic’ arguably sounds nicer (*Winick 2 interview*).

ics has left behind, both with outdated accelerators no longer used and thus surrendered, and with priorities redirected away from the field of high energy physics. The parasitic relationship has manifested itself in two concrete ways. First, in the 60s and 70s and 80s, synchrotron radiation programs in Europe, the United States and Japan developed their experiments by using the radiation produced by the high energy physics machines and made use of the knowledge and competences of high energy and accelerator physicists. Second, in the 70s and 80s and on, synchrotron radiation programs took over obsolete high energy physics machines. In recent years, they have also started to take over whole laboratory organizations and caused a shift of missions of labs previously dedicated to subnuclear study towards synchrotron radiation and the related.

The parasitic synchrotron radiation programs at high energy physics machines of the 60s and 70s are briefly accounted for in the next chapter. Though they occurred at several places more or less simultaneously, they were not enabled by any specific scientific or technical development (other than the general accelerator development) or any policy measure; rather they were small scale initiatives on local level. However when synchrotron radiation programs started taking over high energy physics accelerators in the 70s and on, it was partly a result of discontinuous events accounted for in previous sections, namely the big leap in size of high energy physics accelerators in the late 60s and early 70s. Not least did the realization of the CERN II project divert sufficiently large sums of money from the national high energy physics programs of its member states (see above) to lead to the closing down of practically every high energy physics accelerator in those countries.⁵⁵ CERN can also, in a somewhat more remote sense, be said to have influenced the creation of the laboratory in the third case, as it was the prototype collaboration in Europe and paved way for subsequent efforts.

From this chapter's historical and political exposé, the main conclusion relevant for the following analysis is thus that synchrotron radiation – as a small scale and bottom up venture emerging at several places and establishing itself as parasites on high energy physics laboratories – was aided in its

55. In the UK, the decision to join CERN II lead to the discontinuation of both the domestic experimental high energy physics programs, in Daresbury and at the Rutherford Lab in Appleton (Hughes 2002, p 120), of which the Daresbury accelerator became a synchrotron radiation facility in the late 70s (Robinson 1981a, p 853). In Sweden, the decision to join CERN II had the immediate effect that the accelerator program in Lund was discontinued, and from its remnants the earliest version of a synchrotron radiation facility rose (see chapter 5). Worth noting is of course the somewhat surprising forsaking of the entire UK domestic high energy physics for CERN, especially against the background of their previous reluctance to even participate (Krige 2003, p 913). The German pursuit of a domestic program at DESY is perhaps somewhat more logical given their unwillingness to participate if not the lab would be located in Germany (see above).

development and establishment by the transformation of high energy physics to 'megascience'. Most obviously, the resource concentration and growth in accelerator sizes left machines deserted and ready to be taken over by synchrotron radiation. On the personnel side, accelerator physicists took the opportunity to switch 'lords' and devote their talent to radiation production purposes. The recent plans to take over larger high energy physics machines – primarily the two very similar machines in Hamburg and at Stanford, follows roughly the same pattern. High energy physics machines becoming obsolete, this time due not to a large step in the rise of big physics but rather its fall, has enabled the redirection of their activities to synchrotron radiation. In chapter 4, the case study of synchrotron radiation at Stanford continues the charting of the tight relationship between synchrotron radiation and high energy physics and all the various concrete forms it took in that specific context. The science policy context of the National Laboratory system, described above, appear as influential in shaping this development and auspicious for the early establishment of synchrotron radiation. The two other cases, in chapter 5 and 6 respectively, concern laboratories in Sweden and on collaborative European level, and have clear connections back to the history and politics presented in this chapter. The powers and weaknesses of high-level European collaboration on one hand and a small scale university-based venture on the other are both related to their two very distinctly different political realities, both of which have been described in previous sections. They also take up as main features other important dimensions of the emergence and establishment of synchrotron radiation in science.

As mentioned, with no Cold War threat forcing the United States to consider every potential breakthrough in physics military/strategically important, the interests in big physics were consequently diminished, and the strategic interests in science became diverted to radically different areas in 'small science', such as medicine, nanotechnology, and the life sciences. Future high energy physics accelerators seem to necessitate a global collaboration. With regard to the strategic interests in 'small science', the scientific communities and the governments in North America, Europe and Asia have long since started realizing that accelerators can be utilized for exactly such purposes, and these purposes have long since started to take over.

Synchrotron radiation and synchrotron radiation laboratories

The accelerator physics nuisance

The overall purpose of this thesis – to describe and analyze synchrotron radiation laboratories from a science policy perspective – has two fundamental prerequisites: to understand the political (and thus also historical) context of these laboratories, and to understand their science and technology on a basic level. The preceding chapter attempted to give the general background with respect to history and politics and to present an important (pre)historical factor for synchrotron radiation, namely high energy physics. The present chapter aims at describing the science and technology of synchrotron radiation so that a general understanding of its multidisciplinary and technically sophisticated character can be gained and put to use when analyzing its science and science policy dynamics.

The development of synchrotron radiation from ‘esoteric endeavor’ to ‘mainstream activity’ was identified in the beginning of chapter 1 as a focus for the analysis. This development through which synchrotron radiation laboratories established themselves in science and science policy was scientific, technical, sociological and political. Three of those parameters – the technology, the science and the sociology – are described in this chapter, and the development itself is outlined. In all, it seeks to establish a basic and summarizing understanding of the conditions upon which the analysis rests, and therefore the accounts are deliberately held on a descriptive level.

High energy physics accelerators were the physical and symbolic centerpieces in the big physics hegemony whose rise and fall coincided with the Cold War and its end. Growing continuously in size and cost during the five decades after the war, these accelerators were of two basic designs – linear accelerators, ‘linacs’, and circular accelerators (cyclotrons, synchrotrons and storage rings, see chapter 2). One of the technical difficulties of constructing circular particle accelerators is that particles traveling at high speed lose energy when their trajectories are bent, for example when kept in a circular path. The energy loss is emitted as so-called synchrotron radiation. Since high energy physicists seek to charge particles with as high energy as possible,

the unavoidable energy loss in circular accelerators has traditionally been a nuisance in accelerator physics (Robinson 1975, p 1074; Shenoy 2003, p 3). Therefore, long before synchrotron radiation became utilized for experimental purposes, accelerator and high energy physicists who wanted to minimize the energy loss and get rid of the radiation studied its origin and characteristics in detail. The first accelerators with which the energy loss could be detected and diagnosed were called 'synchrotrons' (see below), and so the radiation bears the name 'synchrotron radiation'.⁵⁶ In strict terms, the name 'synchrotron radiation' is erroneous. It is due only to the fact that the radiation was *first observed* emerging from a synchrotron. Hardly any of the experimental work done in the field – other than some very early exploratory projects – has been done by the use of radiation from synchrotrons. But despite being technically erroneous the name has stuck, also within the field, and it is never contested. The word 'synchrotron' has also been refurbished and is nowadays frequently used as a name for storage rings purpose-built for synchrotron radiation production or in some cases a short name for 'synchrotron radiation laboratories' or 'synchrotron radiation facilities'. Furthermore, synchrotron radiation is sometimes referred to as 'synchrotron light' or plainly 'light', which is not entirely correct as light usually means visible light (see figure 1 on page 78), but it has nonetheless become an established term.

The theoretical prediction of synchrotron radiation was done already in 1864 by the British physicist James Clerk Maxwell, in the famous equations that became the basis of classical electrodynamics (the *Maxwell equations*) (Daintith 2005/1995, p 312; Blewett 1998, p 135). Maxwell's rather general statement about the behavior of electrons was further developed in a 1898 paper by the French physicist Alfred Liénard who described mathematically the basic theory of synchrotron radiation (Blewett 1998, p 136). However, to experimentally confirm these theoretical predictions, sophisticated accelerator technology was required, and hence it was not until 1947 that synchrotron radiation was first observed (in the form of very intense visible light), at the General Electric Research Laboratory in Schenectady, New York (Ibid., p 138).

As a phenomenon of interest in fundamental physics and a newly discovered physical side effect of accelerator physics, synchrotron radiation was studied in detail during the remaining 40s and 50s (Winick and Bienenstock 1978, p 39), primarily theoretically, predicting the properties of the radiation as a function of accelerator performance with the measurable charac-

56. It should also be mentioned that 'synchrotron radiation' has been known as a celestial phenomenon for a long time. Accelerated particles that change direction by the influence of a force of some kind is a common feature in space, and the emitted radiation is similar to that in particle accelerators and also referred to as 'synchrotron radiation' (Schlickeiser and Frahm 2006, p 2).

teristics of the radiation as starting point. Occasional experimental work was also done at the time, verifying the basic theoretical predictions regarding the character and quality of the radiation and giving some experience in the use of the radiation with regard to optics and other technical components necessary for handling it (Ibid., p 41; Haensel 2007, pp 16-17). It became clear from these studies that synchrotron radiation produced by the electron accelerators at the time was infrared, visible light, ultraviolet and x-rays (see below), of higher intensity than had ever been achieved before. The step from that realization to practical utilization of the radiation was largely a matter of developing techniques for practical handling of the rays and getting access to high-performing accelerators.

Electromagnetic radiation

Light, or 'electromagnetic radiation', is waves of energy propagating through space. Different kinds of electromagnetic radiation are characterized by different wavelength. Electromagnetic radiation is caused by the disturbance (or 'perturbation') of an electromagnetic field, which causes the emission of waves, just like a rock thrown into water causes a perturbation on the surface that makes waves propagate from the site of the perturbation. Electromagnetic waves needs no medium to propagate, and in vacuum they travel with the speed of light.⁵⁷ The so-called 'wave/particle duality'⁵⁸ of physics allows for an interpretation of electromagnetic radiation waves as particles by the name 'photons' (Daintith 2005/1995, p 567). The electromagnetic spectrum (figure 1) is the collection of all electromagnetic radiation, identified and categorized by its wavelength, from radio waves to gamma rays. Only a very small part of the spectrum is visible light, and the color of visible light is determined by the wavelength within that range. Surrounding visible light is infrared, with longer wavelength, and ultraviolet, with shorter. As seen in the figure, the full electromagnetic spectrum ranges from wavelengths of picometers⁵⁹ to several kilometers, and radiation of all possible wavelengths in between exists. Wavelength is measured as the length of one full period, i.e. the length

57. The speed of light is approximately $2.99 \cdot 10^8$ m/s (Daintith 2005/1995, p 150). If material is present, the waves propagate marginally slower.

58. The wave/particle duality is fundamental to physics, and holds that particles can show wave-like behavior and waves can show particle-like behavior, and thus depending on the circumstances and the framework of the specific situation, all particles can also be interpreted as waves, and vice versa (Daintith 2005/1995, p 567).

59. One picometer is 10^{-12} or 0.000000000001 m (Daintith 2005/1995, p 390).

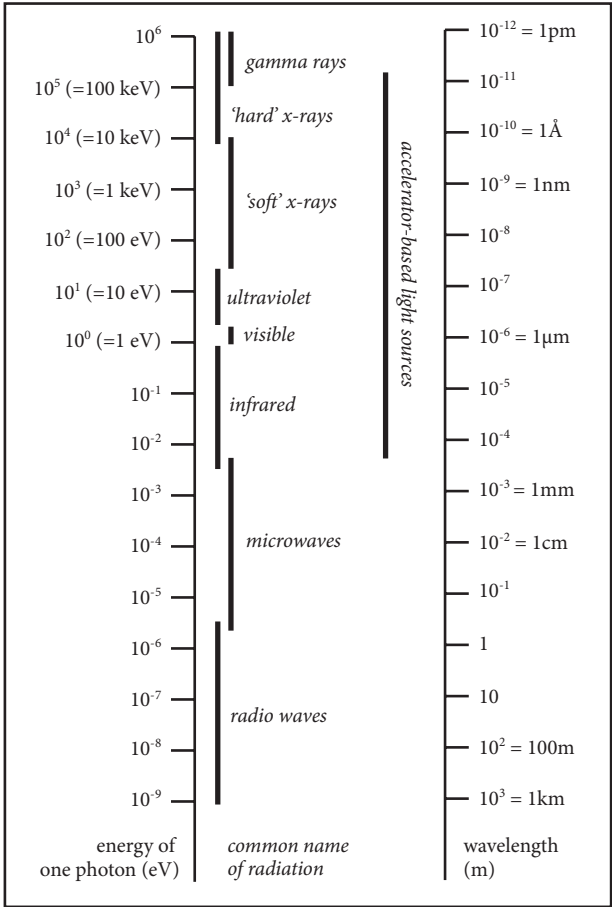


Figure 1: The electro-magnetic spectrum. (Daintith 2005/1995, p 583)

between two successive points of equal phase in a wave (Daintith 2005/1995, p 583). It is also common to refer to radiation of different wavelength as having different energy, as the energy of the photons (measured in electron volts) corresponds to wavelength (see figure 1).

Photons or electromagnetic waves have been utilized as a primary scientific tool since the beginning of human inquiry of the world, simply because they make up the physical precondition for seeing. Visible light is “the form of electromagnetic radiation to which the human eye is sensitive and on which our visual awareness of the universe and its content relies” (Isaacs et al 2003/1984, p 455). Through the ages, new sources of light have been exploited as aids for seeing, and in the recent century, light other than the vis-

ible, i.e. radiation in other regimes of the electromagnetic spectrum has been manipulated and utilized through various technological achievements and thereby made possible a broadening of the scope of things possible to 'see' and study. The progress in finding new sources of radiation and new ways of its utilization has created opportunity of investigating the 'invisible', i.e. things that are so small or so fast that purely physical constraints hinder the human eye to capture them even with the most advanced magnifying optics or recording media.

X-rays and their utility

Wilhelm Conrad Röntgen's 1895 discovery of so-called 'x-rays' (Daintith 2005/1995, pp 459-460) marked a significant step in the development of scientific 'seeing' techniques. X-rays immediately proved useful not least in medicine (e.g. Mould 1996, pp 22-27; Hessenbruch 2000, pp 397-398), but their true nature remained unknown⁶⁰ for a couple of decades, until it was shown in 1912 that x-rays diffract just like visible light, only on a much smaller scale, which proved that x-rays was electromagnetic radiation of very short wavelength (Dardo 2004, pp 94-95).

The first application of x-rays and the one probably most commonly associated to x-rays was 'radiography', a collective name for the common medicine applications (hospital x-ray machines), scanning of materials, and security checks on luggage. The principle is very simple; different materials absorb different amounts of x-rays and will thus create different 'shadow' images on the film placed behind it (Franks 1996, pp 6-7). Other applications that emerged during the first fifty years of x-ray utilization for science include diffraction/scattering, microscopy and different varieties of spectroscopy.

The human eye is incapable of seeing objects or details of objects that are smaller than the wavelength of visible light, because they cannot reflect visible light and thus there is nothing for the eye to register, even if the world's sharpest lenses are used.⁶¹ However, recording of the reflection of ultraviolet and x-ray radiation from small objects is possible and is one variety of x-ray microscopy, a common application of x-rays (Nixon 1996, p 43). Other varieties are used for similar purposes, either to make 'photographs' of very small objects or to produce extremely detailed images of things (Ibid., pp 46-58).

60. Hence also their original name that still sticks in most languages: 'x-rays' where 'x' denotes 'unknown' (Seliger 1995, p 25).

61. This is called the 'diffraction limit' of visible light. There are similar limits for all kinds of radiation; it cannot be reflected by structures that are smaller than the wavelength of the radiation.

Spectroscopy with x-rays, of which several varieties exist, utilize the capacity of x-rays to disturb the electronic structure of atoms by forcing an electron closest to the atomic nucleus to leave its position, thereby creating a 'hole' which must be filled by another electron. This process is both in itself detectable and a trigger of other detectable events in the atom or molecule. All elements react this way to the exposure of intense x-rays, but different elements correspond to different wavelengths (Isaacs et al 2003/1984, p 845), and not only radiation in the x-ray regime is used for spectroscopy but also ultraviolet and infrared. The exact mechanism of various spectroscopic methods differs a lot, as does what kind of event or phenomenon is detected. X-ray scattering or diffraction makes use of the deflection of x-rays by crystalline materials and is a dominant method for the understanding of properties of materials in terms of three-dimensional structure of the atoms of which they are composed (Fuller 1996, p 101).

X-rays are commonly referred to as 'soft' and 'hard', corresponding to longer and shorter wavelength, respectively. The reason for this dates back to before the real nature of x-rays was known, when the quality and character of the x-rays were determined by their penetrating power. X-rays of higher energy (shorter wavelength) had greater penetrating power and were therefore named 'hard' x-rays, and the ones with less penetrating power were designated as 'soft' (Long 1996, p 61).

Accelerators as radiation sources

The first accelerators with real practical utility as producers of radiation were storage rings, because they keep a constant current⁶² and thus produce a continuous flow of radiation. For high energy physics experiments, protons are nowadays the most commonly used particles in accelerators, but for synchrotron radiation production, several things make electrons most suitable, beside the fact that they are easily handled. Most of all, electrons charged with energy over 1 GeV ('giga electron volts'⁶³) are 'ultra-relativistic', meaning that they show distinct 'relativistic behavior' when accelerated to close to the

62. The electrons in the ring are called the current, and the current is measured in Amperes (A). One ampere corresponds to roughly 10^{13} electrons (Williams 1982, p 334), meaning that a storage ring with a current of 200 mA (0.2 A, a typical current of a modern storage ring) circulates about two trillion (2,000,000,000,000) electrons.

63. 'Electron volts' (eV) is a measure of energy carried by particles. Mega means one million and giga one billion. It is defined by referring to one electron passing through a potential electric field of X volts, by which it will acquire an energy of X electron volts (eV) (*SLAC Story*, p 11).

speed of light and therefore, when following a curved path in a magnetic field, they emit energy – synchrotron radiation – to a far greater extent than below 1 GeV (Marks 1995, p 326-327). Accelerating a bunch of electrons and controlling the position of this bunch inside the ring is however tricky matters; electrons are negatively charged and repel each other by nature, leading to a spontaneous defocusing that has to be countered with delicate (and forceful) technology (Margaritondo 2002, p 19-22).

Production of synchrotron radiation relies on the fundamental property of electrons moving close to the speed of light of inevitably losing energy when their trajectory is bent. In a storage ring, so-called ‘bending magnets’ (they bend the electron beam) are located in each corner of the polygon-shaped ‘ring’ and keep the electron beam in its trajectory, while at the same time causing the emission of radiation. Nowadays, so-called ‘insertion devices’, mounted in the straight sections of the ring, are the most efficient sources of radiation as they are designed specifically for the purpose (see below). The motion of the electrons is an oscillating charge and it emits electromagnetic waves similar to an oscillating charge in an antenna emitting radio waves (which are also electromagnetic radiation, see figure 1). The wavelength of the radiation depends on the oscillation frequency of the charge, i.e. the (angular) speed of the electrons. So-called ‘relativistic effects’⁶⁴, at play when the particles reach ‘relativistic energies’, shortens the output wavelength drastically and makes the storage ring produce infrared radiation, visible light, ultra-violet radiation and x-rays and not just radio waves like an ordinary antenna (Margaritondo 2002, p 10). However, the relativistic effects only function as ‘amplifiers’ of other variables, and the basic parameters for the radiation are always determined by fundamental design characteristics of the storage ring, such as electron energy, size of the current (i.e. the number of electrons accelerated), strength of the magnetic fields, and the degree to which the so-called ‘emittance’ is reduced (see below).

The electron beam is produced in an ‘injector’, often a linac, and accelerated to the desired energy in a ‘booster’ accelerator before injected into the main ring (*M Eriksson 2 interview*), but some laboratories inject at lower energies and heighten the energy of the electron beam in the main ring.⁶⁵

64. These ‘relativistic effects’ are due to the fundamental feature of relativity; that moving objects have different velocities depending on frame of reference. The exception is the speed of light, which is always constant, and so for particles with velocities very close to it, these ‘relativistic effects’ introduce themselves (Margaritondo 2002, pp 10-18).

65. In effect, this type of lower energy injection means longer ‘downtime’, i.e. time periods with no radiation delivered to the beamlines. It is however used at some laboratories for economic reasons, since full energy injection requires a second separate accelerator only functioning as ‘booster’ (*M Eriksson 2 interview*).

To compensate for the energy loss caused by the emission of the radiation, devices called ‘radiofrequency (RF) cavities’ are located in some straight sections of the rings, exposing the electrons to an electric field that enhances their energy.

Since electrons tend to interact with everything in their way, the tube in which the electron beam travels in a storage ring holds so-called ‘ultrahigh vacuum’, i.e. very low air pressure⁶⁶ (Margaritondo 2002, pp 59-60). It does, however, take more than the bending magnets and the ultra high vacuum to keep the electron beam in the desired position. Different types of magnets are located in the storage ring’s corners and straight sections, making up the so-called ‘magnet lattice’ of the ring, and are used for controlling the position, focus and emittance of the electron beam, i.e. the fundamental performance of the ring as a radiation source (Marks 1995, p 325; Winick 1994a, pp 477, 483, 485). Although the magnet lattices of modern storage rings perform the exact same task, controlling the electron beam, every storage ring is built with unique lattice designs, optimized for the specific ring design with regard to its size and its exact parameters. An accelerator design is always optimized as a whole and though the function of two rings is the same they are never identically built (*Nyholm 1 interview*).

The electrons move in slightly different trajectories, so that the cross-sectional area of the electron beam is much larger than one electron, which ultimately has an impact on the quality of the radiation. Through technical improvements in different parts of the magnet lattice, the ‘size’ of the beam can be decreased and unwanted deviations of electrons from their desired paths easier to avoid. This is summarized as decreased ‘emittance’, and the development of storage rings in this respect has been significant over the years, especially in the last two decades (Shenoy 2003, pp 3-4).

However sophisticated the technologies are to keep the electrons in desired order – focusing, compensating energy loss and controlling bunch structures – the amount of electrons in the beam decreases naturally (Margaritondo 2002, p 22). After a certain period of time (in modern rings more than 24 hours) the intensity of emitted synchrotron radiation, proportional to the number of electrons in the beam, is too low to be useful, and new electrons must be injected. This is of course also needed if the entire electron beam is lost at a time (so-called ‘beam dump’), which happens recurrently because of human error and occasional malfunctioning of individual components in the storage ring complex. The ordinary lifetime of the beam depends on a number of accelerator design parameters. Low emittance, high vacuum, and high

66. Ultrahigh vacuum means a pressure lower than 10^{-13} atmospheres. For comparison, normal air pressure is 1 atmosphere, i.e. 13 orders of magnitude higher than in the vacuum pipes of a storage ring (Margaritondo 2002, pp 59-60).

precision magnet lattice (especially effectiveness of the RF cavities) are factors increasing beam lifetime (Marks 1995, p 316). At some laboratories equipped with separate booster accelerators, so-called 'top up' or 'top off' injection is customary, meaning that the electron current in the ring is continuously re-filled and the quality of the synchrotron radiation is kept at a high and steady level, also stabilizing the heat load on optical components (Shenoy 2003, p 4, Petroff 2007, p 19).

Early synchrotron radiation history

The theoretical predictions and calculations of the character of synchrotron radiation that were experimentally confirmed in the 50s and 60s made clear that the radiation produced by synchrotrons – if it could be properly extracted and handled – provided a very intense source of electromagnetic radiation in the infrared, visible and ultraviolet regimes. Theory and calculations suggested that particles of higher energy would produce radiation in the x-ray regime, with an intensity never previously achieved. Thus the quality and character of the radiation as such was well known in the early 60s, and exploratory experimental work could be initiated at the Frascati Synchrotron in Italy in 1961, the National Bureau of Standards in Washington, D.C. and the Institute for Nuclear Study in Tokyo in 1963, and at DESY in Hamburg in 1966 (Winick and Bienenstock 1978, p 41; Munro 1996, p 132). The lack of stability and the inability to control beam positioning in the accelerators, however, made the use of these synchrotrons unreliable, and it wasn't until the advent of storage rings that experimental work could be planned and executed on some routine basis.

The first challenge that met the early entrants in the field was simply getting the light out at all. Not only did they have to subordinate their work entirely to the accelerator and high energy physicists in charge of experimental schedules and machine operation and rely entirely on the goodwill and generosity of physicists occupied with prestigious and expensive particle collision experiments. The position and focus of the radiation beam is dependent on the position and shape of the electron beam in the accelerator, and beam positioning on that level of detail was not the explicit interest of high energy physicists who were happy as long as the particle beams collided with reasonable luminosity (*Lindau 1, Winick 1 interviews*). The worth of trying to make use of these unstable radiation beams from existing accelerators was questioned, also by researchers within fields with good potential of improving their techniques with higher intensity radiation (e.g. Kunz 2007, p 14).

All the 60s parasitic and exploratory programs in Europe, Japan, USSR and the US were done with synchrotrons (Haensel 1994, p 15; Jaegle 1989, p 22) (except for one storage ring machine at the University of Wisconsin, Madison), and none of them were able to produce radiation in the x-ray regime of the electromagnetic spectrum. The experiments carried out were therefore mostly spectroscopic studies in solid state physics making use of ultraviolet radiation (Winick and Bienenstock 1978, p 34). At the synchrotron at DESY in Hamburg, however, a program was carried out in collaboration with the Hamburg outstation of the European Molecular Biology Laboratory (EMBL), in diffraction studies of the contraction of frog muscles(!) (Huxley and Holmes 1997, pp 366-368).

When the first storage rings with higher energy opened at Stanford and in Hamburg in 1972 and 1974, respectively, radiation of a spectrum stretching well into the hard x-ray regime was made available (Doniach et al 1997, p 380; Metz 1975, p 445). Pioneering work was done in x-ray spectroscopy, scattering, microscopy and imaging (see below), utilizing the high intensity of the radiation (Munro 1996, p 134). The handling of the very intense x-rays presented the early synchrotron radiation researchers with some problem. The intensity and short wavelength of the radiation required development and construction of entirely new optical elements (such as mirrors and gratings⁶⁷) that was capable of reflecting and focusing the radiation and withstand the thermal load (*Lindau 1, Nyholm 3 interviews*).

In the 70s, synchrotron radiation techniques started to become practically exploitable for new applications, and synchrotron radiation projects were set up at many high energy physics laboratories around the world. The scientific and technical development of both the production of radiation and its applications continuously raised expectations, and at the end of the 70s, 'dedicated' synchrotron radiation sources were planned and built at some places. Important modifications of existing, 'parasitic' sources ones were also made to allow for broader utilization of the radiation. This development had the effect that an entirely new request was laid before accelerator physicists, this time not by high energy physicists but by representatives of the growing field of synchrotron radiation; and this time not asking for the minimization of energy loss in the synchrotrons and storage rings but rather the maximization and optimization of it (Munro 1996, p 134).

67. Lenses are never used, because ultraviolet and x-ray radiation would deposit too much energy in them and thereby destroy them (*Nyholm 4 interview*).

First, second and third generation sources

Synchrotron radiation facilities are normally identified as first, second and third 'generation'. This generational labeling is based on fundamental technological characteristics but is also extensively used without reference thereto, which indicates its rhetorical significance. The sources used in the 60s in so-called 'parasitic mode', i.e. synchrotrons or storage rings built for high energy physics and with partial utilization of the synchrotron radiation they produced are referred to as the *first generation*. Storage rings built for the sole purpose of producing radiation, beginning to emerge at a larger scale in the late 70s, are referred to as *second generation* synchrotron radiation sources (Shenoy 2003, p 3). They were commonly designed for bending magnet production of radiation, but as they were built with many straight sections, it later became possible to make use of insertion devices also in these rings. Storage rings with design optimized for wigglers and undulators, the first of which emerged in the 80s, are called *third generation* rings (Winick 1994b, p 7; Shenoy 2003, pp 3-4).

Insertion devices – *wigglers* and *undulators* – are arrays of magnets placed in the straight sections of the polygonal accelerators, that make the electron bunch turn several times back and forth (or up and down) and thus produce radiation more efficiently than bending magnets. When passing a bending magnet, the electron beam makes one oscillation, emitting light in a very broad planar angle (see figure 2). Even though several beam pipes can be attached to a bending magnet, no beamline can make use of the total amount of emitted radiation, on the other hand the wavelength range of the radiation emitted by a bending magnet is very broad, due to a fundamental law of physics (Margaritondo 2002, p 30). Wigglers function as a series of bending magnets that each makes the electron beam turn and thereby produce radiation in a broad plane, but the angular spread is narrower. Undulators have weaker magnetic fields than wigglers but are designed to make the electrons move in a 'spiral' trajectory, thereby causing the emission of radiation in a very narrow cone, which makes it more intense and also makes possible utilization of the whole emitted radiation beam. Another major advantage of undulators is that they make use of interference effects, i.e. the electrons and the radiation influence each other so that 'peaks' appear in the spectrum – the brilliance is vastly enhanced for tiny wavelength segments of the radiation (Schlueter 1994, pp 379-381). While the intensity of the radiation from wigglers is proportional to the number of periods (magnets), the intensity of undulator radiation is squarely proportional to the number of magnets⁶⁸

68. The wavelength of the radiation produced by insertion devices is amplified by the relativistic

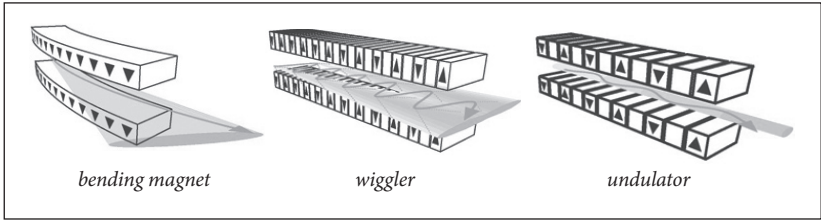


Figure 2: Sources of synchrotron radiation (Figure © Australian Synchrotron, reproduced with permission from the Australian Synchrotron).

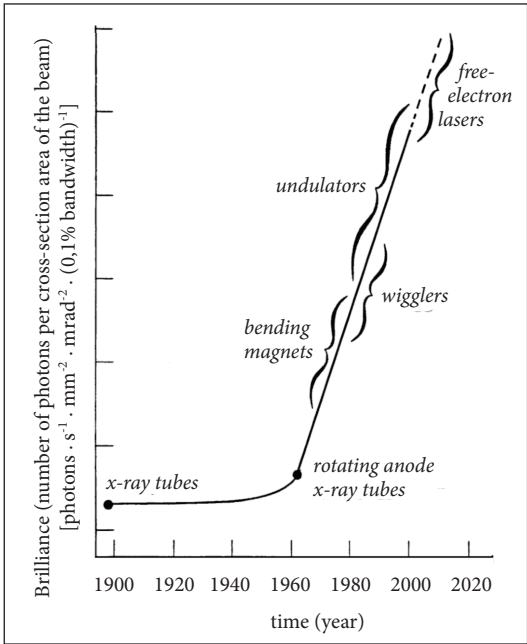


Figure 3: Peak brilliance of synchrotron radiation sources over time (Winick 1994b, p 6).

(Nyholm 4 interview). Together, these features significantly increase the ‘brilliance’ (see below) of the radiation from undulators over bending magnet or wiggler radiation (Munro 1996, p 141). In most cases undulators are the preferred sources of radiation, but because they tend to give sharp peaks of brilliance at certain wavelengths and not an even, continuous spectrum, bending magnets and wigglers are in some cases favored (Helliwell 1998, p 616).

effects, but it nevertheless stands in relation to the oscillation amplitude, i.e. the ‘size’ of the oscillation, which is determined by the size of (and distance between) the magnets, as each magnet causes one full oscillation (Margaritondo 2002, p 9).

Different experiments require radiation of different wavelength and some experiments make use of variable wavelengths. But bending magnets and the magnets in insertion devices are fixed, and the altering of the electron energy in the ring would affect every beamline in the laboratory. Thus choosing radiation of a particular wavelength or wavelength range of interest for a certain experiment is done separately on every beamline, with so-called monochromators,⁶⁹ which are put between the beam port where the radiation exits the ring and the beamline's experimental equipment. Monochromators do not change the wavelength of the radiation but only sorts out a particular spectral range.

Brilliance and polarization

Synchrotron radiation is superior to other light and radiation⁷⁰ primarily because of its extremely high 'brilliance' or 'brightness', i.e. that it contains 'more' light (or simply more photons). Other exclusive advantages are its polarization and its (partial) 'coherence'. Brilliance is measured as number of photons per cross-section area of the beam (Winick 1994a, p 461), and higher brilliance is beneficial for experiments in a number of ways, such as reducing the time required for experiments and improving the number of detectable events. For the past forty years the 'peak' brilliance (i.e. the highest brilliance achievable) of new synchrotron radiation sources has increased with by one order of magnitude every 24 months (!) (see figure 3) (Frahm and Williams 2007, p 3). Early improvements of brilliance were made by increasing current and energy (Barletta and Winick 2003, pp 1-3), but one of the most deciding factors for brilliance is the emittance of the electron beam, i.e. the cross-section size of the electron beam and concentration of electrons in it (Bilderback et al 2005, p 779), which is largely dependent on the design of the magnet lattice. Especially during the 90s and on, vast improvements were made in

69. 'Mono chrom' is Greek for 'single color', i.e. single wavelength. The two general monochromator designs that are in use are grating monochromators and crystal monochromators, and they are used in different wavelength regimes, as for very short wavelengths diffraction gratings are not practically feasible (their spacing would be too small), and crystals can generally only diffract radiation of x-ray range wavelength because longer wavelength radiation would only be absorbed by the crystal. There is some overlap between the two regimes covered by these general monochromator designs, and so the border between them is not sharp, although it is approximately at 1-3 keV (around 1 nanometer wavelength) (Winick 1994a, p 479, *Nyholm 3 interview*).

70. With lasers as exception, whose brilliance is superior to all other light sources. Conventional lasers do however not produce radiation in shorter wavelength than ultraviolet (Isaacs et al 2003/1984, p 441).

this area, and emittance of new storage rings could be drastically reduced (see below) (Barletta and Winick 2003, p 3).

The need for careful control of the electron beam – whose position and geometry largely determine the position and focus of the radiation produced – has increased continuously over time, as samples have become smaller and the level of detail and resolution has increased. Highly sophisticated beam position monitoring systems are implemented at most synchrotron radiation sources, both monitoring the position of the radiation beams at each beam-line, and the position and geometry of the electron beam in the accelerator, automatically sending feedback information to the beam control system (Munro 1996, p 139, *Nyholm 3 interview*).

The design of the magnet array in wigglers and undulators determine the polarization of the radiation, and this is sometimes used to achieve radiation with circular or elliptical polarization (Margaritondo 2002, p 39). ‘Coherence’ is the property of x-rays giving rise to ‘interference’ of visible light, namely that radiation passing through slits can ‘cancel out’ certain wavelengths and ‘show’ only some wavelengths (colors, for visible light) on the surface hit,⁷¹ and the coherence feature of synchrotron radiation has been an increasingly exploited for experiments (see below).

No matter how much emittance is reduced or how sophisticated magnet technology is implemented in the insertion devices, synchrotron radiation always propagate in a cone.⁷² Although this cone can be made very narrow, the beam needs to be focused, especially for experiments with very small samples. For this, various optics devices are used, such as mirrors of different types, constructed out of materials that can stand a lot of thermal load and can be grinded and polished to very high detail, typically crystalline silicon (*Nyholm 3 interview*).

The matured third generation

The steady improvement of the prospects of synchrotron radiation coincided with the development in high energy physics that led to the closing of acceler-

71. For the example of two slits, a particular wavelength is ‘shown’ if the difference in distance between the radiation source and the slits is an integer multiple of the wavelength. If not, the wavelength is ‘killed’. This causes a diffraction or interference pattern (which, in the case of visible light shows itself as a ‘rainbow’). A wave producing a detectable interference pattern is called a coherent wave, but radiation needs not be fully or entirely coherent to make this property useful (Margaritondo 2001, p 39, Munro 1996, p 143).

72. This is inescapable and due to fundamental laws of physics (*Nyholm 3 interview*).

ator programs in several countries. In a few European countries, for example France, the UK and Sweden, proposals were made to utilize the remnants of the dismantled high energy physics machines for synchrotron radiation programs, along with the assembled competencies in accelerator physics and development (Robinson 1981a, p 853). In the United States, the demand for beamtime at the existing laboratories rose continuously as results from experiments were published. This resulted in an assessment of the federal demand for synchrotron radiation facilities, done by a panel on mission from the National Science Foundation. It concluded that the projected demand would saturate capacity by 1985 (*Lynch Report 1983*, p 14). In Europe, the idea of a pan-European collaboration in synchrotron radiation had emerged, and a 'perspective study' on the prospects was done by the European Science Foundation in 1977 (see chapter 6). This study concluded that the demand for experimental facilities for synchrotron radiation in Europe would soon outgrow supply and that the existing high energy physics machines were not very well suited to serve as synchrotron radiation sources, in essence calling for a general effort in Europe to plan for dedicated sources and in particular a European collaboration to achieve a large, high performing source (*ESF Perspective Study 1977*, pp 9, 65-68). This 1977 document contains a comprehensive account on the science and technology of synchrotron radiation and argues strongly for the establishment of synchrotron radiation laboratories as independent research institutions.

The first synchrotron radiation source of the second generation, i.e. the first purpose-built facility, was opened in Tokyo, at the Institute for Nuclear Study, in 1974 (Winick 2007, p 4). The original second generation sources were built entirely without inclusion of insertion devices in their design (*Winick 1 interview*) but many were later modified to host wigglers and undulators in straight sections. A fundamental difference in accelerator design, namely the achievable electron energy (higher energy generally requires a larger ring), separated synchrotron radiation sources capable of delivering ultraviolet and soft x-ray radiation on one hand and hard x-ray radiation on the other (see figure 1 on page 78). With very few exceptions, the second generation sources were designed as so-called 'VUV (vacuum ultraviolet) machines', mostly because the highest demand for beamtime was in the ultraviolet and soft x-ray regime but to some extent also because the larger 'hard x-ray rings' were more expensive.

During the 80s, insertion device technology matured and the advantages of wiggler and undulator radiation were shown in practice. As a result, third generation sources, optimized for use of insertion devices and otherwise designed purposefully for light production, were designed and proposed. Especially emittance was significantly lowered – the third generation sources

built in the 90s provided over 10,000 times brighter radiation than the ordinary second generation sources (Shenoy 2003, p 4).

Third generation sources can generally be sorted into three categories. The 80s and 90s division of spectral range between VUV/soft x-rays and hard x-rays led to two distinct types with two distinct categories of experiments. One was the 1-2 GeV rings with approximately 100-200 m circumference, designed for the longer wavelength (e.g. the Advanced Light Source in Berkeley, MAX II in Lund, ELETTRA in Trieste), and the 6-8 GeV rings with circumference of 800-1500 m designed for hard x-rays (e.g. the 'big three'; the Advanced Photon Source at Argonne, ESRF in Grenoble, SPring-8 in Japan, see below) (Winick 1994b, p 7). The scientific and technical development enabled by the third generation sources led to further demands on stability, brilliance and focus, and in parallel, improvements in storage ring and insertion device technology was furthered. These developments started to erase the previously sharp border between 'VUV machines' and 'x-ray machines', and had the initial effect that the smaller third generation rings became capable of delivering radiation in the x-ray regime, and the big ones expanded their realm into the soft x-rays and ultraviolet. By the end of the 90s, accelerator and insertion device development had reached a point where a new type of third generation synchrotron radiation sources, the 'intermediate energy sources'⁷³ could be designed to cover the whole spectrum of radiation from visible to hard x-rays, making use of technological solutions that made them just as good or even better as their predecessors, but along the whole spectrum (Bilderback et al 2005, pp 781-782). The most significant design improvement of these sources was lattice designs that enabled another major decrease of emittance, thus enhancing brilliance even further, and the ability to achieve very tiny beams of radiation (Ibid., p 778). Several new sources of this type have been built and are at the planning stage around the world as a result of these improvements.⁷⁴ Recent developments in undulator technology has led the development further towards higher and higher brilliance and also increased coherence (Ellemaume and Ropert 2003, p 20-21).

Another way of achieving very low emittance is to make use of very large

73. These have an electron energy around 3 GeV, which is between the VUV/soft x-ray sources of about 1.5 GeV and the 'big three' whose energy is 6, 7 and 8 GeV respectively. The intermediate sources produce high brilliance radiation across the whole wavelength spectrum (Bilderback et al 2005, pp 782-783; *Nyholm 3 interview*).

74. For example SPEAR3, Stanford (opened 2003, see chapter 4), the Canadian Light Source, Saskachewan, Canada (2006), SOLEIL, Orsay, France (2007), DIAMOND, Daresbury, UK (2007), the Australian Synchrotron, Melbourne, Australia (2006), Alba, Barcelona, Spain (2009), the Shanghai Synchrotron Radiation Facility, China (2009), the National Synchrotron Light Source II, Brookhaven, USA (planned), MAX IV, Lund, Sweden (planned) (Bilderback et al 2005, pp 782-783).

accelerator rings, but for economic reasons this path has not been explored significantly for synchrotron radiation sources, until recently. The combination of increased demand and the closing down of experimental programs at some high energy physics machines has led to the exploration of possibilities to turn old accelerators into radiation sources by rebuilding segments of the rings entirely and otherwise making use of the existing vacuum tunnel and magnet lattice; thereby to a comparably low cost make available a potent source of radiation (e.g. PEP-X at Stanford and PETRAIII in Hamburg, see below and chapter 4) (Barletta and Winick 2003, p 3; Bilderback et al 2005, pp 787-789). Already in the 80s and 90s, 'fourth generation' radiation sources were discussed, mainly in the shape of so-called 'free electron lasers' (see below).

Utilities and experiments

A wide variety of scientific utilities of synchrotron radiation have been developed through the decades, creating a scientific environment at most synchrotron radiation facilities that is hard to classify and categorize in terms of traditional disciplines. This broad multidisciplinary nature of the labs has led to claims that there are no areas of science that has not benefited from synchrotron radiation (e.g. Munro 1996, p 146). Such claims are certainly more of advertising kind than accurate descriptions, though the increase in applications over the past decades has been exceptional.

Ever since the discovery of x-rays and their immediate utilization for various purposes, improvements have continuously been made with regard to apparatus and instrumentation used for producing, containing and utilizing them. The advent and development of synchrotron radiation sources can be interpreted as one recent and major step in this development. Increased brilliance, focus and coherence have vastly enhanced the performance of experiments, and the ability to tune wavelength over a broad spectrum has opened new possibilities. Therefore, the scientific development of synchrotron radiation applications can be interpreted as having had two parallel developments. The first one is the utilization of synchrotron radiation on 'traditional' x-ray and ultraviolet applications (such as spectroscopy, crystallography, microscopy and radiology) in order to increase the quality of the results from one or more aspects, shorten the time needed to do experiments and measurements, or expanding the application to samples and study objects of smaller or larger size or to be able to study them under new conditions. The second one is the broadening of the utilization of x-rays and ultraviolet radiation to completely

new fields of study by the modification of techniques and their adaptation to new problem areas. Especially the improvement in quality of the beams at third generation sources has enabled significant steps to be taken in both developments (Bilderback et al 2005, p 781).

Synchrotron radiation has furthered the aforementioned historical expansion of the realm of natural phenomena and events possible to 'see' and 'observe' – the utilization of technologies to gain information about phenomena and events that cannot be 'seen' in the ordinary sense. All experimental work with synchrotron radiation is about exposing or inducing certain things and events, mostly on atomic or molecular level but sometimes on larger scale, and record by different means the manifestations or traces of these things and events in order to create a picture or representation of them. Two generalized categories exist: on one hand the mapping of electronic structure of elements, i.e. the positions and properties of electrons, and on the other the study of geometrical structures of materials on atomic, molecular or compound level. Additional minor applications exist and will be briefly mentioned below, but the predominant part of synchrotron radiation science can in a broad sense be said to be occupied with elements and materials and either their electronic or geometrical structure (*Nyholm 3 interview*).

Spectroscopy and diffraction

Study of the electronic structure and the properties of electrons in elements and compounds in various forms and under various conditions can give a lot of information about the materials with regard to structure, strength, hardness, conductivity, and resistance to external influence such as pressure and temperature. A wide range of spectroscopic methods, making use of varieties of the basic spectroscopic principle described above, gives information of this type (*Nyholm 3 interview*). Spectroscopy has many varieties and diverse applications in science and the vast majority of experiments under the very broad umbrella of 'spectroscopy' are done with other means than synchrotron radiation, with 'conventional' x-ray sources. The wide range of wavelengths of synchrotron radiation, the ability to tune continuously over this range, and the brilliance of the radiation has enabled for faster and significantly more detailed studies, as well as studies of phenomena that are rare or comparably hard to detect. This has opened up completely new areas for spectroscopy and made synchrotron radiation laboratories a location for the most demanding spectroscopy experiments (Munro 1996, p 146; *Nordgren, Nyholm 3 interviews*).

A special spectroscopic technique that had importance for the early development of synchrotron radiation and especially its reputation in wider physics and chemistry communities is 'EXAFS', which reads 'Extended X-ray Absorption Fine Structure'. The brilliance of the x-rays produced by the storage ring at Stanford from 1972 and on was what made the EXAFS technique useful at all for experimental work (Winick and Bienenstock 1978, p 35). EXAFS provides information about the neighboring atoms of a certain element, i.e. what chemical compound it is embedded in, and samples can be studied as if they were in their natural form, not cleaned or manipulated to fit the laboratory setting (Munro 1996, p 148; Robinson 1975, p 1075). An application of EXAFS with high practical interest was the cleanup of former waste sites for United States weapons programs, where tanks containing millions of cubic meters of mixed chemical waste, some radioactive, were buried in the ground in the 50s and 60s. In order to decontaminate the areas where the containers were buried, it was not sufficient to know only what exact elements were in them, but information was needed about the exact chemical form they had evolved into over the years, to determine solubility in water and other properties with importance for decontamination. EXAFS analysis of samples from the sites provided such detailed information and is reported to have speeded up the process significantly (Clark et al 2006, pp 36-38; *Winick 3 interview*).

The study of geometrical structure of materials is largely done with x-ray diffraction, often called 'scattering' because the fundamental principle it relies on is that samples diffract and scatter x-rays, and the pattern of these scatterings are mapped and transformed into pictures of the geometrical structure of a sample (*HASYLAB brochure*, p 16). Nowadays, one of the most voluminous activities at synchrotron radiation laboratories worldwide is the structural determination of macromolecules, a utilization of synchrotron radiation that has gotten a boost from the increases in brilliance which has enabled very fast and very detailed structural determination of large biological molecules (Munro 1996, p 147). Several fields in the life sciences (for example immunology, neurobiology, cell biology, virology, physiology, molecular biology, medicine and biotechnology) have benefited from the expansion of the possibilities of detailed structural determination of macromolecules of several million Dalton⁷⁵ (Holmes 1998, p 618; *BioSync Report* 1997, p 5; *MAX IV brochure* 2004, p 21). But diffraction is regularly used also for the study of materials and objects of far larger size than molecules.

It has been argued that the most significant impact of synchrotron radia-

75. Dalton (Da) is another name for 'atomic mass unit', the unit for molecular weight, defined as $1/12$ of the weight of an atom of the isotope carbon 12 and approximately $1,6 \cdot 10^{-27}$ kg (Isaacs et al 2003/1984, p 59).

tion on any science has been in chemistry and the life sciences through x-ray crystallography (Wakatsuki and Earnest 2000, p 13; *Stöhr interview*), a debatable assertion but in terms of *measurable* impact probably true.⁷⁶ Especially the capability of tuning the radiation source over a range of wavelengths has had importance in crystallography (Hendrickson 1991, p 51), and it is clear from the development in crystallography that many of the crystal structures of macromolecules would never have been solved if it wouldn't have been for the high brilliance of synchrotron radiation, which has enabled an increase in detail and significant shortening of data collection time (Bilderback et al 2005, p 780; Prewitt et al 1987, pp 313, 317). Several other technical developments have however also contributed; computer programs, cryogenic⁷⁷ cooling, detectors, automation and robotics, not to mention increased and targeted user support (Wakatsuki and Earnest 2000, p 6-9). Diffraction is used in many other ways than determining structure of macromolecules, such as in various experiments in materials science (*HASYLAB brochure*, pp 18-19).

Diffraction studies with synchrotron radiation started with another application (the studies of frog muscles at DESY mentioned above), nowadays called 'imaging'. The extremely focused beam of synchrotron radiation makes it possible to study very small objects and the brilliance and coherence of the radiation enables a very high level of detail of the images (Kocsis and Snigirev 2004, pp 79-80; *Nyholm 3 interview*). As a non-destructive and efficient technique, imaging with synchrotron radiation has become a valuable tool in medicine, environmental studies, paleontology, archeology, and the history of arts, in essence every area where very small objects need to be studied with regard to structural, chemical and magnetic properties, or larger objects but with a very high level of detail (Detlefs 2008, p 36; Reiche and Sauerborn 2007, p 44).

Industrial use and new developments

During a brief period of time, an application of synchrotron radiation entirely separate from other techniques was exploited for industrial applications, so-called 'x-ray lithography', or 'x-ray pattern printing'. The x-rays are then used

76. The share of the protein structures submitted to the global 'Protein Data Bank' solved with synchrotron radiation grew from 17% in 1995 to 80,9% in 2008, with the total number of submitted structures growing from 971 to 6333 in the same period (*Protein Data Bank website*).

77. Cryogenic cooling is the cooling with liquefied gas, i.e. extremely low temperature gas. (Isaacs et al 2003/1984, p 200).

to expose a surface to draw something or make an imprint, for example for circuit manufacturing in the electronics industry (Munro 1996, p 152-153; Smith 1996, pp 158, 161-163). In the late 80s, speculation had it that specialized synchrotron radiation facilities for such chip production would be built all over the world and fulfill as much as half of the industry's need (Goodwin 1988, p 52), but the technical delicacy of accelerator systems and the overall operation (in)stability of synchrotron radiation laboratories prevented the technique from reaching production lines (Smith 1996, p 164; *Lindau 1, Nyholm 1 interviews*). For similar reasons – despite the general superiority of synchrotron radiation over conventional x-rays – medical applications have not been exploited to an extent beyond testing and exploring of possibilities (Rubenstein 1988, pp 27-29; Lewis 1997, pp 1213, 1225-1227).

Increased detail in the control of the electron beam in the accelerator, so-called 'filling modes', affecting the pulse structure of the radiation beams (Revol and Hardy 2006, pp 22-24; Shenoy 2003, p 10), has enabled new types of spectroscopy, crystallography and imaging experiments with which the phenomena of interest can be studied also with regard to how they change over time (Bilderback et al 2005, pp 780-781; Galburt and Stoddard 2001, p 33).

Synchrotron radiation laboratories are frequently referred to as 'materials science labs', and although this is but one of many scientific areas in the lab, it very well represents the diversity of synchrotron radiation. 'Materials science' in a broad sense makes use of several synchrotron radiation techniques (spectroscopy, imaging, diffraction and a lot of varieties within and between them) for a variety of purposes such as studies of electronic materials, polymers, semi- and superconductors, and magnetic materials (Bilderback et al 2005, p 780; Munro 1996, p 149). The development of nanotechnology has benefited a lot from the breadth and quality of these experimental techniques, because of their detail and capacity to handle complex samples (Detlefs 2008, p 36). Not least have the prospects of combined studies, made possible by the variety of instruments operated simultaneously at synchrotron radiation laboratories, been vastly increased in recent years (Sinha 2004, p 633). Parallel observations of several features of complex systems are possible, such as determining geometric or electronic structures simultaneously with the study of magnetism, conductivity or time-resolved features (Paszkowicz et al 2005, p 1).

But synchrotron radiation laboratories are also often called 'biology labs'. This is probably due to the recent growth in sheer numbers of biology and life sciences applications and users at synchrotron radiation laboratories worldwide, as well as the high visibility and direct impact of the results. The life

sciences have traditionally been comparably reluctant to enter the realm of synchrotron radiation, despite its promises, but far-reaching automation and streamlining of beamlines, experimental equipment and routines for user support has made synchrotron radiation more attractive for the life sciences in the past decade. Life sciences users continue to grow in number, parallel with technical and organizational adjustments to the needs of these user communities (*BioSync Report 2002*, p 6-7). Life sciences work at beamlines is often rather called 'measurements' than 'experiments', indicating that it is more of routine character than other synchrotron radiation laboratory work (see chapter 7 for further discussion). The 'real' laboratory work in the life sciences is often done months ahead of the visit to a synchrotron radiation laboratory, such as the growing of samples, and work at the beamline is then a matter of simple data collection. Despite the turnkey operation character, such routine measurements do have great impact and significance in their respective fields (Wakatsuki and Earnest 2000, p 9; *Hodgson, Knotts, Larsen, Mason 1, Ursby interviews*).

Macromolecular crystallography has been one of the areas where industrial research and development has entered synchrotron radiation labs. Especially pharmaceutical companies have been successful in utilizing crystallography in early stages of drug development, for the study of interactions between inhibitors and target macromolecules, and at some synchrotron radiation laboratories around the world investments and direct involvement from pharmaceutical industry has propelled the development towards more automation and streamlining of measurement equipment and user interfaces (e.g. *APS Science 2007*, pp 99, 144; *MAX IV brochure 2004*, p 29). As an example of the general level of industrial involvement at synchrotron radiation laboratories can be mentioned the estimation that 30% of all experiments at the European Synchrotron Radiation Facility (ESRF) in Grenoble have direct links with industry, and that approximately 20% of all beamtime at SPring-8 in Japan is used for industrial research (Rodriguez Castellano 2005, p 5; Watanabe 2008, p 40; *ESRF Purple Book 2007*, p 9).

A recent application of synchrotron radiation that has been widely acknowledged for its spectacular merging of advanced x-ray technology with classical research in the humanities is the use of x-ray fluorescence spectroscopy to unveil the content of old Archimedes manuscripts never possible to read before because the original writings been painted over in the thirteenth century. Fluorescence spectroscopy induces energy loss of electrons in compounds, different elements corresponding to x-rays of different wavelength, which makes the sample 'glow'. The technique is used for detection of small trace amounts of metal in various substances, and when it was discovered

that the ink used by Archimedes contained a higher degree of iron than the paint that covered his writings, the possibility arose to use synchrotron radiation x-rays to unveil it. By tuning the x-ray beam to iron and recording the fluorescence of the iron atoms in the scribbles behind the gold paint, the manuscripts could be ‘read’⁷⁸ (*Bergmann 1 interview*).

Both the opening of completely new and unexpected areas like the reading of the Archimedes palimpsest and the continuation of long traditions in x-ray related sciences are due to the mostly incremental but occasionally discontinuous improvements that have been made in every part of the complex technical systems that make up synchrotron radiation laboratories. Increases in brilliance due to reduced emittance or developments in optics leading to better focus are the improvements usually brought up, but also detector technology, sample handling, computer systems for data analysis, and routines for user support are crucial features of synchrotron radiation laboratories that are continuously improved.

Continuous optimization

The third generation synchrotron radiation sources, optimized for the use of insertion devices, were purposefully planned and built as synchrotron radiation laboratories, technically and organizationally. Second generation sources were planned and built with a focus to fill an urgent need and probing new territory, and they often delivered below acceptable standards in terms of operations stability and user support (Robinson 1983a, p 313). The third generation sources clearly aimed to push the limits of operations stability, quality of the radiation, user support, and overall performance of experimental equipment. The ‘big three’ – APS, ESRF and SPring-8 – were the real ordeal in the sense that the investments were on par with the most expensive research infrastructure across the board and that the ambitions were set very high (Winick 1994b, p 7). These large scale projects also established synchrotron radiation as “big science in its own right” (Balter 1991, p 794), simultaneously in Europe, Japan and the United States. State of the art accelerator systems were constructed and used solely for synchrotron radiation, and accelera-

78. For a comprehensive account on this work, the whole story of the Archimedes palimpsest in question, and the work to unveil its writings, see Netz and Noel (2007, esp pp 273-280). Other similarly remarkable studies have been done with fluorescence spectroscopy on synchrotron radiation laboratories, for example so-called pigment analysis, which has been used to determine age and origin of ancient paintings (e.g. Calza et al 2008).

tor R&D groups were formed to work on their refinement and development. Instrumentation groups, beamline designers and constructors, vacuum and optics scientists, technical support groups, computer systems developers, and user administrators were employed on large scale for the ultimate purpose of fulfilling the needs of a user community. Early synchrotron radiation scientists and technicians who moved to these newly established third generation sources experienced a difference “between the life in a village and the life in a city” (*Comin interview*) and the sudden arrival of a much more industrial mode of work, where every task was optimized to fulfill a specific demand and the organization had an overarching goal not defined ad hoc by the scientists on the floor but by demands placed on the laboratory from financiers and other stakeholders and by the collective will of the user community (*Brennan, Winick 2 interviews*).

The development and construction of accelerators, beamlines, experimental stations and auxiliary equipment is a balance act between innovativeness and ‘playing it safe’ – the result has to be competitive and pioneering but it also needs to work according to standards and have stable operation (*Nyholm 3 interview*). The process for new concepts to emerge and become implemented in practice is a lengthy process, but it is mitigated by the existence of a critical mass of technologies and manpower in the fields. Whereas first and second (and to some extent third) generation synchrotron radiation sources had most – if not all – of its technical components uniquely designed and built for their specific purposes, nowadays many devices are commercially available, and the pushing of technical limits can be better concentrated on specific areas (*Comin, Feldhaus, Glatzel, Nyholm 3 interviews*).

Apart from the growth in number of scientific disciplines and techniques in the laboratory, the most significant development of the past 15-20 years has been the improvement of the character of the radiation due mostly to developments in storage ring technology. While magnet lattice developments has lowered emittance significantly and thus vastly improved brilliance and coherence, insertion device developments have increased capabilities to produce high quality radiation in a larger spectrum of wavelengths (Elleau and Ropert 2003, pp 20-21; Bilderback et al 2005, p 782). Among other important technical improvements, such as feedback systems for the electron beam and continuous ‘top up’ injection (Barletta and Winick 2003, p 3), this development has allowed smaller rings to deliver hard x-rays and has made possible the aforementioned ‘intermediate energy’ synchrotron radiation laboratories that produce high brilliance radiation across the whole spectrum (from VUV to hard x-rays).

Next generations

Developments in storage ring technology is said to be reaching its limits, and laboratories are increasingly turning to improvement and broadening of their scientific programs (*Comin, Glatzel, Lindau 1, Szilagyi interviews*). Nonetheless, storage rings are “likely to remain the workhorses of synchrotron radiation science for many years to come” (Barletta and Winick 2003, p 8), because of their breadth and because their mature design can provide stable operation and largely uninterrupted delivery of experimental time (Kim 2006, p 6).

The overall demand for beamtime seems still to be growing, and the completion of the ‘intermediate energy’ sources at several places, as well as continuing operation of older sources for experiments with somewhat more modest demands will likely be able to facilitate this demand (*Feldhaus, Stirling interviews*). At the same time, entirely new accelerator designs have emerged that aim at taking the development in peak performance further. At least two general concepts have competed for some time for becoming canonized as the ‘fourth generation’ light sources, both significantly more specialized than storage ring sources and projected to open up completely new experimental possibilities.

The ‘free electron laser’ (commonly abbreviated ‘FEL’) emerged as a technical concept in the 70s (e.g. Winick and Bienenstock 1978, p 57; Pellegrini 1980, p 717), but did not become technically feasible until the 90s. Several free electron lasers are now in operation, under construction or in the design phase over the world (Pellegrini and Stöhr 2003, p 35). Free electron lasers consist of linear accelerators with very long undulators and their ground-breaking feature is that the produced radiation interacts with the electron beam through a ‘self-amplifying’ process, thereby producing coherent,⁷⁹ high intensity radiation in extremely short pulses (Feldhaus et al 2005, p 800). The process is similar to traditional, optical lasers, but with the difference that free electron lasers exploit unbound, ‘free’ electrons, just as the ones in a storage ring, whereas traditional lasers generate light from excited atoms (Plummer 2008, p 15). The design parameters of free electron lasers reveal a discontinuous improvement of the radiation character, reportedly constituting “as big a jump in peak brightness [brilliance] above storage rings as storage rings were above laboratory X-ray machines” (Ibid.). Most analysts however agree that free electron lasers will not replace storage rings but rather attract different groups of users (Barletta and Winick 2003, p 8; *Arthur 1, Feldhaus, Galayda,*

79. In contrast to the ‘coherent’ radiation of modern storage rings, the radiation from free electron lasers is coherent in the absolute sense, i.e. all waves in the beam are in phase with each other (*Galayda, Nyholm 2 interviews*).

Lindau 2, Milton interviews). The extreme performance parameters with regard to brilliance, coherence, and time structure has brought about envisioning of spectacular scientific utilities, such as taking 'motion pictures' of chemical processes on atomic scale (Pellegrini and Stöhr 2003, p 33). Speculations also hold that the new nature of the x-rays produced and the sophisticated instruments will require larger teams and a furthering of the expert division of labor within the teams,⁸⁰ as well as efforts to develop entirely new and specialized optics and detector systems (Gehrke and Feldhaus 2006, pp 20-21; *Galayda, Lindau 2 interviews*).

The second contender for the epithet 'fourth generation light source' is the so-called 'energy recovery linac' (ERL) concept; an old idea that has only recently become practically feasible by advances in accelerator technology and that is studied by several labs (Gruner and Bilderback 2003, pp 25-28; Kim 2006, p 6). While enhancing the performance parameters greatly, energy recovery linacs are at the same time reportedly compatible with existing instrumentation (Bilderback et al 2005, p 794).

Other 'next generation' concepts exist as well. The refurbishment and partial rebuilding of old, large storage rings originally built for high energy physics⁸¹ will result in synchrotron radiation sources that are conceptually discontinuous, primarily because of their size and their extremely low emittance.⁸² Also design concepts for so-called 'ultimate' storage rings that would compete in brilliance with both free electron lasers and energy recovery linacs are sometimes drafted (e.g. Elleaume and Ropert 2003), as are prospects of so-called 'table-top' sources, not providing very high brilliance, but with low cost and compact size (Kim 2006, p 3).

80. This has already been shown on the free electron laser FLASH (Free Electron Laser Hamburg) at DESY in Hamburg, that opened in 2005 and that serves partly as prototype for the future European XFEL, where experimental groups are bigger on average than the groups at synchrotron radiation laboratories (Gehrke and Feldhaus 2006, pp 20-21). The XFEL ('X-ray Free Electron Laser') is an international (European) project, proposing to build a 3.4 km long free electron laser facility in Hamburg (Tscheentscher et al 2006, pp 15-16).

81. For example PETRAIII at DESY in Hamburg, which is the rebuilding of 1/8 of the old 2.3 km circumference PETRA (Positron Electron Tandem Ring Accelerator) ring, taken into operation for high energy physics experiments in 1976 (Bilderback et al 2005, p 787; Franz et al 2006, p 25). A similar proposal, to rebuild the Stanford PEP ring that was recently taken out of high energy physics operation, is currently under preparation at Stanford.

82. Large rings naturally have very low emittance, and when it opens in 2009, PETRAIII is expected to be the lowest emittance storage-ring available (Franz et al 2006, p 28), with "a factor 3-4 advantage compared to the ESRF, APS and SPring-8" (Bilderback et al 2005, p 791).

Organizing a user laboratory⁸³

Organizational structures differ slightly among synchrotron radiation laboratories, in management philosophy and the laboratory directors' and staff's conception about their mission and function in the user community and in science. In some fundamental aspects, though, all synchrotron radiation laboratories operate according to roughly the same pattern. They are all accelerator laboratories with radiation as their 'product', they are all user oriented, they have a day-to-day or week-to-week turnover of users, they need to accommodate different needs and requests of these users, and they do all to some extent seek to compete for the 'best' users and want to be able to share the credit for scientific achievements made at their laboratory.

The storage ring is built to keep a bunch of electrons moving around at high speed on a very controlled path, producing radiation that is led out of the ring to the experimental stations. It requires a machine or accelerator group, responsible for the operation, maintenance and improvement of the accelerator system. Accelerators are complex pieces of technology requiring a lot of attention and service on a constant basis to work properly, and the accelerator group does therefore usually consist of both technicians and accelerator physicists, to be able to both solve urgent technical problems and make continuous improvements on longer term. The same is basically true for all other technical components of the laboratory. Insertion devices, monochromators, optics, vacuum chambers, detectors, and computer systems all require the attention of technicians, focused on the day-to-day operations, and scientists, focused on academic research in the respective areas.

Receiving and accommodating users and ensuring that their beamtime is utilized in the most effective way is something that requires both scientific and administrative competences. Most laboratories have a 'call for proposals' once or twice a year, with a deadline at which beamtime applications are supposed to be sent in. An appointed expert review panel, or in rare cases specially chosen external reviewers, make assessments of all the proposals and grade them with respect to scientific merit and technical feasibility. When the assessment and grading is done, the user administration makes the detailed scheduling of the beamtime together with beamline staff, based on technical

83. This section is entirely based on the collected information obtained by observation and interviews with directors, beamline scientists, and administrative personnel at the three case study laboratories; especially *Cerenius, Elleaume, M Eriksson, Glatzel, Hedman, B Johansson, Knotts, Larsen, Mason, Mårtensson, Nordgren, Nyholm, Pianetta, Poloni, Rodriguez Castellano, Stirling, Stöhr and Ursby interviews*. Exact references are therefore made only when exact facts or figures are presented or in connection with direct quotes.

specifics and to some extent the requests of the user groups. Beamlines differ in popularity; some are heavily oversubscribed while some operate below capacity. Lower-rated proposals can be granted time on a beamline of their second choice if such alternatives exist, or be granted less time than applied for. User groups are put in contact with a beamline scientist ahead of their beamtime to be able to make some planning of the experimental run and to discuss technical issues. A user administration office of some sort is usually in place to coordinate all these issues.

A few exceptions from the ordinary user-laboratory relationship exist, exceptions that can render some alterations in organizational structure. At some laboratories, external user groups have been given the full responsibility for beamlines, from design and construction to user support and in part also review of beamtime applications, through so-called 'participating research team' (PRT)⁸⁴ contracts. Usually, this means that the contractor group makes both financial investments in the beamline and its instrumentation and effort investment in design and construction of the equipment. In return, they get a share of the total time on the beamline, usually between 50% and 80%, on which they are free to carry out their own research, invite collaborating groups, or award time to external researchers by their own choice. The conditions are usually that they take responsibility for the maintenance of the beamline and the user support on the share of the time that is subject to the laboratory's ordinary beamtime allocation process, and that the science done on their designated time match the standard of the rest of the laboratory. The scientific performance of PRT beamlines is usually evaluated by the laboratory on a regular basis.

The other exception from the ordinary beamtime allocation process is the case of proprietary research. Beamtime can be purchased if the applicant wants to escape the customary obligation to openly publish the results obtained on the beamtime, if they don't want to disclose the content of their study to the proposal review panel, or if the study in question has too low overall scientific value to be competitive in the peer review process. Most laboratories have some kind of regulation for the maximum amount of time that can be taken away from the open competition and sold, but the share of industrial research is still so low at most synchrotron radiation laboratories that these restrictions need not be applied in practice. At some laboratories, industry has invested directly in instruments and thereby engaged in a kind of combined PRT and proprietary research arrangement.

84. These are have different names at different labs: 'principal research teams' (PRT), 'collaborative access teams' (CAT) and 'collaborating research groups' (CRG) are common names.

The scientific and technical personnel at the beamlines are the prime mediators between users' scientific ambitions and the scientific and technical capabilities of the beamlines and experimental stations. Beamline scientists are specialists on the instrumentation on their beamline and have broad insight and knowledge in the scientific area(s) of the users they serve. They have the opportunity (and are sometimes required) to do their own research on the beamline and though they are usually given some beamtime external to the ordinary beamtime allocation process, they are also encouraged to apply for time.

The degree to which beamline scientists need to be involved in experiments depends on a number of factors, such as the knowledge and experience of the users and the degree to which instruments are automated or modifications of the equipment is made for the specific experiment. At some laboratories, crystallography beamlines have been automated to the degree that users need only to mount a cassette with crystals in a robot, which takes care of everything, and so they don't even have to be present in the lab but can monitor the measurements in a web-based interface. On the opposite end of the spectrum, experiments in for example spectroscopy can involve several modifications of the equipment and the samples; modifications that have to be done instantaneously and with some improvisation. The work of the beamline scientist as a mediator between the users' requests and the capabilities of the instruments is crucial, and often the beamline scientist is a collaborator in projects and eventually shows up as co-author of resulting publications.

Users have different ways of influencing laboratory activity and development; one natural channel is involvement in instrument development. Most laboratories also have a users' organization, and in some cases this organization has representatives in the laboratory board. The users' organization often co-organize an annual users' meeting together with the lab, where workshops and discussion sessions about users' requests are held and the user community is briefed about the development of the lab.

And of course, there are directors and administrators. Most labs have a board or a council with the highest governing authority given to them by the financers. Every lab has a director; most labs have deputies with specific areas of responsibility such as oversight over the collected scientific activity and the scientific development of the lab. At larger laboratories, beamline groups organized with respect to basic technology or science or both adds one more level in the hierarchy of the organization chart. The user administration is usually organized as a separate organizational entity but in rare cases its activities are distributed among scientific coordinators and beamline scientists. The role of scientific directors or coordinators extends into the planning and

execution of instrumentation upgrades and new installments. The laboratory director has an overarching operations responsibility and otherwise traditional 'CEO' management and administrative duties.

Important organizational entities common to most synchrotron radiation laboratories are the semi-internal, semi-external committees with consulting roles. The 'proposal review panel' or 'program advisory committee' (those are the two most common names) engaged to review and grade beamtime applications is often also used as reference panels for upgrades of instruments and beamlines, as they are composed of experts of the concerned sciences and of synchrotron radiation technology. In some cases, this role is given to the so-called science advisory committee (SAC), that all synchrotron radiation laboratories have, and who mostly do overall reviews of the collective scientific program on a regular basis. This committee can also be brought in as evaluators of major upgrades or changes in the laboratories' scientific program, and to evaluate PRT arrangements. It is often composed of internationally leading experts (frequently directors or board members of other labs) and so the committee's judgment usually has a strong credibility and can be put forward when laboratories make their requests towards funding agencies. It is common to have a SAC or the like also during the construction and commissioning of laboratories to monitor the overall process and act as external consultants, complemented by a 'technical advisory committee' (TAC) or 'machine advisory committee' (MAC) to assist as consultants in the accelerator construction process. Because members of advisory committees are usually directors, accelerator constructors, scientific coordinators and prominent synchrotron radiation users, these committees often function as forums where experience is mutually shared between experts in the accelerator and synchrotron radiation fields, where important benchmarking is done, and the latest developments are discussed among peers.

Measuring of performance and quality is as common as it is difficult. The laboratories with highest percentage of 'delivered time' or 'uptime' – i.e. uninterrupted periods of normal operation – often put forward their high figures in this area; the ESRF in Grenoble is reportedly reaching 98% (*Elleaume interview*). Since this is to some extent an area of strategic choice – laboratories may choose to use their resources for other improvements – uptime is not considered a uniform quality measure among all synchrotron radiation laboratories. A commonly used measure of quality is a classic counting of publications and more specifically the counting of publications in prestigious journals such as *Nature*, *Science*, *Physical Review Letters*, or *Cell*, but the differences in sheer size and scope between synchrotron radiation laboratories limits the solidity of such comparison. No commonly agreed quality or performance measure exists among the laboratories.

Conclusions

“The most straightforward and most important conclusion of this study is that over the past 20 years in the United States synchrotron radiation research has evolved from an esoteric endeavor practiced by a small number of scientists primarily from the fields of solid state physics and surface science to a mainstream activity which provides essential information in the materials and chemical sciences, the life sciences, molecular environmental science, the geosciences, nascent technology and defense-related research among other fields” (*Birgeneau-Shen Report 1997*, p 7).

“To do this they had to learn a new, to them at least, mode of working: they had to leave their laboratories and travel to the few sources then in existence where their presence was tolerated. They had to learn to adapt their work habits to the requirements of the proprietors of the facilities where the machines were maintained for quite different purposes” (Rowe 1978, p 332).

Although the potential of synchrotron radiation was theoretically known for a long time and its advantages were demonstrated in practice already in the 60s and 70s, it took a long time until a broader user community was established and representatives from all the disciplines with potential interest in synchrotron radiation started to pay attention to it. This was largely due to technology – several technical developments were necessary to make prospective experiments and measurements feasible and practically attainable. But also social and socio-technical adjustments had to be done; scientists had to adjust to working with instrumental setups and in laboratory environments completely new to them.

Regardless of the promised performance of the x-ray source, many researchers in the early days did not regard the prospects of becoming a synchrotron radiation user an attractive way of pursuing their career. They had to travel to a remote site, enter a building with occasional radiation hazard warning signs, rely in part on the goodwill of particle physicists and accelerator physicists for the success of their experiments, and they could not be sure whether there would in fact be any data at all to handle after the experimental run (*Feldhaus, Galayda, Hodgson interviews*). In chemistry and the life sciences, where most experimental work is very remote to large scale apparatus and is done at a laboratory bench in a room next to the office, this was especially true, but also in most other natural sciences. Accelerator based science was at this time largely viewed as the realm of big physics and thus viewed with some prejudice and aversion by many other scientists who had seen the

prospects of increased funding and support for their own activities plunge repeatedly as one high energy physics accelerator project after the other passed federal appropriation in the 50s, 60s and 70s (see chapter 2).

From this chapter, it can be concluded that the shift from 'esoteric endeavor' to 'mainstream activity' was largely a shift in *user base* and in the relationships between the users and the laboratories. It happened gradually; in reciprocity with and on basis of technological development. Synchrotron radiation slowly spread itself over the world and through the veins of the natural sciences, fertilizing scientific disciplines and subfields while at the same time developing itself from an unreliable venture to a streamlined, user-oriented asset with broad support in the sciences. By this development, synchrotron radiation acted as a force of collectivization and sophistication for the disciplines it closed the gap to and that became beneficiaries of synchrotron radiation through the process.

Because of the superiority, at least theoretically, that synchrotron radiation offered over home laboratory sources (x-ray tubes, but also other instrumentation for analysis than radiation), and because of the instability and 'unfriendliness' experienced by pioneering users, the shifts for different disciplines can be identified as the moment in time when performance started to outweigh nuisances. A clear lack of trust in the operation stability (home sources were "much more stable", *Feldhaus interview*) and the effort it took to conduct experiments ("it was doable, but it was doable by force", *Hodgson interview*) not only scared off potential users but hindered word to be spread about the advantages that showed themselves when experiments actually succeeded. Although experiments yielded remarkable results in the mid 70s,⁸⁵ it took several more years until the laboratories and instruments were adapted to accommodating regular users on broader basis. The reason – which will be returned to in later chapters – is likely that the threshold for adopting entirely new experimental techniques or adapting to entirely new laboratory settings is not only different for different disciplines but also varies within the disciplines. Forerunners – *scientific entrepreneurs* – likely exist in all fields. One interesting task that lies ahead in this thesis is to identify and classify them.

85. As mentioned earlier, the first EXAFS experiments at Stanford showed a hundred thousand times better performance than had previously been achieved. In diffraction, a group at Stanford did crystallography measurements published in an article in the Proceedings of the National Academy of Sciences of USA in January 1976, and the comparison offered between the results with "the same crystal and instrument parameters" done with an x-ray tube and at the synchrotron radiation beamline was "a factor of at least 60 greater" intensity, and the article concludes that synchrotron radiation offers "unique advantages ... in x-ray diffraction studies of protein crystals" (Phillips et al 1976, p 128).

Despite being purpose-built, second generation sources retained much insufficiency that seems to have prevented larger steps towards 'mainstream' to be taken. With the third generation sources of the late 80s and early 90s, however, a larger set of obstacles seems to have been overcome and the user focus sharpened. The political status of synchrotron radiation was heightened, and with planning and design of the large, third generation sources that became the APS, ESRF and SPring-8 in the mid 90s, synchrotron radiation became incorporated in the research strategies of many major countries (Robinson 1983b, p 826). The establishing of these 'big three' and the expansion of the user community they brought was of major importance in the development and improvements from a user perspective. The reliability of the sources and instrumentation was significantly improved. The user community grew to a critical size and started to act as a driving force in the development towards new applications and better performance, a process enhanced by improvement of communications between the user communities and the facilities. A critical mass was built up, locally and globally, enabling more efficient use of resources and benchmarking (*Feldhaus interview*). Synchrotron radiation achieving this kind of critical mass and becoming "big science in its own right" (Balter 1991, p 794, op. cit.) is one apparent feature of turning into a 'mainstream activity', also with particular political importance as it suggests that the laboratories then have become research infrastructure assets with their own political significance.

Synchrotron radiation seems to have reached a status as generic experimental technique in most – if not all – of its scientific and technological aspects. The range of applications has been constantly expanded during the years and the number of users has grown rapidly, especially in the last decade. Perhaps most important of all is the erasing of technological, social and organizational barriers that previously made the prospects of utilizing synchrotron radiation for experiments and measurements perceivably unrealistic or inconceivable. This is among the most important conclusions of this chapter: becoming 'mainstream activity' is not only a question of technology but to a large extent also sociology. In the following chapters, the case studies are used to provide detailed and case-specific insights to this technological and sociological development and to relate it to politics.

Synchrotron radiation at Stanford – from parasitic to symbiotic and back

The setting

SLAC National Accelerator Laboratory⁸⁶ is located a few kilometers west of the main Stanford University Campus, in Silicon Valley, south of San Francisco, California. It is a dual-mission US National Laboratory for particle physics/particle astrophysics and so-called ‘photon science’. SLAC operates a synchrotron radiation user facility, the Stanford Synchrotron Radiation Lightsource⁸⁷ (SSRL), and a free electron laser (the Linac Coherent Light Source) scheduled to open for users in 2009.

SLAC was founded in 1962 as a single mission research center⁸⁸ for high energy physics and has operated a number of high energy physics accelerators during its decades of existence. It is run by Stanford University under the supervision of the US Department of Energy (DOE) through its Office of Science. The synchrotron radiation program at SLAC began its activities as ‘parasites’ on the high energy physics in 1972, when a beamline for synchrotron radiation was attached to the accelerator SPEAR,⁸⁹ and has since

86. Before, SLAC was an abbreviation for Stanford Linear Accelerator Center. It is commonly pronounced ‘slack’ and the laboratory is widely known under that name. Therefore, when the laboratory was renamed in 2008, ‘SLAC’ was kept as part of the name and is no longer an acronym (Cho 2008b, p 515).

87. The SSRL started out as the Stanford Synchrotron Radiation Project (SSRP) in 1972 and was renamed the Stanford Synchrotron Radiation Laboratory (SSRL) in 1976 (see below). In 2008, the name was changed to the Stanford Synchrotron Radiation *Lightsource*, with the abbreviation SSRL unchanged.

88. To avoid confusion, the following distinction needs to be made: In this chapter and throughout the thesis, ‘single mission’ refers to official lab activities as stipulated by political authority, whereas ‘single-purpose’ or ‘multi-purpose’ refers to the actual function of laboratories and their infrastructure. For example, SLAC was formerly a ‘single mission’ lab because its activities were clearly stated by the steward agency to be high energy physics (although with supporting activities such as an accelerator physics program). It is nowadays a ‘dual-mission’ laboratory because it officially pursues particle physics / particle astrophysics *and* ‘photon science’. It can however rightfully be called a ‘multi-purpose’ laboratory, because of the wide variety of scientific work it supports by its user facilities and pursues by its inhouse programs (see below).

89. SPEAR reads ‘Stanford Positron–Electron Asymmetric Ring’. In its first proposal, SPEAR consisted of two separate ‘pear-shaped’ rings and two interaction points. For economic reasons it

<p><i>SLAC National Accelerator Laboratory</i>, founded 1962.</p> <p>Approximate number of staff: 1,500</p> <p>Approximate number of external users (annual number), visiting scientists and graduate students: 3,000</p> <p>Funded by the US Department of Energy's Office of Science.</p>	
<p><i>Stanford Synchrotron Radiation Lightsource</i>, founded 1972.</p> <p>Approximate number of staff (including visiting scientists): 200</p> <p>Approximate annual number of external users: 1,000</p> <p>Accelerator: SPEAR3, a 3 GeV, third generation storage ring</p> <p>Approximate number of operational beamlines: 23</p> <p>Funded by the US Department of Energy's Office of Science and the National Institute of Health.</p>	<p>Table 1: SLAC and SSRL basic facts (<i>SSRL Strategic Plan 2007</i>).</p>

then expanded successively. Since 1991 the SPEAR accelerator is dedicated to synchrotron radiation, and SLAC has been turned into a dual-mission National Lab. As a division of SLAC, the SSRL is a national user laboratory funded by the DOE, built on the strong accelerator tradition of SLAC and the scientific strength of Stanford University in several synchrotron radiation related fields, and a well-established user community of both industrial and academic users. The goal of the laboratory is not primarily formulated as striving towards being the most user-oriented synchrotron radiation lab or the most productive in terms of publications per beamline, but rather to be a vital part of a broader strategy at SLAC and Stanford:

“It is the goal of SLAC and Stanford University to become the world’s leader in the new multidisciplinary field of Photon Science, the study of matter through its interaction with photons” (*SSRL Strategic Plan 2007*, p 1).

The transformation of SLAC from its original single mission as a National Laboratory towards such an articulated ambition is remarkable, and part of the rationale for choosing it as a case in this thesis. In the original 1957 proposal, SLAC was nothing but a very long, very high-performing linear accelerator (‘linac’) for high energy physics experiments, with a surrounding organization to run it. The original SLAC proposal was submitted to the government in the late 50s high energy physics heyday⁹⁰ and described a single-

was turned into two symmetric rings in the same accelerator tunnel, but the name was kept (Panofsky 2007, p 120).

90. As described in chapter 2, the late 50s and early 60s was a spectacular time for high energy physics, with machines of bigger and bigger size built everywhere. Only in the United States, by

purpose lab with a single huge (a 3 km linac) machine, initiated and developed by the German-American physicist Wolfgang 'Pief' Panofsky together with Stanford colleagues (Sessler and Wilson 2007, p 38). The linac tradition at Stanford was already strong, partly originating in the 1937 inhouse invention of the 'klystron' (see chapter 2), and a series of linear accelerators (with their length measured in meters rather than kilometers) built in the 40s and 50s (*SLAC Story 1966*, pp 39, 46-48). In 1953 high energy physics got its own department at Stanford University, with Panofsky as director (Galison et al 1992, p 63) and funding for accelerators from the Atomic Energy Commission (AEC). In times of seemingly unlimited governmental support for accelerator projects, Stanford high energy physicists could not resist making bold plans for the next step, the 'Project M' (where 'M' stood for 'Monster'). The proposal was submitted to the AEC on April 18, 1957 (Panofsky 1992, pp 131-132), and though it was strongly backed by the Stanford University administration the proposed accelerator was of a size that superseded the capacity of the university both spatially and in organizational terms. The congressional committee overseeing AEC also "questioned whether public funds of such magnitude could justifiably be allocated to a private university" and thus, as amendment to the AEC's decision in favor of the project, it was suggested that it be established as a National Laboratory and thereby become a national user facility (Lowen 1997, p 179). With a time lag of one year, on September 15, 1961, Congress authorized the project with the cost estimation of \$114 million (Panofsky 1992, p 132; *SLAC Story 1966*, p 57). Groundbreaking for the project, that now was called SLAC, took place in July 1962. In November 1966 the first high energy physics experiments were conducted (*SLAC Story 1966*, pp 58, 114). Panofsky, who became the first director of SLAC and remained in that position until 1984, writes in his memoirs:

"I was often asked, after the initial completion of SLAC construction, how long the laboratory could productively operate. My standard answer was: 'Ten years, unless someone produces a good idea'" (Panofsky 2007, p 126).

MIT physicist Burton Richter had arrived at the Stanford University High Energy Physics laboratory in the late 50s to start working on new accelerator concepts, especially colliding beams storage rings (Sessler and Wilson 2007, p 82). In the early 60s, Richter and colleagues had finalized a design of an electron-positron storage ring that SLAC adopted. After the proposal had been

1957 both Argonne National Laboratory and Brookhaven National Laboratory were well under way with their large accelerators, and the Lawrence Berkeley Lab was planning the next generation (Westwick 2003, p 183).

turned down by the AEC six times, Panofsky reached an agreement with the agency in 1970 that SLAC could build the ring without separate construction authorization, but using equipment funds from the regular SLAC budget (Panofsky 2007, p 119). Construction of the SPEAR storage ring started in 1970, and it stored its first beams in April of 1972. After two years it was upgraded to higher energies, the so-called SPEAR 2 upgrade, which almost immediately led to its first significant success, the events in the fall of 1974 that got the name the ‘November Revolution’ and set particle physics on a new route (see below). Somewhat ironical, given the obstacles of the process to get funding, the SPEAR ring allegedly became “probably the most effective particle collider ever built as measured by its productivity in relation to its cost” (Panofsky 2007, p 120).

The Stanford Synchrotron Radiation Project (SSRP)

“My purpose of writing this is to ascertain whether or not there might be any long-term interest in using the cyclotron [sic] radiation from the SLAC storage ring for solid state studies. The possibility of using the radiation is beginning to open up new fields for solid state physics and chemistry. If SLAC does obtain a new storage ring, it would seem a pity not to explore any long-range possibilities for the use of the cyclotron radiation for solid state studies. I would be glad to discuss this with you if there appears to be any interest on the part of SLAC” (Quote from William Spicer’s letter to SLAC director Panofsky, dated 18 June 1968 and reprinted in Deken 2002, p 24).

As mentioned in chapter 3, synchrotron radiation was a well-known phenomenon in high energy physics in the 60s, and at SLAC, careful studies of the radiation had been done as part of the developmental work for the SPEAR storage ring construction. Among potential users, the flickering and unstable light flashes from synchrotrons were not perceived as usable, but the prospects of obtaining a stable enough beam of radiation from a storage ring seemed good enough for Stanford physicists to draft the idea when they learned SLAC was working on such a machine:

“Doniach and Spicer both came to me and they said basically, not these words but this is the way I remember it: ‘If you’ll let those x-rays out, we will revolutionize condensed matter physics’” (*Richter interview*).

To evaluate the feasibility of such a bold prediction, Richter gave a SLAC solid state physicist and an accelerator physicist within his own ranks the assignment to assess both the prospects of using x-rays from the SPEAR ring and the risks involved. The resulting study report from December 1972 (the *Fischer Study* hereafter) argued that the risk that the project would in any way damage the ordinary high energy physics program at SPEAR was very small – storage rings produced radiation all the time and the only modification to the ring was the attachment of a pipe to let that radiation out (*Richter interview*). SLAC director Panofsky was reportedly “very open minded” (*Lindau 1 interview*) and with clear indications that the ordinary activities at SPEAR would not be interfered with, he reportedly decided to concur. In his own recollection of the events, Burton Richter actively supported the proposal and decided to assist:

“I just modified one of the vacuum chambers, it cost us 25000 dollars, to let the x-rays out. [...] We drilled a hole in the shield wall, and then I also bought them a garden shed from Sears Roebuck. Sears Roebuck had these things that you put in your garden, they didn’t have anything to house their equipment. I don’t remember what the garden shed was, it was only a few hundred dollars” (*Richter interview*).

With regard to the prospects of maintaining a national user facility, which was the ambition of the Stanford group, the Fischer study was cautiously positive and recommended “a phased, step-by-step development” with careful evaluation of demands and capabilities in each instance (*Fischer Study 1972*, p 7).

The Stanford professors Sebastian Doniach and William Spicer had started to “initiate discussions among Stanford faculty members” over the possibilities of using synchrotron radiation in their work in 1970 (Doniach et al 1997, p 384) and gotten several positive answers. In the Fischer study, a list of 21 expressions of interest was included; nine of them bore names of Stanford faculty and the rest were fairly equally distributed among 10 academic and industry institutions in the US.⁹¹ These 21 groups were also involved in the first application for funding to the National Science Foundation (NSF), for the establishment of the Stanford Synchrotron Radiation Project (SSRP), which

91. These were California Institute of Technology in Pasadena, Ca (3 groups), University of Washington, Seattle (2), Xerox Corp., Palo Alto, Ca (2), National Bureau of Standards in Washington, DC (2), University of California, Berkeley (1), Carnegie Institute, Troy, Mi (1), the Naval Air Weapons Station, China Lake, Ca (1), University of Illinois, Chicago, Il (1), Argonne National Lab, Il (1), and Bell Labs, Murray Hill, NJ (1) (*Fischer Study 1972*, pp 4-5).

was submitted already in 1971 (Doniach et al 1997, p 384). The application contained basic descriptions of synchrotron radiation, including comparisons with other x-ray sources, and summaries of “two classes of experiment [sic], both of which show considerable scientific promise” and a few further possibilities (*Stanford proposal 1971*, pp 8-9). The two were medical diagnostic x-ray techniques and x-ray photoemission spectroscopy (XPS). Both descriptions were based on the unique capabilities of the intense radiation, that “it would be extremely hard to achieve this resolution by any means other than the synchrotron radiation source”. The future possibilities discussed were x-ray diffraction for crystallography studies in biology as well as diffraction for materials science, and so-called absorption edge spectroscopy (*Ibid.*, pp 9-12).

With the go-ahead from the SLAC management, and the SPEAR ring already equipped with basic capabilities for attaching a first beamline, the pilot project could start without awaiting the answer from NSF to the 1971 application. With money granted by the Stanford Center for Materials Research (CMR) and the Office of the Dean of Engineering, the first installments were made to extract radiation from SPEAR (Doniach et al 1997, p 384). To cover for the costs of experimental equipment, an agreement was made with NSF that resulted in the submittal of an interim proposal. This proposal nonetheless included descriptions on facility management, user and proposal policies, staff requirements, building layout, time and cost schedules, and details on the initial five experimental stations to be constructed (*Stanford proposal amendment 1973*). The NSF awarded an interim grant of \$59,000 for the pilot project and it started formally on January 1, 1973 (Doniach et al 1997, pp 384-385). On July 6, 1973, the first results from a synchrotron radiation experiment at SLAC was obtained (Cantwell 1994b, p 5).

The National Science Foundation had started evaluating the prospects of national synchrotron radiation programs at about the same time. The 1966 demise of the Midwestern Universities Research Association's (MURA) proposal to host the next federally funded high energy physics machine (see chapter 2) and the consequential decision in the Atomic Energy Commission to cut all support for intermediate accelerator projects within MURA had left the ‘Tantalus’ storage ring half-built at the University of Wisconsin, Madison. With support from the US National Research Council, Tantalus was redesigned and ready-built for synchrotron radiation, and started operation in 1968 as the world's first dedicated storage ring for synchrotron radiation (Lynch 1997, pp 334-335). Its achievements triggered the National Science Foundation to make plans for further synchrotron radiation initiatives, and issue a call for proposals for projects in 1972 that could make available radiation in the x-ray regime. The Tantalus group, a group at Harvard (where the

Cambridge Electron Accelerator, CEA, had been in operation for high energy physics since 1965), and the Stanford group submitted proposals (Sessler and Wilson 2007, p 124; *Winick 1 interview*).

The two proposals submitted to the NSF from Stanford and Harvard contained extensive descriptions of planned experiments, primarily in spectroscopy. Remarkable differences however separated the two proposals. Whereas the Stanford proposal was a comparably modest request for an approximate amount of \$1.2 million for ‘parasitic’ operation with one beam port at the SPEAR storage ring (Doniach et al 1997, p 380), the CEA in fact proposed to rebuild the whole storage ring to turn it into a dedicated synchrotron radiation source, with insertion devices and several beamlines. The amount of money requested also stands out: the whole program at CEA would cost over \$4 million during a 36-month period (*Harvard CEA proposal 1972*, p 1). In short, the choice NSF would make was to either fund an entire dedicated source at Harvard, which probably in practice would mean a commitment for a longer period of time than the 36 months cited in the proposal; or small-scale, parasitic operation on a ring already in use, with a potential of gradual expansion should the research turn out successful. The prospects for success in the harder x-ray region were probably unsure enough to make the NSF choose Stanford, a smaller commitment in nearly every aspect⁹² (Sessler and Wilson 2007, pp 123–124; *Bienenstock, Doniach, Richter, Winick 2 interviews*). As a result of the NSF decision to grant the Stanford proposal money, the Harvard accelerator was shut down, while NSF funding for Tantalus in Madison, covering the soft x-ray spectrum, continued (Lynch 1997, p 336).

Starting up

With the \$1.2 million from the National Science Foundation granted in July 1973, the Stanford group could start its expansion of the first beamline to

92. Several reasons are given for the NSF choice of Stanford. Later SSRP director Arthur Bienenstock says NSF regarded synchrotron radiation a very risky venture and therefore went for the cheaper alternative. He also claims the scientific base at Harvard was judged insufficient by the NSF (*Bienenstock interview*). Sebastian Doniach, founding director of SSRP, names existing accelerator infrastructure and competence to build and run it, something Harvard lacked but SLAC were nation-leading in, and also points at the obvious advantage of parasitic operation: “The machine ran with DOE money so the NSF didn’t have to pay for the machine. That was the big deal” (*Doniach interview*). The long-term ambition to create a national user facility probably also weighed in, since the NSF is a national agency with foremost national ambitions that likely would not have granted funding on that level to an internal Stanford University project (*Doniach, Richter interviews*).

serve the five experimental stations that had been planned in the original application (Doniach et al 1997, p 380). Organizationally, the SSRP became a 'project' within the W. W. Hansen Experimental Physics Laboratory at Stanford, and was given the same status as any outside users group at SLAC (*Fischer Study* 1972, p 2; *SSRL Users Newsletter October 1993*, p 4). As it established itself more permanently at the SLAC site, arrangements had to be made to assure a smooth coexistence of the synchrotron radiation activities and the ordinary high energy physics program at the SPEAR ring. In need of a person with some experience of beamline design and operation and with accelerator physics background, the Stanford group took the opportunity created by the close-down of the Harvard accelerator project and recruited Herman Winick, technically responsible for the Harvard proposal to NSF. As the only accelerator physicist in the synchrotron radiation project group and with experience of high energy physics research from Harvard, Winick became the 'mediator' between the synchrotron radiation project and their users on one side and the rules laid out by Richter and the SPEAR operations team on the other. The relationship was truly 'parasitic', as there was no doubt about the priorities:

"SLAC will not charge SSRP for the cost of producing synchrotron radiation. [...] SLAC will have no scientific program responsibility for the conduct of the SSRP program. [...] Experiments must meet the University's requirements for research projects to be done on Campus, including that the work be unclassified. [...] SLAC must have effective control of the design and installation of the building and associated utilities, and of any hardware which connects to SPEAR, in order that SPEAR's use for particle physics shall not be interfered with" (Ground rules for the synchrotron radiation program at SLAC, *SLAC-SSRL statement* 1973, p 2).

"I was able to work with the high energy physicists and they were like, we're busy, we're doing real physics, the fact that the light doesn't shine into your slits is your problem, you know the ground rules. And they were right" (*Winick 2 interview*).

"People were coming from the east coast [...] and as they were on the plane the high energy people changed the schedule because they got some new idea and there would be no x-rays" (*Winick 1 interview*).

Despite these insecure conditions, successful experimental work was done and especially the spectroscopy results showed the "highest resolution yet

seen" (Cantwell 1994b, p 5). The very first results from synchrotron radiation use at SPEAR were published in the July 19, 1974 issue of *Nature* (Lindau et al 1974).

In order to manage the setting up of all five projected experimental stations on the first beamline despite shortage of funds, Principal Research Teams (PRTs) were created (though not named PRTs at the time). Groups of researchers from Bell Labs, Caltech, the US Naval Weapons Center at China Lake, Xerox, Stanford University and the University of Washington were assigned experimental stations and received guaranteed beamtime in return (Doniach et al 1997, p 380). The geographical distribution of these teams helped ensuring the project's status as 'national facility'. From the Stanford group's perspective, it was necessary to bring in external competencies in order to establish a user facility, which had been the clear strategy already from the start.⁹³ The inspiration came largely from the high energy physics program at SLAC, which was equipped with a Program Advisory Committee (PAC) and a system of external referees to review experimental proposals from outside users (*Doniach interview*; *SSRP outside users document* 1972). Recruiting users from across the nation was likely also a strategy to attract complementary competencies, primarily specialists in the experimental work, to achieve the 'task heterogeneity' required to operate the instruments successfully.

The 1973 amendment to the proposal to NSF contained detailed descriptions of user and beamtime policies and how these were to be put in practice, with a proposal review panel and external expert referees, in all amounting to a facility "planned to be completely 'user-operated'" (*Stanford proposal amendment* 1973, pp 8-9, 12).

The Stanford Synchrotron Radiation Project opened to external users in May 1974 (Doniach et al 1997, p 388). In its first year running as a national user facility, it hosted 19 groups of totally 55 individual experimenters, doing 29 experimental runs (*SSRP document* 1975, p 1). The first users' meeting was held in October 1974, attracting 100 people (*SSRL Users Newsletter* October 1993, p 2).

93. Already in the SSRP 'Outside Users Document' from 1972, it is clear that the ambition of SSRP was something similar: "[W]e intend to establish functions for program review following the SLAC model [...] [I]t is our intention to evaluate priorities on the basis of scientific merit and on this basis to make the facility freely available to any qualified user" (*SSRP outside users document* 1972).

First steps towards a multipurpose laboratory

The main enhancement of synchrotron radiation performance offered at SLAC was the hard x-rays that could be produced when SPEAR ran on its highest energy, close to 4 GeV. In a 1975 *Science* article, the capability to produce x-rays with wavelengths down to 0.3 Ångströms was highlighted as a “unique” feature that only the Stanford lab could provide (Robinson 1975, p 1074). The first real experimental breakthrough offered by these “unique” conditions was the EXAFS⁹⁴ experiments done by a University of Washington/Boeing group (Cantwell 1994b, p 5). These first EXAFS experiments are often mentioned as particularly important for showing the usefulness of x-ray synchrotron radiation (*Bienenstock, Winick interviews*) – one researcher was able to collect as much EXAFS data in three days at SLAC than he had managed to do in the previous ten years. For the EXAFS technique, this is described as the arrival of a “new era” (Lytle 2007, p 9). The x-rays from the SPEAR ring was a hundred thousand (100,000) times more intense than that from state-of-the-art lab sources, which in effect meant a shortening of the required time to take a useful EXAFS spectra with a hundred thousand times, an extraordinary improvement in experimental performance:

“You know, having worked in high energy physics where an increase in colliding beam luminosity of a factor of two was worth spending millions of dollars on, to suddenly get a factor of a hundred thousand... That unleashed a whole flood of people interested in hemoglobin in particular and other biological materials, enzymes, proteins, they just flooded into this place and started doing things. So within literally a few months to a year, all hell broke lose. All the speculations whether it would be useful or could be done were answered very clearly” (*Winick 1 interview*).

The theoretical benefits of this dramatically higher brilliance made the conviction firm among the people involved that synchrotron radiation would prove very useful for most sciences that was already using x-rays in their experimental work. But some time and effort was clearly needed to convince representatives from these branches of the sciences:

“We all thought it was a good idea. [...] The interesting thing was that many people within the x-ray science community thought this was a waste of time, because they said we already have our x-ray machines” (*Doniach interview*).

94. Extended X-ray Absorption Fine Structure, a spectroscopic technique. See chapter 3.

“They would say ‘we can do everything in our own labs, we don’t need to come to a synchrotron radiation facility’” (*Bienenstock interview*).

What made most potential users hesitant to come, although they probably realized the potentials, was the risk of operation instabilities ruining their data collection, after them having traveled far and spent time and effort on learning how to operate new equipment:

“Some people have a sense that this source is so much better that they are willing to put up with the limitations of parasitic operation, the frustrations and all the things that we went through, others are more limited, they’d say ‘if I come here three or four times and come back each time with no data then the heck with you guys!’ They’d have a more reasonable life at home even though they can’t do as much in principle as they could do here” (*Winick 2 interview*).

The active *recruiting* of users was important early on. Keith Hodgson⁹⁵ had become assistant professor in chemistry at Stanford University in 1973, without knowing anything about the synchrotron radiation program at the SLAC site nearby, but with experience in x-ray crystallography. After learning about SSRP, “it didn’t take much thought” to recognize the possibilities, and since Doniach and Spicer were “kind of looking for people interested in trying out things” the connection was established. A group from Hodgson’s Stanford department joined the efforts of the group building the EXAFS station on Beamline I, and that way the group got some beamtime already in 1974, for x-ray absorption studies. Another of the stations at the first beamline was built for fiber diffraction, and with some effort it could be modified to also host other diffraction studies. So the group took a diffraction camera from their laboratory on the Stanford campus and brought it to the beamline at SPEAR. “There was nothing optimized in alignment, we kind of leveraged this camera into the hutch, built it up, manually aligned it by going in and out, and that is how we were able to collect the first diffraction data” (*Hodgson interview*). The results were published in a 1976 article in the Proceedings of the National Academies of Science of the USA (Phillips et al 1976), highlighting that smaller samples could be used and that the results still showed 60 times better resolution than what could be achieved with a conventional x-ray source. The tunability⁹⁶ of the source was mentioned as a major advantage. In summary, the article claims the source to be “very useful for single crystal protein

95. The following paragraphs are entirely based on an interview with Keith Hodgson.

96. The possibility to tune the x-rays over different wavelengths. See chapter 3.

diffraction studies" (Phillips et al 1976, p 128). Keith Hodgson comments that although the results "unequivocally" showed that synchrotron radiation offered "significant advantages for crystallography", the measurements were only "doable by force", and everything was "well below optimum conditions, so you can't imagine a macromolecular crystallography group from a normal university coming in doing something useful, it just wasn't at that stage" (*Hodgson interview*).

It was under such comparably suboptimal circumstances that the first experimental data was collected with hard x-ray synchrotron radiation; in crystallography and in EXAFS and other spectroscopic experiments. Although both the advertising efforts from the SSRP group and the published results from experimental runs were met with much hesitance and disinclination in the broader scientific communities, some synchrotron radiation users found their way to SLAC, and the number of users increased with an approximate annual doubling the first three years (*SSRL Users Newsletter October 1993*, p 3). A second NSF grant in 1975 allowed extension with another bending magnet beamline, which began operation in June 1976 (Doniach et al 1997, p 381). The three stations on this extension, called Beamline II, were built by outside teams consisting of researchers from Berkeley Lab, Bell Labs, Oak Ridge, Argonne and IBM (*SSRL Highlights document 1983*). But by that time, the 'November Revolution' in high energy physics had already caused the first 'x-ray drought' at SLAC.

The first 'x-ray drought'⁹⁷

A famous moment in the history of particle physics took place in November 1974: The simultaneous discovery at Brookhaven National Laboratory and SLAC of a new particle that eventually got the name J/psi. These events had a big enough impact to be called 'the November Revolution' among particle physicists; it was "an event that would help open the 'new' physics" (Galison 1987, p 1). At SLAC, in the detector of SPEAR, a particle trace that resembled the shape of the Greek letter psi was recorded, and the very same day, a Brookhaven team of scientists reported the discovery of what they chose to call the 'J' particle. The leaders of the respective groups, Burton Richter at SLAC and Samuel Chao Chung Ting of MIT, shared the Nobel Prize in physics only two years later. For particle physics, this was a decisive moment.

97. This term is used by most interviewees and in official SSRP newsletters from the 70s, and can therefore be regarded as the canonized expression for the shortage of synchrotron radiation in the x-ray range at SLAC caused by the events described in this section.

For the synchrotron radiation program at Stanford, it was a "disaster" (SSRL *Users Newsletter* October 1993, p 2).

The momentous discovery was done with SPEAR running at 1.5 GeV per beam, which was well below its design capabilities of 3.0 GeV per beam (SPEAR3 *Close-Out Report* 2004, p 3) and an energy at which no radiation in the hard x-ray region was produced. Following the discovery, the program at SPEAR was entirely focused on exploring this energy region and doing further studies of the newly discovered particle, leaving the synchrotron radiation program with radiation only in the VUV regime and a small part of the soft x-ray spectrum (Cantwell 1994a, p 44). This meant that the experimental stations designed for hard x-rays, as many as three on beamline I (and three planned for beamline II) became practically useless. The ground rules laid down at the start of the program and cited above left the synchrotron radiation group with no influence whatsoever over this situation: "As a parasitic operation, the SSRP had no control over the electron energy" (Doniach et al 1997, p 382). On short term, this meant that several of the most promising developments at SSRP were halted, and the prospects of convincing biologists and other potential users to try the possibilities of synchrotron radiation were severely damaged (Bienenstock, Winick 1, 2 interviews). Only after a few years of x-ray drought did a technical solution to the problem emerge.

Wigglers existed as an idea already in 1972, and it was mentioned in an appendix to the SSRP feasibility study cited above (*Fischer Study* 1972, pp 30-31). By the mid 70s, the wiggler concept had reached maturity enough to make a trial in practice reasonable. At SLAC, the synchrotron radiation group wanted to use a wiggler to be able to produce hard x-rays even with the storage ring running at lower energies, but it took a few years before a design was developed that could convince the high energy physicists to allow for a modification of the magnet lattice of the ring. The SSRP accelerator physicist Herman Winick had had some experience with early versions of the wiggler concept at Harvard and knew that wigglers could be used also to affect the particle beams in accelerators in certain advantageous ways.⁹⁸ Together with the fact that the wiggler was made up of electromagnets and thus could be switched off entirely if it in any way would harm the high energy physics program, this helped in convincing the high energy physicists to modify the

98. The CEA at Harvard was originally built as a synchrotron in which the electron bunch was tilted up and down as part of the acceleration process. When the CEA was to be turned into a storage ring, wigglers were used to counter this tilting and stabilize the beam. The wiggler implemented in the SPEAR ring at SLAC had a different effect; it expanded the beam and thereby it enabled more current to be put in the beam. This enabled a higher collision rate in the collision experiments and hence yielded more data output for the high energy physics program (Winick 2 interview).

Year		Source	Stations
1974	Beamline I	bending magnet	4
1976	Beamline II	bending magnet	4
1978	Beamline III	bending magnet	4
1978	Beamline IV	wiggler	3

Table 2: Expansion stages of SSRP/SSRL during the 1970s (*SSRL highlights document* 1983).

storage ring (*Winick 2 interview*). The wiggler was tested in SPEAR in 1978, and having a positive rather than negative effect on the high energy physics program (cf. note 97), it was allowed to remain there and the ‘x-ray drought’ was over after nearly four years.

Several things had happened in the meantime. As mentioned above, good results primarily in EXAFS and crystallography had been produced before the ‘x-ray drought’. The user base had begun to grow slowly. The second beamline had become operational with three experimental stations in 1976 (see table 2), and the plans for the major Phase II upgrade of SSRP – mentioned as the second step already in the 1971/1973 applications – was turned into an application for funding to the National Science Foundation in 1976. Apart from additional beamlines, the proposal called for a major new building to house the new experimental stations, plus plans for dedicated operation of SPEAR on 50% of the total uptime, and the development of the wiggler (*SSRL Users Newsletter October 1993*, p 4). The synchrotron radiation program at SLAC had developed into a lively activity of its own, and the scientific results together with the growing number of users spurred action both at SLAC and elsewhere. In a letter from January 1976, SLAC director Panofsky stated that “[o]ur problem is essentially one of ‘embarrassment of riches’ in respect to both elementary particle physics and synchrotron radiation use of SPEAR”, and the conclusion of the letter is a promise that SPEAR would be made available for dedicated synchrotron radiation use on 50% of its time once the next accelerator at SLAC, called PEP (Positron Electron Project), reached a stage of 50% operation in experimental mode. This was a message well received not only locally but also on national level, and it contributed to the realization of the Phase II (*SSRL Users Newsletter October 1993*, p 4).

In a 1976 report on synchrotron radiation issued by the National Academy of Sciences (NAS), the collected national need for beamtime was projected to increase, and the report concluded that the current facilities in the United States had inadequate capabilities to meet these demands. The report based its analysis partly on the experiences from Stanford of operating a multi-GeV storage ring for light production and the success of experiments done there (Doniach et al 1997, p 381). It set off a general “spurt of construction” (Robinson 1982, p 1211), including the funding and start of construction of the National

Synchrotron Light Source (NSLS) at Brookhaven National Laboratory, the initiation of a new facility in Madison, Wisconsin, and an upgrade of the facility at Cornell University (Ibid.; Batterman and Ashcroft 1979, p 159). The report also had the effect that the National Science Foundation immediately commenced funding for the Phase II expansion of the synchrotron radiation program at SLAC. The grant, in total \$6.7 million, was awarded in July 1977 (*SSRL Users Newsletter October 1993*, p 4). Simultaneously, the organizational form of the project was changed, so that it became an independent laboratory within Stanford University. It was renamed the Stanford Synchrotron Radiation Laboratory (SSRL) and organizationally placed directly under the Stanford University vice-provost for research (*SSRL Users Newsletter October 1993*, p 4). The formal transition was done with a dedication ceremony on October 27, 1977 (Deken 2002, p 49).

Partly and fully dedicated

From a user and operations perspective, the changes of organizational status and the name change from SSRP to SSRL were minor compared to the long-awaited materialization of Panofsky's earlier promise that 50% of the SPEAR running time would be dedicated to synchrotron radiation once the next storage ring for high energy physics at SLAC was taken into operation. In October 1979, SPEAR became 50% dedicated for synchrotron radiation use (Cantwell 1994a, p 44; *SSRL Activity Report 1983*), and SSRL started running SPEAR on its own premises, maximizing beam stability and lifetime and otherwise optimizing operation for synchrotron radiation research. But the 50-50 division of running time between the two experimental programs was only a basic principle for the scheduling, and operation of SPEAR was still under SLAC control, meaning that in practice SSRL were still parasites on the SLAC machine group to get their radiation out through the beamlines. The original 3 km SLAC linac was still used for SPEAR injection, but it also had a major part in the commissioning of new high energy physics accelerators at SLAC, the PEP ring and eventually the SLC (SLAC Linear Collider), who both had overriding priorities and caused severe disturbance to the synchrotron radiation operations, despite the principal 50-50 division.

The PEP was a storage ring collider similar to SPEAR but many times larger and with several times higher energies. Construction began in June 1977 and it stored its first electrons in April 1980 (Deken 2002, pp 49, 57). Though never producing results as spectacular as those of the 'November Revolution' (Panofsky 2007, pp 138-140), PEP experiments pointed out the next step for

Year	Days	% delivered
1979	36	78.2
1980	129	64.9
1981	117	72.7
1982	117	79.4
1983	127	78.9
1984	41	80.7
1985	202	66.1
1986	119	70.9
1987	143	69.2
1988	0	-
1989	54	43.4
1990	31	74.4
1991	115	59.7
1992	204	77.1
1993	197	84.2
1994	216	89.1

Table 3: Annual number of days of synchrotron radiation operation on SPEAR, 1979-1994. An estimation of the total average number of days of SPEAR operation can be made from the figures of the last three years (1992-1994), when SPEAR was entirely dedicated to synchrotron radiation and the independent injector had been taken into operation. The numbers in the right column denote the percentage of the time in the second column actually delivered to users (*SSRL Activity Reports 1989, 1994*).

SLAC, namely the SLAC Linear Collider (SLC) concept, built on the existing linac and for which construction started already in 1983 (*Ibid.*). The sizes and magnitudes of these two projects unbendingly made them dominate SLAC activities from mid 70s and for two decades to come, setting the agenda for operations, construction schedules, and the extent to which the synchrotron radiation 'parasites' could be allowed to interfere. After all, SLAC was still a single-mission US National Laboratory and SSRL had in principal only the status of an 'outside user's group'. From the 50%-dedication in 1979 and for the following more than ten years, SSRL ran SPEAR for approximately half of its uptime (see table 3) (*SSRL Activity Reports 1989, 1994*). In the years 1980 to 1983, the promise was fulfilled and approximately half of the time of SPEAR operation was dedicated to SSRL. In 1984, however, SLC construction had started, and the state of the whole SLAC, including operation of the linac, was altered. In 1988, due to linac and PEP operations and SLC construction, no time at all was given to synchrotron radiation operations, and in 1989 and 1990 the amount of dedicated days was well below 50% (*SSRL Activity Reports 1984, 1985, 1988, 1989*). This period of limited operations is often referred to as the 'second x-ray drought', for example in a 1989 report on synchrotron radiation sources summarizing the past decade's activities at SSRL and the National Synchrotron Light Source (NSLS). The report commented on the shortcomings of both labs with respect to operation stability and user friendliness, and the 'x-ray drought' at SLAC was explained as characterized by four manifested insufficiencies: "limitations on scheduled beam

time, poor machine performance when running, and capricious scheduling”, as well as “last minute cancellations of scheduled runs and scheduling with short notice” (*Cardillo Report 1989*, p 20). Two primary reasons for the SSRL shortcomings as user facility were mentioned: “Poor maintenance of SPEAR in the absence of HEP [high energy physics] interest” and “availability of the linac” (*Ibid.*). The SSRL Activity Reports from the period in question express a mixture of optimism over the general scientific and technical development of synchrotron radiation, at SSRL and elsewhere, and pessimism over the x-ray drought and the constant subordination of SSRL progress and expansion to the high energy physics program at SLAC. But also budgetary limitations within SSRL caused a damping of the activities (*SSRL Activity Report 1987*, p 2; mentioned also in *SSRL Activity Reports 1986, 1988, 1989*).

Another source of optimism was the brief window SSRL had for making use of synchrotron radiation from the PEP ring. In 1985, an undulator beam-line was added to PEP (Cantwell 1994b, p 6), and though operation was entirely parasitic, PEP ran some days in so-called ‘low emittance mode’, which allowed for extended testing of the radiation and some tentative experimental work. In the 1986 SSRL Activity Report, it is said that the radiation from PEP is “approximately equal” to that expected from the APS (the Advanced Photon Source, one of the ‘big three’, see chapters 3, 6 and below), which was on design stage by then (*SSRL Activity Report 1986*, p 1). One consequence of the x-ray drought had been the conceptualizing and preliminary design of a new, dedicated storage ring for synchrotron radiation on the SLAC site, as part of future SSRL activity. The promising test runs on PEP however changed the ambitions of SSRL and the plans to construct a new multi-GeV ring were abandoned in favor of “developing SPEAR and PEP as synchrotron radiation sources” (*SSRL Activity Report 1986*, p 1).

The ultimate source of pessimism at SSRL was doubtless the SLAC Linear Collider project. In an SSRL status report from 1991, it is stated that “the amount and quality of SPEAR’s beam time for synchrotron radiation users has been lower than in previous years” and that this is largely because of “the high priority given by SLAC to its linear collider (SLC) project which resulted in reduced staff support of technical components and reduced availability of the linac as an injector to SPEAR” (Tatchyn and Winick 1991, p 17). In addition to making the linac largely unavailable as SPEAR injector, the SLC had severe startup problems that made the whole of SLAC concentrating its efforts away from SPEAR and PEP (Plummer 2008, p 18).

SLC started producing experimental data in 1989 (Deken 2002, p 79), but the years before and around the start of operation are represented by very low figures of delivered beam time at SPEAR (see table 3). Though the SLC experience was damaging for the synchrotron radiation program on short

term, it eventually resulted in a turn of events that was beneficial in the long run, namely the decision to discontinue the high energy physics program at SPEAR and invest in a new injector to make SSRL completely in charge of SPEAR operations. For quite some time, the view internally at SSRL had been that separate injection was the only solution “if SSRL was to meet its users’ needs” (Cantwell 1994a, p 44), and the Department of Energy (to whom SSRL stewardship had been transferred a few years earlier, see below) had started to lose patience with the unstable operations of parasitic (or by this time, semi-dedicated) running of SPEAR for synchrotron radiation (*Cardillo Report 1989*, pp 22-23).

Funding for the new injector commenced in February 1988 (Tatchyn and Winick 1991, p 14), and it was taken into operation in October 1990 (Cantwell 1994b, p 6). An upgrade of the SPEAR magnet lattice lowered emittance but postponed full-scale user operation for another one and a half years. On February 17, 1992, SSRL began its first run as fully dedicated synchrotron radiation laboratory for outside users (*SPEAR3 Design Report*, p 4; *SSRL Activity Report 1994*, p 1).

The SPEAR ring had received continuous incremental upgrades since its commissioning in 1972 and an energy upgrade in 1974 (that turned it into SPEAR 2), but the core remained the same – an old and basically outdated storage ring not designed for synchrotron radiation production but rather adapted to it step-by-step. In January 1997, a study group was formed to begin design studies of a major upgrade of the SPEAR ring to answer the demands of the user community (*SPEAR3 Close-Out Report 2004*, p 3). In May 1997, the study group presented its report to SSRL users at a workshop, which gave its full support:

“The important new opportunities for Science and Technology afforded by SPEAR3 provide compelling reasons to proceed as rapidly as possible. The use of undulators in SPEAR3 will provide unparalleled capabilities in the 1-4 keV spectral region [sic]. This new technology promises to foster scientific advances in many fields. The SSRL users’ workshop gives its strongest possible endorsement to the SPEAR3 project” (*SPEAR3 Design Report*, p 1).

The upgrade included a complete replacement of the existing magnet lattice, the vacuum chamber and the radiofrequency system. Its primary goals were to reduce emittance with one order of magnitude and to double the current (*SPEAR3 Design Report*, p 4). The endorsement from the users gave a go-ahead for applications to federal agencies, and the 1997 *Birgeneau-Shen Report* (a major federal assessment of synchrotron radiation facilities, see be-

low) strongly recommended government support for the SPEAR₃ upgrade (*Birgeneau-Shen Report 1997*, pp 10, 120-121). The relatively high calculated return for investment made funding of the project a priority in Washington. Especially the life sciences program was predicted to receive a boost from the upgrade, which is shown by the eventual splitting in half of the final bill of \$58 million between the National Institute of Health (NIH) and the 'ordinary' steward agency the Department of Energy (*SPEAR₃ Close-Out Report 2004*, p 43). SPEAR₃ was completed in 2003, three months ahead of schedule and within its budget (*Ibid.*). The upgrade took SPEAR from second to third generation synchrotron radiation source, which has far-reaching effects on the performance of beamlines and experimental equipment, as well as on the demands placed by funding agencies and the user community on the capabilities of the surrounding organization to scientifically and technically meet the challenge of the enhanced performance given by a modern storage ring (*Pianetta interview*). Consequently, SSRL is undergoing an organizational upgrade and is scheduled to receive a major budget increase, a matter that will be discussed in further detail below.

The national context

When first funded by the National Science Foundation and given access to synchrotron radiation from SPEAR, the Stanford Synchrotron Radiation Project was little more than a university project, with involvement of external users but run by a few Stanford professors, post-docs and students. Larger scale projects utilizing heavy infrastructure – 'big science' in short – was the responsibility of the Atomic Energy Commission and its successors the Energy Research and Development Administration (ERDA) and the Department of Energy (DOE). In 1977, when the SSRP Phase II expansion had been granted money and the promise of half time dedication of the SPEAR accelerator to synchrotron radiation was approaching realization, the synchrotron radiation program at SLAC started to "grow beyond the scope of a traditional NSF project" (Cantwell 1994a, p 44).

As mentioned in chapter 2, the heterogeneity, scale and scope of the National Laboratories enabled for continuous renewal and development of their missions and purposes, initiated both locally at the Labs and through central initiatives by the steward agency, and this renewal and revitalizing emerged as a means for survival. In the mid to late 70s, synchrotron radiation had started to prove itself useful and feasible, and it was becoming clear that synchrotron radiation would require collocation with major accelerator

facilities and on longer term their own dedicated rings. The 1976 National Academy of Sciences Assessment Report, concluding that the present and future demand for synchrotron radiation in the US would not be met by the available sources, recommended that “the present 7 hard x-ray and 16 soft x-ray and ultraviolet experimental stations available in the United States be increased to 60 and 40 respectively” (Robinson 1977, p 148). The answer of the steward agency was a swift request to Congress for funding of a new national synchrotron radiation laboratory, purpose-built and dedicated (Ibid.). Brookhaven National Laboratory, at the time the only multi-mission National Laboratory with a major high energy physics and accelerator program,⁹⁹ had drafted an idea of building and operating a dedicated synchrotron radiation source already in 1972, as it slowly became clear that the federal priorities in high energy physics laid elsewhere (at Fermilab and SLAC) (Crease 1999, p 364). When the National Synchrotron Light Source (NSLS) was proposed in Washington, Brookhaven soon emerged as the natural choice for the new federal facility.¹⁰⁰ SLAC was still a single-mission laboratory for high energy physics, at least in the view of the federal government, and although the chief competence and experience of synchrotron radiation within the National Laboratory system existed at SLAC, an establishment of a new, dedicated facility on the SLAC site would mean a switching focus of the SLAC mission, which neither the government nor SLAC management wanted (*Richter interview*). This also meant that the existing synchrotron radiation program at SLAC, now a separate laboratory within Stanford University, would remain the responsibility of the National Science Foundation for some more years (Robinson 1977, p 148).

The synchrotron radiation program at SLAC, however, did not cease to grow. With the Phase II upgrade and a partial dedication of the storage ring uptime, SSRL was in fact the largest synchrotron radiation laboratory in the United States, and furthermore it was still the only lab providing radiation in the hard x-ray range¹⁰¹ (*Eisenberger-Knotek Report 1984*, p 68). This and the fact that SSRL gradually became a larger part of the collected SLAC activities (though still very small compared to high energy physics and definitely sub-

99. The other two National Labs with high energy physics programs were Fermilab and SLAC, both single purpose labs. Argonne had a high energy physics and accelerator program, but it was terminated in the mid 70s in favor of Fermilab (Holl 1997, p 330).

100. According to one source, the decision of ERDA to build their new synchrotron radiation facility at Brookhaven was also made easier by the lobbying from east coast synchrotron radiation users tired of going to California to access hard x-rays (*Doniach interview*).

101. Though the NSLS project at Brookhaven consisted of two storage rings, one 700 MeV ring for VUV and soft x-rays, and one 2.5 GeV for hard x-rays (Robinson 1977, p 148), the hard x-ray ring was not finished until the mid 80s (*Eisenberger-Knotek Report 1984*, p 70).

ordinate), led to the 1982 transfer of stewardship for SSRL to the Department of Energy (Cantwell 1994a, p 44). At the same time, NSLS at Brookhaven started operation of its first storage ring, and the National Laboratory system had two synchrotron radiation laboratories in their battery of user facilities.

In 1983, another assessment of the national capacity in synchrotron radiation was done by the National Research Council, acknowledging the growth in number of users and publications from the existing labs and making the estimation that user demand would saturate national capacity by 1985 (*Lynch Report 1983*, p 14). Lawrence Berkeley National Laboratory soon released plans to construct a VUV/soft x-ray synchrotron radiation source, the *Advanced Light Source (ALS)*. This proposal and growing expectations with regard to purpose-built storage rings optimized for the use of insertion devices (especially undulators had been given great notice in the Lynch report) led the DOE to establish a study committee to assess the need of the growing community and report on their findings in order to enable for the inclusion of ALS into the fiscal 1985 budget (Robinson 1983b, p 826). The committee issued its report in early 1984, recommending construction of two new sources: one low-energy ring, "capable of providing fundamental undulator radiation in the soft x-ray region of the spectrum" and one high energy ring "capable of providing fundamental undulator radiation in the x-ray region" (*Eisenberger-Knotek Report 1984*, pp 14-15). It is clear from the report that the ALS proposal inspired the bid for a new low energy facility ("The Advanced Light Source (ALS) is such a machine", *Ibid.*, p 15), and that the contemporary European plans that eventually materialized in the ESRF had part in the report's recommendations for a high energy facility. Apart from suggesting investments in these two new facilities, the report recommended the Department of Energy to commit to continuing improvement of the existing facilities (NSLS and SSRL) and commence funding of upgrade programs for these facilities to enable them to start using undulators (*Ibid.*, p 14). The report generally marks a first shift in government attitude towards synchrotron radiation from ignorance to interest and commitment, acknowledging its potential and emphasizing its future importance for "university, industry, and defense interests" (*Eisenberger-Knotek Report 1984*, p 13).

The concrete outcome of the report was consequently a strong federal commitment to synchrotron radiation, including the doubling of DOE synchrotron radiation facilities by funding and construction of the two facilities suggested, the Advanced Light Source and *Advanced Photon Source (APS)* respectively. Though design studies of dedicated 6 GeV rings had been done at both NSLS and SSRL (*Eisenberger-Knotek Report 1984*, p 83), the eventual location of the American 'big one' would not be Brookhaven or Stanford but

rather Argonne National Laboratory outside of Chicago, Illinois, which can be seen as a sign of the strategy of 'parceling out' large projects among the National Labs, mentioned in earlier chapters.¹⁰² The ALS had been a Berkeley project right from the start and remained so, granted money in 1986 and opening in 1993 (Service 1999, p 1344).

The Advanced Photon Source (APS) opened in 1996. A very large project, initiated in a period of tight competition for federal research infrastructure investments, the APS did not receive full funding by Congress. The \$456 million granted in 1991 for APS construction did not include a complete set of beamlines and experimental stations (Holl 1997, pp 472-473), and so most of the beamlines were outsourced to external groups forming Participating Research Teams, at APS called Collaborative Access Teams (CATs). These teams took on full responsibility for the beamlines from the beam port in the storage ring to experimental stations, including all maintenance and user support. Highly specialized and focused in their scientific aims, they had success in getting their beamlines funded, but insufficient coordination and cooperation between the units eventually led to ineffectiveness in technical maintenance and user operation (*Galayda, Lindau 1, Lubell interviews*), also recognized by the DOE in its 1997 comprehensive assessment of its synchrotron radiation facilities (*Birgeneau-Shen Report 1997*, p 101).

The NSLS and SSRL had received similar criticism in the 1989 DOE-initiated *Cardillo Report*. In particular SSRL was heavily criticized in this report, with regard to operations stability and reliability, problems that were most of all due to the semi-parasitic relationship with SLAC mentioned above in connection with the 'second x-ray drought'. The Cardillo Report actively suggested a revision of the status SSRL held at SLAC, and a renegotiation of the priorities of synchrotron radiation research and the high energy physics program at SLAC:

"[T]he status of synchrotron radiation research has changed significantly with time. [...] On the other hand the relationship between SLAC and SSRL has not changed very much. This Committee feels that the time to re-examine this relationship with respect to scheduling, cost for operation,

102. An argument for locating the Advanced Photon Source at Argonne was allegedly that the lab had "missed a major role" in the Superconducting Super Collider project (Holl 1997, p 463). Furthermore, quite a few federal research infrastructure projects were in line to be initiated in the mid 80s, with the Superconducting Super Collider as a flagship. Oak Ridge was considered the natural place for a new reactor project, and the high energy physics tradition at Brookhaven weighed enough to place a heavy-ion collider there (Holl 1997, pp 467, 472-473). SLAC had enough to attend to with PEP and the SLC in operation and under commissioning, and the ALS was already destined for Berkeley (Service 1999, p 1344).

and maintenance of SPEAR is long overdue. It is inappropriate for a large fraction of the national research output of X-ray applications to biochemistry, chemistry, condensed matter physics, materials science, medical diagnosis, and surface science to be hostage to the vagaries of the HEP [high energy physics] community at SLAC, which has a wavering interest in its own storage ring experiments” (*Cardillo Report 1989*, p 24).

The result of the report was the decision of the Department of Energy and SLAC central management to suggest that SSRL be made a division within SLAC, thereby revoking the status of SLAC as single mission National Laboratory and elevating the synchrotron radiation program to a separate and – at least on paper – equal part of the laboratory mission. As described above, the SPEAR accelerator became fully dedicated to synchrotron radiation in 1990 and two years later it reopened as the second dedicated synchrotron radiation user facility within the US National Laboratory System. The third and fourth, ALS and APS, came on track within the course of a few years.

The Birgeneau-Shen assessment

With four national synchrotron radiation sources in operation, in 1997, the Department of Energy initiated a nationwide evaluation of synchrotron radiation. A special panel of sixteen members “who were leading scientists and technologists working in academia, industry, and the national laboratories” was convened (*Birgeneau-Shen Report 1997*, p 6) and given the charge to assess and evaluate the scientific impact of synchrotron radiation based research during the past decade; the expectations for the next decade; the scientific and technological demand for synchrotron radiation (also with respect to specific disciplines); the user demand, expected future capabilities, and scientific visions at the four DOE sources; and the future scenarios for the facilities should funding stay at the present level, be increased, or decreased. On basis of their evaluations, the panel was asked to give specific recommendations for the DOE policy regarding its synchrotron radiation laboratories for the coming years.

The 150 page report of this investigation is one of the most thorough assessments of synchrotron radiation science and its facilities made, in general and in the specific US case, on individual laboratory level, and with respect to user groups and areas of application. The report presents findings on both the general patterns in the US synchrotron radiation landscape and the specific

performance of the four facilities, along with very concrete recommendations for the immediate and long term future. In the report, the “remarkable diversity” of current synchrotron radiation research is named as “one of [its] most impressive features” (*Birgeneau-Shen Report 1997*, p 8). The growth in “number of participants” and the diversification of the field is expected to continue “for at least the next decade”, and so the facilities are urged to respond accordingly and develop themselves to accommodate this growth and diversification. The facilities are encouraged to broaden their user base and accommodate also “novice, non-specialist users who require significant technical support in order to carry out their experiments” (*Ibid.*, p 7). The individual facilities’ scientific and technological capabilities and performance, and their capabilities to accommodate users, are given a good overall assessment. Especially NSLS and SSRL receive favorable judgments that stand in sharp contrast to the ones in the *Cardillo Report* of 1989:

“The panel was very impressed by the outstanding performance of the second generation facilities (SSRL and NSLS), by the number of users they serve well, by their ability to renew and improve themselves, by their ability to continue cutting edge research even though the storage rings themselves are not the most advanced, by their commitment to education, and by their abilities to engage new users and address new problems. Given the outstanding track record and clear vision demonstrated by these facilities, the panel expects these facilities to continue to thrive scientifically in a cost-effective manner” (*Birgeneau-Shen Report 1997*, p 8).

On this basis, and given the fact that the Advanced Photon Source (APS) investment only recently had started to give returns (the APS opened for users in 1996), the panel’s recommendations for funding prioritized APS, NSLS and SSRL at the expense of the ALS in Berkeley. Operation of the three prioritized labs was placed highest, along with R&D for fourth generation light sources. Second highest priority was given to investments at APS and NSLS (primarily beamlines), and third a three-year commitment to NSLS and SSRL major upgrades. Only after these came ALS operation (*Birgeneau-Shen Report 1997*, pp 9-10, 119-121).

As one of the clearest priorities of the 1984 *Eisenberger-Knotek Report*, the ALS had been funded in full by the DOE (unlike for example the APS) and by 1997 it had been in operation for only four years. Still the priorities of the *Birgeneau-Shen Report* placed ALS regular operations budget below even possible investments at other labs. According to the co-chair of the committee, Zhi-Xun Shen, the low ranking of ALS was based on the relatively low return-for-investment: “The facility’s \$33 million operating budget is

50% larger than that of SSRL, but it has fewer than half the users” (quoted in Service 1997, p 377). The Birgeneau-Shen report is said to have “stunned ALS officials, who had expected that their \$100 million machine would sail through the review” (Malakoff 2000, p 1733). The absence of praise of the sort given NSLS and SSRL can be interpreted as criticism towards ALS with regard to productivity and management (Ibid.). Another interpretation holds that the committee was faced with a clear choice to prioritize either hard or soft x-rays, and that their response to a situation of tight competition for money was that the priorities ought to be operation and upgrades of the existing hard x-ray sources rather than continuing support for a laboratory focused on soft x-rays (Service 1999, p 1344), a priority that also would be easier with ALS performing below the expected.

The immediate outcome of the report was that the Department of Energy cut the ALS budget by 10% “within weeks” and “postponed some proposed upgrades”, and so the laboratory continued to operate on DOE support but with clear incentives to change and improve (Malakoff 2000, p 1733). A follow-up committee put in place in 2000, to assess whether the laboratory had responded to the criticisms of 1997, found that the lab had improved on nearly all accounts and suggested “that the penalty imposed on the ALS in light of the Birgeneau report be lifted” (*BESAC ALS Report 2000*, pp 8, 19).

Interestingly, there are indications that the Department of Energy expected the review to suggest a closing of SSRL, because of its age if not for any other reason. With two large investments recently made, the DOE wanted a streamlining of its synchrotron radiation user facilities, and expected the SSRL, running on an outdated though partially upgraded ring, to be placed lower down in the priority list and thus be eligible for phasing-out. The report suggested the opposite, giving SSRL the highest and the ALS the lowest ratings respectively for user support. With the ALS just been recently built, for \$100 million, the DOE would not likely shut it down, but it was clear from the review that SSRL was a well-functioning laboratory with a thriving user community that needed more support rather than closure (*Arthur 1, Lindau 2, Lubell, Richter interviews*). This result had some broader ramifications that will be further discussed below.

The SSRL organizational model

The synchrotron radiation program at SLAC was one of the first with an outspoken ‘user facility’ strategy and it did immediately after its inception reach out to user groups across the country. Though it was never at any

point defined and created in full as a synchrotron radiation laboratory but evolved into it step by step, SSRL eventually took over SPEAR and managed to become a dedicated synchrotron radiation laboratory in its own right after twenty years of parasitic operation, some years later also upgrading SPEAR and evolving into a third generation synchrotron radiation laboratory. There are still traces of this history at SSRL, with some unconventional practices and policies built into the organization. It shows particularly in the general organizational structure, in the comparably low funding level (in DOE comparison), and the dual focus on serving external users and maintain inhouse innovative capability, manifested through its special staff policy for inhouse scientists, which will be discussed later.

Already in the 1997 *Birgeneau-Shen Report*, it was concluded that the NSLS and SSRL had lower funding profiles than ALS and APS, also when weighed against number of beamlines operated and number of users served (*Birgeneau-Shen Report 1997*, pp 97, 106). It is a rule of thumb for user facilities within the National Laboratory system that the annual operational budget is leveled at a certain percentage of the total construction costs (*Hedman interview*), but SSRL was never ‘constructed’ in that sense – SPEAR was never built or completely rebuilt as a dedicated synchrotron radiation source¹⁰³ and a whole set of beamlines have never been attached to the ring during a dedicated construction and commissioning period. Therefore, the SSRL operations budget has been continuously determined as the lab has grown, instead of once determined with reference to a clear investment, which would probably have placed it closer to those of ALS or APS (*Hedman interview*).

The lower funding level has contributed to the preserving of an efficient organization structure at SSRL, but also partial insufficiency on the user and technical support side. Furthermore, it is argued that the SPEAR3 upgrade necessitated a substantial increase of the operating budget of SSRL to secure and maximize return for investment (*Hedman, Lindau 2, Pianetta interviews*). Consequently, a decision by the DOE in 2006 to adjust the levels of funding of its four synchrotron radiation facilities had the result that the suggested SSRL operating budget for the fiscal year 2007 was increased with 40% compared to previous years. This suggestion was made part of the DOE budget proposal and scheduled to be incorporated in the federal budget, but a so-called ‘continuing resolution’¹⁰⁴ in Congress delayed it. The calculated

103. The SPEAR3 upgrade of 1999–2003, see above, certainly meant a reconstruction of the SPEAR ring, but the total project costs were merely \$58 million, compared to for example the ALS that cost over \$100 million back in 1993 and APS, which in total amounted to \$812 million (*Birgeneau-Shen Report 1997*, p 114; *SPEAR3 Close-Out Report 2004*, p 43; Service 1997, p 377).

104. The ‘President’s budget’, including suggestions of all federal spending, needs to be approved

effect of the budget increase was judged by SSRL management to take the laboratory to acceptable levels:

“It would basically have taken us halfway to having sort of a coverage level that we deem is adequate, that we deem is being something that is really sustainable. And then the following year there was another scheduled increase that would have taken us pretty close to where we wanted to be, from both the operating point of view and the capital point of view” (*Pianetta interview*).

The historically small operations budget is one reported reason for the special organizational arrangements of SSRL that shows primarily in the structure of scientific planning, technical support and user support. Due in part to the heavy reliance on Participant Research Teams solutions at APS and to some extent NSLS, but also depending on the general strategy for funding and execution of construction plans, the three other DOE funded synchrotron radiation laboratories have a far more compartmentalized organizational structure than SSRL (*Hedman, Lindau 2, Pianetta interviews*). The academic roots of SSRL, its gradual development from a small scale project to a full scale user facility, and the comparable shortage of funds, has created a different model. SSRL is more centralized in planning, and a greater degree of standardization of detector and computer systems and a more fluid organization on the technical support side allows for mobilization of ‘task forces’ to solve problems as they emerge. The claim by SSRL management is that at facilities with a compartmentalized organization of technical and user support, where every beamline or small group of beamlines has its own computer systems and its own technical staff, collaboration between them can be significantly lower than at SSRL (*Hedman interview*). The SSRL staff is ‘mobile over the board’ because beamlines and experimental stations have been constructed with a clear strategy to implement the same solutions regarding hardware and software as far as possible (*Hedman interview; Birgeneau-Shen Report 1997, p 100*). It is claimed that “the only way for SSRL to survive” on its small budget

by both houses of congress to enter into force. If no such budget agreement is reached, a ‘continuing resolution’ is enforced, in effect resulting in a leveling of every federal expense post at a monthly rate of 1/12 of the budget’s expense the previous year. After the Democratic takeover of congress in the 2006 general election, the FY 2007 budget under way “fell apart entirely”, and instead of trying to rescue it, Congress decided to go for a continuing resolution (*Chang 2008*). It should perhaps be emphasized that the DOE expenditure, including the increase of the SSRL budget, was not part of the explicit reasons for Democratic rejection of the budget (*Lindau 1, Stöhr interviews*), and that the continuing resolution only delayed the budget increase, not disallowed it.

has been to organize the facility this way (*Hedman interview*). According to SSRL management, the DOE has acknowledged the advantages of the SSRL organizational model, including its comprehensive model for user support and limited presence of PRT arrangements, and are presently seeking to implement such features also at its other synchrotron radiation facilities (*Hedman, Lindau 2, Stöhr interviews*).

The central SSRL laboratory management consists of one director and two deputies, between whom responsibilities are divided essentially according to the separation of structural biology and 'non-structural biology'. This quasi-disciplinary division regards primarily the planning of the scientific programs, including operations of beamlines and user support (*Hedman, Pianetta interviews*), but it is also due to the division of the funding of the facility. The main operational budget for SSRL comes from the Department of Energy, which also funds most of the 'non-structural biology' side. The structural biology branch on the other hand has a so-called 'synergistic funding and program model', i.e. the funding is shared by the DOE, the National Institute of Health (NIH) and a small part from industry (*Hedman interview; SSRL Strategic Plan 2007*, p 7).

SSRL and the 'user facility' concept

The general impression given by SSRL management and staff scientists, and to some extent also expressed in reports and other printed material, is that SSRL throughout its history has been capable of maintaining a strong user focus while at the same time making innovative contributions to the developments in synchrotron radiation. Some of the allegedly most significant innovations have been mentioned: the development and first use of insertion devices and the early EXAFS and crystallography accomplishments were pioneering ventures with big eventual impact. The stated user focus is not as straightforwardly confirmed in the material.

In the 70s, external users were present at SSRL primarily through the Participating Research Teams, taking part in design, construction and maintenance of beamlines and being in charge of the experiments. The users were therefore almost exclusively synchrotron radiation specialists in control of a whole experimental setup, and their patience with instabilities, x-ray droughts and the general volatility of living as second class citizens at SLAC was important. Only after the full dedication of the SPEAR accelerator to synchrotron radiation in the early 90s was SSRL able to make use of its full potential as user facility – with secure and stable scheduling, full control over

operations, and complete responsibility for upgrades and refurbishments. The staff and user community that was slowly established at SSRL was heavily influenced by this gradual development. The position as parasites forced the SSRL staff (and users) to adapt to not always knowing whether the next scheduled block of beamtime would be available (*Hedman interview*) and this may have helped in creating a practice among staff to make the absolute most out of every minute and constantly search out new opportunities to secure good data. The response of a potential user community to such circumstances may be stratification; the prospects of convincing users who expect turnkey operation of the instruments they use that they would gain from traveling to an accelerator laboratory with technical instability and unsecure availability are discouraging. On the other hand, user groups committed to technically demanding work or more technology-oriented in their experimental activities may search out the opportunities regardless of mentioned instabilities and limited availability. The potential users with greater inclination to take risks and search out experimental opportunities in the early days did not represent as broad a scientific community as the ones that started to organize themselves around third generation synchrotron radiation sources a few years later. It is therefore reasonable to suggest that the history of SSRL as a general user facility – organized and optimized to enable for external research groups to make use of the experimental facilities in peer review competition – is significantly shorter and started in the 90s rather than the 70s. There are also signs that the stratification of the user community has prevailed also as the laboratory developed into a broader user facility with streamlined operation. The 1989 *Cardillo Report* identified a “clubbiness or elitism” at SSRL that posed a threat of “shutting out new users with novel and valuable contributions to make” (*Cardillo Report 1989*, p 27), and though it comments on the state of things in 1989, there are indications suggestive of similar patterns today. It is reasonable to expect the greatly enhanced overall laboratory performance achieved through the dedication of SPEAR in the 90s and the SPEAR3 upgrade in the early 2000s to have led to an increase in the demand for beamtime similar to that of other synchrotron radiation facilities, so that oversubscription rates would be around or above 2. The overall oversubscription rate at SSRL is about 1.4 (*Knotts interview*), which is comparably low given the repeated claims that the user support and scientific capabilities of SSRL are so prominent (*SSRL Strategic Plan 2007*, p 1; *Hedman, Stöhr interviews*). In the SSRL beamtime allocation process, which in some respects is unusual and will be presented in detail below, “almost everyone” who applies is awarded beamtime (*Knotts interview*). This is made possible by giving the highest rated proposals the amount of time requested, while giving the lower-rated shorter slots or relocating them to other beamlines, perhaps second or

third of their choice, where the demand is not as high (*Knotts interview*). One possible interpretation is that instead of raising the oversubscription rate, the long history of high quality proposals has repelled the broader community and caused limited turnover in the user community.

The SSRL beamtime allocation procedure differs somewhat from the practice at most synchrotron radiation laboratories.¹⁰⁵ One significant feature is that beamtime proposal handling is divided into the categories 'crystallography' and 'non-crystallography'. For crystallography, there is a call for proposals three times a year, whereas the non-crystallography ('VUV and x-ray' hereafter¹⁰⁶) call for proposals is twice a year. The foremost reason is the higher demand for frequent and rapid access in crystallography. For the VUV and x-ray proposals, there is a two-step procedure for evaluation and beamtime allocation; proposals are sent out to external referees for peer review apart from being judged by an ordinary proposal review panel. When submitting a proposal the experimenter can suggest two or three reviewers, and also indicate if there is a possible conflict of interest disqualifying some reviewers. On top of this, the ordinary SSRL proposal review panel convenes and recommends three additional reviewers for each proposal, so that every proposal is sent out to six external referees for review, who send their evaluation and comments back. The panel then meets again and finalizes the rating and ranking of the proposals, almost always arriving at a rating for each proposal that corresponds to the average rating of the six reviewers. The grades given by the reviewers are in two categories, overall science and overall competence of the investigators, on a grade scale with five steps. The reviewers are asked to give a justification for their rating. A very rare feature of this process, compared to other labs, is the opportunity provided the applicant to respond to the rating and evaluation given by the reviewers. Since crystallography proposals need a faster handling, they are not subjected to this very time-consuming external review process but instead handled only by a proposal review panel.

Another interesting feature of this external review process is the occurrence of radically disparate ratings received by reviewers. Beamtime applications that propose truly pioneering work may be rewarded with very high grades by some reviewers, whereas others may well dismiss them as not fea-

105. This paragraph is in its entirety based on an interview with the head of the SSRL users administration, Cathy Knotts (*Knotts interview*).

106. In the beamtime schedules and in common language, SSRL uses the categorization 'crystallography', 'VUV' and 'x-ray' for their user groups. Thus the 'non-crystallography' group will hereafter be called 'VUV and x-ray', although the labels are inconsistent with scientific and technological meaning of the words (for one thing, crystallography makes use of x-rays). However, repeatedly using 'non-crystallography' as a label is also incorrect and may give the erroneous impression that crystallography is somehow a model or original technique or method.

sible and give an overall low rating. The advantages and drawbacks with this semi-external review system – foremost issues of transparency and bias – are especially visible against the background of the claimed overall high quality of proposals. There are reportedly very few proposals that get rated so low that SSRL wouldn't want to grant them beamtime, in fact the head of the user administration "cannot recall" ever having gotten any proposals back from the external reviewers with poor overall rating. However, it does happen that individual reviewers give very low grades that stand out among otherwise good ratings, suggesting that personality could play in: "Maybe they just don't like the science that individual does" (*Knotts interview*).

A popular service provided to crystallographers, for example used by the team in the measurements that contributed to the work that gave Roger Kornberg the Nobel Prize in chemistry 2006,¹⁰⁷ is 'remote access crystallography'. It is not a mail order service as the one provided by for example ESRF (see chapter 6) but a remote control service made possible by the implementation of sophisticated robotics at the crystallography beamlines. The investigator can deposit a cassette with samples at the beamline, which is then mounted in the robot that does the sample switching. Switching of samples, data storage, and interpretation is remotely controlled through a computer user interface that the user can log on to from any location of their choice, and thus nobody has to be at the beamline to do crystallographic measurements. This system has been implemented at all the crystallography beamlines at SSRL, and the remote control users amount to approximately 10% of the total number of users (*Knotts interview*).

The staff scientists

The declared SSRL ambition to keep both a strong user focus and an innovative strength has an incarnation in the policy regarding the scientific personnel at SSRL, the staff scientists. They are called the laboratory's main asset, the 'blood' of SSRL (*Hedman, Stöhr interviews*), and named "the single most important group of actors" in the lab (*Lindau 2 interview*). The staff scientists are supposed to divide their time and effort between user support (as laboratory service work) on one half and the carrying out of their own research projects (as contribution to the inhouse scientific program) on the other. The

107. The 2006 Nobel Prize in chemistry was awarded to Stanford University professor Roger Kornberg "for his studies of the molecular basis of eukaryotic transcription" (*Nobel Foundation website*), work that included the mapping and analysis of a large number of proteins by crystallography, mainly carried out at SSRL (*Brennan, Kornberg interviews*).

official line is that staff scientists are employed to “deliver value to the organization” (*Stöhr interview*), and taking care of outside users and assisting them in their experimental work on the beamlines is obviously part of this. But “deliver value” has a wider definition:

“Value to this organization can come in many different ways. It can come in novel ideas, doing outstanding research and publishing in *Nature* and *Science* and *Phys Rev Letters*. It can come in not doing experiments at all, but coming from instrumentation development. It could come from saying ‘I’m going to take off two years and develop this instrument and after I’m done building this instrument it will benefit a lot of other people.’ It can come by in a given year saying ‘This year I know that we have a shortage of scientists on the floor that can help our users effectively, and this year I will not do research on my own but I will dedicate my time to make this user group successful’” (*Stöhr interview*).

This policy of time and effort divided in equal shares is a model with exceptions, individually adjusted and coupled with annual evaluation. The staff scientists “basically have to earn their freedom” (*Stöhr interview*) and show that they deserve to have 50% of their time to research of their own choice:

“If you do very well in your own research and we see that a given person really has outstanding results and outstanding publications, then there are no questions asked, that person has 50% of their time to do research, because they earned it in terms of their scientific accomplishment. If we see that a person really produces mediocre results, that are of no scientific value, then we will talk to that person [...] and say we don’t see that you are exceeding in this area, but on the other hand that person might do extremely well in user support, so we say we think it would be to your benefit if you do more user support. You’d do better in this organization” (*Stöhr interview*).

The annual evaluation of the staff scientists and their work is based on their own reported plans for a coming year. Thus it is largely a “self-regulating system” in which all tasks are attended to, but flexibility allows the staff scientists to choose and to a large extent be in charge of their own time (*Stöhr interview*). The strategy is regarded by one of the deputy directors as a convenient way of running a laboratory organization: “For me it’s easier to manage somebody who is doing their 50% facility support because they want to, those are the dos that they pay to be able to do their own research” (*Pianetta inter-*

view). The aim is also that staff scientists as far as possible should let user support and their own research overlap, and it is the laboratory management's opinion that the best staff scientists are the ones who manage to make it overlap entirely (*Pianetta interview*). A peculiar feature of this arrangement is the freedom given the staff scientists on the time they have for their own research. SSRL staff scientists are actively encouraged by their managers to search out new scientific and technical opportunities, even if that includes work at other labs and thereby the investing of time and effort in experimental setups at other synchrotron radiation facilities:

"It's unusual and you will not find that on other labs, they are much more protective and they have the argument that they pay for them, so they are staff scientists, so they need to work here. I have a very different attitude, because I know that first of all, I want my scientists to grow in their knowledge because ultimately that will help our lab. And part of that growth comes from actually experiencing the outside world. So they need to understand what facilities are being built at other synchrotron radiation labs, what are the forefront areas that are pursued there? What is possible there that is not possible at this facility? And when they use those for their own research at other labs, they will naturally be lead to saying 'next time, I don't want to travel to Argonne, I want to create this at SSRL.' And therefore they will help me build new facilities so that these experiments can then be done here. So to me, this is a much better way" (*Stöhr interview*).

The policy is reportedly sanctioned by advisory committees and Department of Energy administrators (*Stöhr interview*). A practical mechanism, suggesting a will to increase competitiveness among staff scientists, is that no beamtime is awarded directly to them outside the regular allocation process. Though entitled to priority beamtime amounting to 60 days, so-called 'staff time', such time requires a regular application and does only render some priority in the process, and no complete shortcutting of the system (*Knotts interview*). Even the top laboratory managers regularly have active proposals and scheduled beamtime, including the director and the two deputy directors as well as Keith Hodgson who is now director of the 'Photon Science' program at SLAC (see below) (*Knotts interview*).

The reasons for this special treatment of the staff scientists is reportedly that SSRL is more 'academically oriented' than other labs, due to its close ties to Stanford University (*Lindau 2, Stöhr interviews*), and the history of inhouse scientific achievement (*Hedman interview*). According to some, the freedom of staff scientists to pursue their own research programs has created a special

‘working spirit’ at SSRL, noted by the user administration and through the end of run summary forms that every regular user is supposed to fill out when they leave. This ‘spirit’ is reportedly manifested in a willingness to assist users also at inconvenient times and to work overtime on projects to get things ready in time (*Knotts, Lindau 2 interviews*). The willingness to maintain the ‘spirit’ has made it a component in the recruitment process for new staff, not only for staff scientists but also other positions. Attention is regularly paid to the personality of applicants and their willingness to work overtime and during times contribute extra time and effort (*Lindau 2 interview*). A reasonable suggestion is that the decades of parasitic operation at SPEAR also helped in creating this ‘spirit’, as a ‘survival instinct’ strategy and philosophy, because of all uncertainties and instabilities that threatened to ruin long-term plans.

A free electron laser in the linac

The various events in the 90s, drastically changing the positions of high energy physics with regard to size and scope of projects and the future of the field (see chapter 2), had some particular implications for SLAC.

The SLAC Linear Collider (SLC) ran with a predicted completion of its experimental program in the late 90s, and its sequel PEP-II (an upgrade of PEP) was on the drawing board and the projected centerpiece of the experimental program of the early 2000s. But the long-term utilization of the SLAC site with its infrastructure and manpower was under questioning, not least since the emerging plans for the next large high energy physics machine, the Next Linear Collider (NLC hereafter), clearly showed that it would not be possible to contain at the SLAC site, regardless of who got the main scientific and technical responsibility for it (*Arthur 1, Richter interviews*). The synchrotron radiation program at SLAC had gained momentum and had gotten SPEAR fully dedicated in 1992, and construction of the Advanced Photon Source (APS) at Argonne, the largest synchrotron radiation source in the world so far, was under way along with counterparts in Europe and Japan, firmly declaring that synchrotron radiation sources were large scale infrastructure projects in their own right and that the parasitic days were over.

In 1992, the first of three workshops on ‘fourth generation light sources’ took place at SLAC (*LCLS DSR 1998*, p vi). One of the early proponents of the free electron laser concept (see chapter 3), Claudio Pellegrini,¹⁰⁸ contrib-

108. Pellegrini published articles on the free electron laser concept already in the late 70s (Pellegrini 1979, 1980), and continued to do so during the 80s, establishing himself as an authority in the field.

uted by sharing with the workshop participants his idea to build a large free electron laser using the SLAC linac. A number of interested people at SLAC formed a group together with Pellegrini to work out a tentative design concept for the machine, which was given the name the *Linac Coherent Light Source (LCLS)*, and applied for a grant from the DOE to support their work (*Arthur 1 interview*). The application asserted that a future free electron laser “would open a new experimental regime for many areas of research in physics, chemistry and biology” and provide “a revolutionary tool for scientific research” (*LCLS initial proposal 1992*, pp 6, 10). An early review of the plans gave a positive response but emphasized the need for extensive studies in order to determine technical feasibility and projected scientific utilization. While at the same time probing an entirely new field, not least technologically, the prospects of significant scientific breakthrough were judged promising. The review stated “emphatically” that “important scientific opportunities may be missed” if the project group would not be given opportunities for future studies and design (*LCLS TRR 1992*, pp 1-3).

The political prospects of turning the SLAC linac into a free electron laser were just as uncertain – if not more. While enthusiastically probing the scientific and technical terrains, the LCLS advocates were aware that in the long run, their idea would have to compete with the main SLAC high energy physics program, not only locally on site, but in the federal budget and National Labs strategy. Thus the LCLS was not put forward as a concrete plan to take over the SLAC linac but rather as a speculative and imaginary concept of possible future utility of the facilities on site. From the scientific perspective and among prospective users, going from storage rings to linac-based light sources was not the obvious progression, and the reaction was cautious with regard to the scientific utilization (*Arthur 1 interview*). The challenge provoked more enthusiasm among accelerator physicists, who took the initiative during the first two years of LCLS planning. The growing need for a more developed scientific case led to a workshop at SLAC in 1994, at which a relatively broad international interest as well as increased support at SLAC was brandished and a team of people from a number of US institutions was put together to start working on a serious proposal (*LCLS DSR 1998*, p vi; *Arthur 1 interview*). At another international workshop on fourth generation light sources, held at ESRF in Grenoble in January 1996, the LCLS project was presented with “some real parameters associated with it” (*Arthur 1 interview*) and it “convinced nearly all the participants” that the next generation light sources were going to be linac based free electron lasers (*LCLS CDR 2002*, pp 3.2-3.3; *LCLS DSR 1998*, p vi). Among the workshop’s ‘key players’ was David Moncton, director of the Advanced Photon Source (APS) at Argonne which had recently opened, and his attitude change illustrates the importance of this

1996 Grenoble workshop for establishing credibility for free electron lasers within the community:

“So he [Moncton] wasn’t really interested in hearing about the next generation of machines, things that would make his not the best in the world anymore, and he is a very forceful personality so he came with lots of objections. But over the course of a couple of days, he completely changed his mind and he said ‘you know, this is going to be the next big machine’. And so rather than think of it as a competition to his APS he started to think of it as an opportunity, like ‘this is the next machine that I can build.’ And so he went from being a critic to being a booster of the whole concept of free electron lasers.” (*Arthur 1 interview*)

Though the project was still limited to a “demonstration machine” with a total budget of only \$100 million¹⁰⁹ (*Arthur 1 interview*), the political prospects of realizing the LCLS plans were doubtful. Having only recently finished building two light sources (the ALS and APS) that in their respective realms were among the world-leading, the DOE apparently had no room for new light sources in their long term plans, thinking rather that they had “oversaturated the market” (*Arthur 1 interview*). The 1997 *Birgeneau-Shen Report* not only changed drastically the priorities for the existing DOE synchrotron radiation sources but also strongly endorsed the concept of an x-ray free electron laser as the next (fourth) generation light source, discussing the LCLS and related plans extensively (*Birgeneau-Shen Report 1997*, pp 91-95) and concluding:

“It appears likely that ‘fourth generation’ x-ray sources will be based on the free electron laser concept. [...] It is our strong view that exploratory research on fourth generation x-ray sources must be carried out and we give this item very high priority” (*Birgeneau-Shen Report 1997*, p 118).

The DOE decided to convene a follow-up panel on “Novel Coherent Light Sources” to assemble more knowledge and input on the concepts to guide further action by the agency. The report of the panel was issued in 1999 and gave general recommendations on future directions for new light source concepts as well as a clear endorsement of the LCLS project to be realized at SLAC (*Leone Report 1999*, pp 19-20).

Commentators regard the positive signals given in these reports the real watershed events for the LCLS. Before it, the scientific advice gathered by

109. For comparison, the final LCLS construction budget amounts to over \$400 million (Woods 2006, p 12).

the Department of Energy suggested that the project was only an “awfully expensive” way to achieve something that ordinary lasers soon would provide (*Galayda interview*), but with the comprehensive Birgeneau-Shen assessment another view emerged, strongly in favor of the idea, and the DOE “had no choice” but to move forward and grant money for further LCLS design studies (*Arthur 1 interview*). A ‘conceptual design’ was finalized in 2002, encompassing both a technical design of the facility and details on five experimental areas chosen by the LCLS Scientific Advisory Committee and planned by a large number of scientists from the US, Canada and Europe¹¹⁰ (*LCLS CDR 2002*). On basis of this, the DOE in 2004 decided to fund the LCLS project with \$400 million. The decision was foregone with the inclusion of the LCLS in the *DOE Office of Science Strategic Plan* (issued in February 2004) as a facility initiative with high priority (*DOE Office Of Science Strategic Plan 2004*, p 15; *DOE Facilities 20-Year Outlook 2003*, p 13). The LCLS director of construction points at the fact that the DOE didn’t fund the LCLS until it was conceptually developed into a general user facility:

“In 2002, I was told by the head of the [DOE] Office of Science, Ray Orbach, that he was not going to spend the amount of money we were asking for to just build a simple test of an x-ray free electron laser, he wanted to know he was making a good long term investment” (*Galayda interview*).

Obviously, SLAC had the advantage of a very long linac already on site, at the time only used for injection into PEP-II,¹¹¹ as SLC had been taken out of operation in 1998 (Deken 2002, p 105). The estimates are that “hundreds of millions of dollars” are saved this way, not to mention the advantages of having a 40-year history of forefront accelerator physics to build on (Woods 2006, p 12). The obstacles or potential controversies connected with outgrowing the high energy physics program at SLAC are, however, of a different kind.

110. The experiment categories were Atomic Physics Experiments; Plasma and Warm Dense Matter Studies; Structural Studies on Single Particles and Biomolecules; Femtochemistry; and Studies of Nanoscale Dynamics in Condensed Matter Physics. The report bore the names of 43 scientists from 22 institutions in 9 countries (*LCLS First Experiments Report 2000*, pp 1, 13, 35, 63, 85).

111. The LCLS makes use of only the last third of the linac, as the first two thirds were used for injection to PEP-II (*LCLS DSR 1998*, p iv). Since the close-down of the experimental high energy physics program of PEP-II in 2008, plans have been made to make use of the full linac for future expansions of LCLS (*BESAC 20-year roadmap*, p 12). However, the PEP-X proposal, suggesting the conversion of PEP-II into a light source, is also on the table as a future possibility (see further below).

The transition of SLAC

In 2004, when the LCLS project got passed by congress and construction started, SLAC had already founded its 'photon science' division and publicly redefined its laboratory mission to a dual of 'photon science' and 'particle physics and astrophysics'. Although during the decades since SSRP started its activities at SPEAR in 1972 the synchrotron radiation program at SLAC has only been growing, SLAC was in practice a single-mission National Laboratory for high energy physics as long as an experimental high energy physics program ran on an accelerator on the SLAC site. Right from the start, the LCLS plans have been based on the idea to take over part of the SLAC linac, but there have been high energy and accelerator physicists among the SLAC staff believing all along that there would be a next big project for SLAC to host and to commit to (*Lindau 1 interview*). The realization that this was not the case has come gradually. According to many analysts, it had become clear from the late 90s and on that SLAC would have to change direction and move into other territories, in one way or the other. The NLC was too big for SLAC to host, and the lab would need a new mission (*Galayda, Richter interviews*).

SLAC had been running experimental high energy physics programs on their own machines uninterruptedly since the original linac was taken into operation in 1966 (see table 4). As noted earlier in this chapter and before, the US National Laboratory system and its steward agency the Department of Energy has a (unspoken) strategy of always providing every lab with a main mission, either in the form of a large infrastructure project or an assembly of several smaller undertakings, or in some cases both. When it was realized that the next high energy physics machine would surpass the physical (and perhaps organizational) capacity of SLAC, the DOE would most certainly embark on another major infrastructure initiative as the lab's new mission (*Richter interview*, similar reasoning: *Arthur 1, Galayda, Lindau 2 interviews*). With repeated indications that fourth generation light sources would be free electron lasers and that these were technologically feasible but highly sophisticated construction projects, the accelerator tradition at SLAC – a core competence of the laboratory – needed not be discontinued (and thus wasted) but could carry on, only switching foot from one utilization to another¹¹² (*Hodgson, Richter interviews*).

112. An interesting political implication of this would be that the synchrotron radiation branch of the National Laboratory System, and thereby the DOE Office of Basic Energy Sciences (BES), would be inclined to take over much of the responsibility for accelerator development from the high energy physics programs and the Office of High Energy Physics: "It is to get the SR people to recognize that they have to assume a much bigger role in funding advanced accelerator R&D.

Since the spring of 2008, when the last high energy physics experiment on PEP-II was closed down, the transition of SLAC has entered full speed. Although SLAC accelerator physicists continue to be heavily involved in design work for the International Linear Collider¹¹³ and the particle physics division is extensively involved in experiments at CERN, no experimental high energy physics program runs at SLAC anymore. Therefore, with regard to experimental science and user facilities, the lab is nowadays dominated by synchrotron radiation and its emerging parent field 'photon science'. This transition is not entirely unproblematic, and there is some uneasiness among both high energy and accelerator physicists at SLAC (*Hodgson, Lindau 2, Richter interviews*). Unintentionally, the closing of the last high energy physics experiment became an extra painful reminder of the DOE's changed priorities for SLAC, as it was scheduled for 2009 but executed earlier due to the 'continuing resolution' funding shortages.¹¹⁴

Accepting the fact that SLAC is abandoning high energy physics after forty years of active participation and several prominent contributions to the field's development would have been hard also without this premature closing of PEP-II. Although some "could see the handwriting on the wall" (*Richter interview*) in the mid 90s and the combined development in high energy physics and 'photon science' was and is still obvious and evident, "there are people who still don't believe this is true, they think there will be another big project" (*Lindau 1 interview*). The image and identity of SLAC have always been almost entirely the experimental high energy physics program, although the synchrotron radiation program has been successful in its pursuit and strongly contributed to the overall scientific performance of SLAC ("Together, synchrotron radiation and high energy physics, that is quite an impressive show

Now if you look at Washington, at BES, they have not funded significant advanced accelerator R&D. But now they recognize that they are going to have to begin to take on that. [...] I don't want to exaggerate, but BES is going to have to begin to put money into this" (*Richter interview*). So far, the LCLS is the only new infrastructure project initiated after this became the political reality, and therefore it is too early to evaluate how the DOE manages the change.

113. The linear collider concept 'ILC' – International Linear Collider, is under development through a joint effort between the European, Japanese and US high energy physics communities, based on three linear collider concepts, the 'JLC' ('Japanese Linear Collider'), the 'NLC' ('Next Linear Collider', USA) and 'TESLA' ('Teraelectronvolt Energy Superconducting Linear Accelerator', Europe). So far, no decisions whatsoever have been reached, but several meetings and conferences have been held, resulting in thick technical reports. The present design is a linear accelerator between 30 and 50 km long (Clements 2006, pp 11–12).

114. SLAC was in fact hurt in many respects by the continuing resolutions for the fiscal years 2007 and 2008. Not only was the last high energy physics experiment forced to close ahead of schedule and the 40% budget increase for SSRL delayed (see note 104); the construction of LCLS was also partly halted because of the first continuing resolution (Broad 2007), and the second forced SLAC to lay off 120 people (out of a total staff of about 1600) (Chui 2008, p 8, Chang 2008).

	Opened	Closed
Original linac	1966	1974
SPEAR	1972	1984
PEP	1980	1994
SLC	1989	1998
PEP-II	1998	2008

Table 4: SLAC high energy physics accelerators, 1966-2008. The years denote start and end of the experimental programs on the machines, not construction starts (Deken 2002, pp 15, 33, 79, 93, 105).

scientifically”, *Bienenstock interview*). Ongoing discussions about whether to extend the mission and activities of SLAC to synchrotron radiation or other fields did not result in any organizational changes until 1992, when the dedication of SPEAR made the synchrotron radiation laboratory a division within SLAC, and it wasn’t until the LCLS plans became feasible and probable that SLAC became a real dual- or multi-mission laboratory. The single-mission lab structure had been persistent through the decades, partly because of the first director’s conviction, which was influential (“I felt SLAC being a single-function laboratory was a source of strength”, Panofsky 2007, p 126). Today, the public message is that SLAC is a dual-mission National Laboratory for “photon science and non-accelerator-based particle physics” (*DOE National Labs Brochure* 2008, p 21). The term ‘photon science’ is unique among the National Labs; SLAC is the only one having ‘photon science’ among its core programs, as presented by the DOE.¹¹⁵

Clearly, though SLAC is moving into something entitled ‘photon science’, there is a whole new multidisciplinary touch to the future directions. The developments at SSRL and the prospects of opening new experimental opportunities with LCLS led the Nobel Laureate in chemistry 2006 Roger Kornberg to conclude that “SLAC has evolved from a physics laboratory to a biology laboratory” (*Kornberg interview*). ‘Photon science’ is neither a discipline nor simply a buzzword at SLAC, but the generic term for a range of new or re-shaped scientific activities that are supposed to be an important constituent in the future National Laboratory. To ensure appropriate and fruitful utilization of the user facilities the LCLS and SSRL, the ‘Photon Science Division’ was formed in 2005 as an additional branch of the laboratory organization

115. These core programs are presented as the laboratories’ “business lines” in a report on the DOE plans for the National Labs for the coming four years, requested by the US House of Representatives Committee on Appropriations. The other three National Labs with synchrotron radiation facilities have “business lines” like “materials science”, “advanced biosciences”, “energy and environmental science and technology”, “basic energy sciences”, “science for a secure and globally sustainable energy future”, “leading facilities in VUV, soft x-rays, and ultrafast science”, “develop novel materials and nanodevices”, i.e. several programs closely associated with synchrotron radiation facilities and science, but the term “photon science” is only used for SLAC (*DOE Laboratory Plans* 2007, pp 12-14, 22-23, 45-47, 87-88).

(Seife 2005, p 1393). This division's primary mission and responsibility is to establish and run institutes (or 'centers' as they are called), interdisciplinary and research based. The work of these centers is entirely external to the running of the experimental facilities and instead focused on creating and maintaining scientific programs of multidisciplinary character, similar to the ones that the original multi-mission US National Laboratories were once created to run (see chapter 2). At the end of 2008, two such centers had been created at SLAC, with involvement from Stanford University departments (*Hodgson interview*).

Several other initiatives complement the picture of SLAC as a laboratory in transition. In October 2008 the laboratory's name was changed to *SLAC National Accelerator Laboratory*, with 'SLAC' no longer an abbreviation. The willingness to acknowledge the laboratory's past accomplishments made the Department of Energy keep the word 'SLAC' in the name (Cho 2008b, p 515) and 'National Accelerator Laboratory' can be interpreted as a diplomatic compromise between the photon science program (which is accelerator-based) and the remaining accelerator and particle physics programs that still work with accelerators, though overseas.

The establishment of a photon science program division with its own scientific programs can be seen as an appeal to make further modifications and redefinitions of the infrastructural assets on the SLAC site. When the closedown decision for the last experimental program on PEP-II came in late 2006, a task force was immediately formed to assess the prospects of using the ring as a future synchrotron radiation source, as well as to explore other possibilities of new light sources to be located at SLAC (*SSRL Strategic Plan 2007*, p 15). In June 2008, a study group released a status report on the plans to utilize the PEP ring for a light source, called PEP-X, similar to the PETRAIII modification of the PETRA ring at DESY in Hamburg (see chapter 3), only somewhat larger at the present, provisional, stadium. The overview of the plans in the PEP-X report summarize well the strategy as conceived by the SSRL and photon science directors and staff at SLAC:

"The physical size (circumference) of SPEAR3 constitutes a barrier to upgrading it to achieve significantly higher brightness, a direction increasingly driven by the need to study complex materials on the nano-scale. Therefore the longer term future of SSRL is based on the transfer of the evolving scientific programs from SPEAR3 to a higher-performing synchrotron source. To be viable, such an evolution must result in transformational new capabilities measured on an international scale. [...] [W]e envision an adiabatic transition from SPEAR3 to a future state-of-the-art storage ring: PEP-X" (*PEP-X Status Report 2008*, p 10).

With the momentum gained at SLAC for photon science, and with the experimental high energy physics program no longer occupying the accelerator infrastructure on site, one interpretation of these plans is that it would in fact be “irresponsible” not to seriously examine possibilities and opportunities as they emerge (*Lindau 2 interview*). The transition of SLAC occurs as part of a broader trend in the large scale science infrastructure landscape of the US, Europe and Japan, visible especially at SLAC and at DESY in Hamburg, Germany. The history of the German laboratory for high energy physics resembles that of SLAC as they both were single-mission labs who saw parasitic operations of their smaller storage ring facilities begin in the mid 70s and grow step by step, taking over the rings and proposing new facilities for photon science as the high energy physics programs slowly phased themselves out.

Conclusions

By the recent developments at SLAC described in the previous sections, there are enough signs of transformation and change for drawing the conclusion that the laboratory is on the verge of single-mission status again, this time for ‘photon science’. With such a future development in sight, SLAC emerges as a distinct and evident example of the trajectory or ‘career’ synchrotron radiation has had in the world of high energy physics, and which has been so important for its establishment and its development into today’s shape. It points out the importance of institutionalized high energy physics for the early emergence and growth of synchrotron radiation initiatives, the negotiations and struggles over access to accelerator infrastructure, the opportunities provided by the abandonment of machines due to upgrades and new projects, and the final shift of power balance due to structural changes in the respective fields that alters whole laboratory missions and make synchrotron radiation *mainstream*.

This conclusive observation correlates with the original intent of choosing the synchrotron radiation program at SLAC as one of the case studies in this thesis. The case gives substance to the claim that a key part in the emergence and establishment of synchrotron radiation and its institutionalization has been the strong but changing relationship with high energy physics. Through the analysis of this relationship, important conclusions can be reached regarding the changing demographics and sociology of science: Large-scale scientific infrastructures and the laboratories hosting them are switching roles

from single-mission to multi-purpose, thereby altering and enlarging their constituency and adding significantly to the sweeping changes in the dynamics of science. The National Laboratory system seems to have been a comparably 'friendly' environment for this development to take place in, both scientifically and with regard to science policy.

Within this overall conclusion and advancing of the thesis' argument, a number of smaller observations deserve attention. Synchrotron radiation at SLAC has gone from parasitic to a symbiotic relationship with high energy physics, and back again to parasitic – this time in another sense of the word, namely that of the parasite ultimately consuming its host. In this transformation, technical and institutional concepts have been invented and tried that subsequently have become mainstream in synchrotron radiation at large.

In its first instance, the synchrotron radiation program at Stanford would never have come into being at all if not the radiation produced by the SPEAR accelerator would have been made available to it *for free*. This is possible to generalize: the groups probing the possibilities in synchrotron radiation in the 60s and early 70s had no access to the necessary accelerator infrastructure other than that offered at high energy physics laboratories, and herein lies the earliest concrete example of the importance of this relationship.

Secondly, the power imbalance, most tangibly shown by the 'x-ray droughts', was decisive for technical and institutional developments in synchrotron radiation that eventually were adopted by the community in general. The implementation of the first wiggler at the end of the 70s was largely provoked by the first x-ray drought and paved the way for subsequent important developments towards high-end insertion device technology. The second x-ray drought alerted science policymakers on the insufficiencies of parasitic operation of the sources from a user perspective and had an impact on the 90s comprehensive effort to establish synchrotron radiation laboratories as *user facilities*.

The second x-ray drought also eventually led to the physical detachment of the SPEAR ring from the SLAC main linac and its full dedication as synchrotron radiation laboratory, setting the wheels in motion that ultimately changed the institutional status of SLAC by the incorporation of 'photon science' into its core program. In this context it shall also be noted that this relationship between the synchrotron radiation program and high energy physics at SLAC – and the changes it has undergone and is undergoing – provides excellent examples of the difficulties of redirecting and transforming missions, curricula and *identities* of large-scale institutions. Difficulties are largely due to the self-image of high energy physics (as discussed in earlier chapters) and the enormous size of the institution, organization and infrastructural assets

in question. Synchrotron radiation laboratories tend to emerge as extremely flexible in comparison, which seem to further strengthen their position in a changed science policy context characterized by altered demands and quicker returns.

The role of the Stanford synchrotron radiation program in defining the synchrotron radiation *user facility* concept has less direct connection with the high energy physics relationship and the institutional status of the program at SLAC. Nonetheless, in line with the supplemental motivation for the choice of SLAC as a case study, it shall also be noted that the early establishments of practices and policies with respect to external user groups were influential for the continuing process of establishing synchrotron radiation in science and science policy. The enrollment of external researchers – the *recruitment* of potential users – was a key part in the early development of the laboratory and the establishment of a user community.

These conclusions are all contained within the general conclusive observation presented above, but the deciding factors and the actor groups or institutions to which they are attributed seem to vary. At the stage preceding political involvement on broader scale, the synchrotron radiation program at SLAC was largely the work of one type of *scientific entrepreneurs* whose initiatives were well received within a laboratory context with good condition to allow for the establishment of a pilot project. At a later stage, when synchrotron radiation had established itself at SLAC and a user community had started to organize, the comparable ‘friendly’ *science policy context* showed itself and facilitated future developments including further expansion of SSRL and the construction of a second-generation facility at Brookhaven. Not only were the National Labs eminent institutional environments for the establishment of new (and costly) initiatives, but also it was a part of their intrinsic logic to be receptive to new large scale initiatives that could replace fading programs. This is shown not least in connection with the LCLS: It was politics that ultimately made SLAC go for the ‘real thing’ rather than a prototype and thereby finally take the step over to ‘photon science’.

In this chapter, the scientific and technological development and evolution in the area of synchrotron radiation science, described in chapter 3, has been put in a specific institutional and political context. Through the main theme of the chapter as described above, the co-development and interdependence of synchrotron radiation and high energy physics, an interesting feature has presented itself. The emergence and establishment of synchrotron radiation, following the pragmatic and evolutionary patterns of a small scale enterprise growing successively, has interacted with its host institution and the established order it represents in various ways through the activity’s

gradual growth. This can be interpreted as synchrotron radiation establishing itself in the nexus of two strong forces. On one hand the bottom-up, evolutionary and pragmatic scientific development and progress, searching out new opportunities as these present themselves. On the other hand the top-down, institutionalized political governing of heavy investments and long-term commitments. The Stanford case, as presented in this chapter, provides only one example of this dual power structure, and the coming two cases will shed additional light on this apparent crucial relationship.

MAX-lab – The laboratory that was never intended to be

Beginnings

The MAX laboratory, commonly referred to as MAX-lab, is a Swedish national research facility for synchrotron radiation and nuclear physics, officially inaugurated in 1987 after a decade of construction and since then continuously expanded and upgraded. It is located at the campus of the Lund Institute of Technology (Lunds Tekniska Högskola, LTH) in Lund and has a dual organizational status; it is a 'Swedish national facility', under supervision from the Swedish Research Council, and a so-called 'special entity' within Lund University,¹¹⁶ answering directly to the Office of the Vice-chancellor. Among the many incremental upgrades in the laboratory's history, the MAX II project stands out as the largest and so far most important. It was initiated in 1991 and finished in 1997, and consisted of the construction of the accelerator MAX II, a third generation storage ring entirely dedicated to synchrotron radiation (the previous MAX I ring had been, and is, shared with the nuclear physics program), with a number of simultaneous beamline and instrumentation projects.

The choice of MAX-lab as one of the case studies in this thesis was grounded on its availability for study, as well as its basic institutional status as national facility, relatively small scale, and situated in a distinct national science policy context. Its apparent evolutionary growth from small-scale university activity to a national (and international) user facility is another incarnation of the transformation from 'esoteric endeavor' to 'mainstream activity' and a potentially very interesting one.

At present, MAX-lab runs three storage rings, in total equipped with 15-20 beamlines, depending on the count.¹¹⁷ The numbering of the accelerators at MAX-lab might seem confusing, but is completely logical: they are simply

116. 'Särskild verksamhet' in Swedish; i.e. a unit outside the regular faculties organization.

117. Beamlines may be temporary out of use due to upgrades and some beamlines support many experimental stations. Counting the number of beamlines at a synchrotron radiation facility is always associated with ambiguities. However, a rough estimate can tell the approximate size of the lab, for comparison.

MAX-lab, inaugurated 1986			
Approximate number of staff, including postdocs and visiting scientists: 90			
Approximate number of annual external users: 600			
Approximate number of operational beamlines: 15			
Funded by the Swedish Research Council, Lund University, private foundations, and a number of smaller external sources.			
MAX-lab's accelerators			
Name	MAX I	MAX II	MAX III
Start of operation	1986	1995	2005
Energy	550 MeV	1.5 GeV	700 MeV
No. of active beamlines	4	9	2
Circumference	~30 m	90 m	36 m

Table 5: MAX-lab basic facts. (Forkman 2001; MAX-lab Activity Report 2007)

numbered in chronological order. The existing three accelerators are called MAX I, MAX II and MAX III. MAX II is the largest accelerator and thus the main ring at today's laboratory; MAX III is significantly smaller but newer and hence its name. Though the proposed new MAX laboratory (see below) includes both the construction of an entirely new ring, the MAX IV, and upgraded versions of the MAX II and III accelerators, it is as a whole referred to as 'MAX IV'.

The first step towards a Swedish synchrotron radiation source was taken in 1970, when the Swedish Natural Science Research Council issued a grant for the construction of a synchrotron radiation facility at the existing electron synchrotron LUSY (Lund University Synchrotron) in Lund (*NFR Working group Report 1980*, p 2) that had been taken into operation in 1962 (Forkman 2001, pp 39-41). The synchrotron radiation program carried out with LUSY studied the characteristics of the radiation without putting it to experimental use. That happened only years later, at the initiative of a physicist from Chalmers Institute of Technology (Chalmers Tekniska Högskola, CTH) in Gothenburg, Per-Olof Nilsson, who did some experiments on the accelerator with instrumentation from his home laboratory (*P-O Nilsson interview*).

The Swedish decision to join CERN II (see chapter 2) and the accompanying unavoidable refocusing of nuclear and particle physics research to Geneva resulted in the decision by the Swedish Atomic Science Research Council (Atomforskningsrådet, AFR) in 1972 to end its financial support for LUSY (Forkman 2001, pp 71-72), a decision that ended the development of the synchrotron radiation work in Lund. Words of synchrotron radiation and

its possible application had however also reached Sweden from abroad, and in 1975, Per-Olof Nilsson organized a conference in Gothenburg for Nordic researchers interested in synchrotron radiation.¹¹⁸ The twofold purpose of the conference was “to inform about present and possibly future research using synchrotron radiation” and “to have a preliminary discussion about realistic alternatives for the nordic [sic] countries to participate in such research” (*Nordic Conference Report 1975*, p 1). Invited speakers from emerging synchrotron radiation programs abroad reported about the development in their respective countries and successful experiments done at existing facilities, and representatives for the LUSY project briefed the conference about their work (*Ibid.*, pp 2-3). Most participants showed lukewarm interest, questioning the value of synchrotron radiation to their respective fields and the level of interest possible to mount in Sweden or the Nordic countries (*Ibid.*, pp 6, 8). The concrete outcome of the conference was to give Nilsson the task to do a “prestudy of the project” (*Ibid.*, p 11).

Meanwhile in Lund, the nuclear physicists had started planning for their future after the complete dismantling of LUSY. Several opportunities were evaluated, and it soon became clear that an accelerator project of an economically modest but scientifically highly interesting type, a so-called *pulse stretcher*, was an option for the future of accelerator-based nuclear physics at Lund University¹¹⁹ (Forkman 2001, pp 74-78). The project, given the name MAX¹²⁰, was granted money from the Swedish Atomic Research Council in 1976, in total 2,4 MSEK during four years. The grant secured an uninterrupted accelerator activity in Lund, since construction of the MAX accelerator could be initiated one year before the final dismantling of LUSY was completed (Forkman 2001, pp 79-81). About the same time, Mikael Eriksson, a nuclear physicist turned into accelerator physicist, arrived in Lund from

118. The conference was meticulously documented; Per-Olof Nilsson recorded all talks and discussions on tape and transcribed it into a summarizing report of the conference (*Nordic Conference Report 1975*), hence the level of detail of the reporting in these paragraphs.

119. It may seem odd or foolish that the Lund University physicists decided to initiate and try to get support for a new accelerator project right after LUSY's close-down decision. Accelerator development was however hasty enough at the time to allow for completely novel projects to surpass their predecessors' performance parameters several times while at the same time cost significantly less. A pulse stretcher is an accelerator concept designed to achieve pulses of electrons that are used in nuclear physics experiments, typically by firing the electrons at the nucleuses of elements and thereby studying their properties (Forkman 2001, pp 55, 74-79).

120. Opinions differ regarding the original meaning of this acronym. Some claim it reads “Microtron Accelerator for X-ray production” (*P-O Nilsson interview*) whereas others suspect it has something to do with the founder, Bengt Forkman, whose birthday is the 1st of February, which is the name day of Max in the Swedish almanac (*M Eriksson 1 interview*) and whose dog reportedly was called ‘Max’ (Thorin 2009, p xiii).

KTH in Stockholm and started working on the MAX project.¹²¹ According to Per-Olof Nilsson, the idea to modify the MAX accelerator and make it usable also as electron storage ring, was his and Eriksson's joint idea, and together they drew up the plans (*P-O Nilsson interview*). In January 1978, a proposal was submitted to the Natural Science Research Council (NFR) for a 444 kSEK grant to modify the MAX accelerator to enable use of the accelerator as a synchrotron radiation source. The purpose of the application was explicit: "construction of a Swedish national synchrotron radiation source" (*NFR Working Group Report 1980*, p 2; Forkman 2001, p 100; *Berggren Report 1978*, pp 1-2).

The MAX proposal was evaluated, and the report on the accelerator concept stated that "it is principally possible to use the converted MAX as a beam stretcher and a radiation source" and called the amount of money applied for "moderate compared with [sic] the proposed work" (*Husmann Report 1978*, pp 1, 3). The overall evaluation of the proposal stated that there was a Swedish demand for synchrotron radiation in "biology, atomic and molecular physics, photoemission and surface physics"¹²² (*Berggren Report 1978*, p 10) and concluded that the proposed modification of the MAX ring was cheap and cost effective – "very moderate" costs compared to corresponding facilities abroad – even though no beamlines or experimental stations were to be covered by the grant (*Ibid.*). By the work of these evaluations, the MAX project had won the necessary credibility and was granted the requested money by the research council in 1979 (Forkman 2001, p 102).

Thus in the four years from the conference in Gothenburg in 1975 to the granting of money in 1979, synchrotron radiation in Sweden had gone from almost completely unknown to modest priority of the research council. Several Swedish physicists had returned from stays abroad where synchrotron radiation projects were well underway and the development and prospects of the technique were continuously expanding, through the achievements at for example Stanford and Brookhaven, as well as in Hamburg, Daresbury and Orsay (*MAX II Evaluation 1990*, p 5). A national user base was slowly developing and the first steps towards a user facility had been taken in Lund.

121. Eriksson completed his PhD in 1976 on a thesis entitled 'Studies on a 100 MeV race-track microtron/pulse-stretcher system' (Eriksson 1976).

122. The report also concluded that an expansion of MAX wouldn't cover the whole national need for synchrotron radiation, and recognized that the hard x-ray rings were significantly more expensive than VUV and soft x-rays rings (see chapters 3 and 6). It therefore concluded that the reasonable commitment for Sweden was the VUV and soft x-ray regime, i.e. exactly what the application specified, to complement a commitment to foreign facilities for hard x-rays (*Berggren Report 1978*, p 11).

The birth of MAX-lab

LUSY had been built and run in a basement at the physics department at Lund University, and the early MAX pulse stretcher accelerator was still small enough to be squeezed in there. A modified MAX for synchrotron radiation would, however, need another home to make room for beamlines and experimental equipment. The suggestion to use an empty 'machine hall' in one of the buildings at the Lund Institute of Technology campus met resistance in local university politics, but after a few years the issue was resolved and in 1982 the MAX project could move in there (Forkman 2001, pp 106-112).

The aforementioned 1978 application to NFR had been entitled *Construction of a national synchrotron radiation source (Uppbyggnad av en nationell synkrotronljuskälla)*, and the ambition of its proponents was always, ever since the 1975 Gothenburg conference, that a synchrotron radiation laboratory in Lund (or elsewhere) should be a resource for the whole Swedish research community. Per-Olof Nilsson had been "looking for supporters" (*P-O Nilsson interview*) ever since the Gothenburg conference, and had started to coordinate national interest once the council stated its support for the MAX project.¹²³ The ambition was clear when the Lund University Vice Chancellor's Office in 1981 laid down its regulatory document for MAX-lab: The laboratory was to become a common resource for all Swedish researchers, and be open for international users (Forkman 2001, pp 112-113). In 1982, MAX-lab had made its way to the Swedish governmental budget bill, in which full support for the project was declared and the ambition to develop MAX into a Swedish national research facility was emphasized (*Ibid.*, pp 113-114). Once the half-built accelerator had been moved into the 'machine hall' (which has been its home ever since) and the organizational status of the laboratory had been determined, construction and commissioning of scientific equipment could start. The MAX accelerator stored its first electrons in March 1985 (Forkman 2001, p 134), and hosted its first experimental run with external users in 1986 (*MAX-lab Background Material 2002*, ch 2 p 4).

The construction and commissioning of MAX I and its beamlines was for the most part the small scale work of a few people, who shared a devotion and enthusiasm for the project and were prepared to work off-hours and improvise their way out of troubles (Forkman 2001). The machine director Mikael Eriksson, the first coordinator for synchrotron radiation research Anders Flodström, and Per-Olof Nilsson who organized the user community nationally, are named as especially important for MAX-lab to have come into being

123. The first MAX-lab users' organization was initiated by Nilsson already in 1978 (see further below) (Forkman 2001, p 102).

(Forkman 2001, pp 129-130). MAX I was a 'home-made' accelerator project, constructed in a step-by-step fashion and with no overarching schedule or budget determined from start to end (Forkman 2001, p 116; *M Eriksson 2, Flodström interviews*). The constantly changing financial situation often made the whole project uncertain, let alone the question of when or how it would be completed. But work continued: "For some reason, we were a few people who were very enduring, and finally it started to rotate" (*M Eriksson 1 interview*). This patchy way of proceeding, however, did not prevent MAX I from reaching high standards, and commentators agree that although MAX I fell below most contemporaries on specific performance parameters, the whole experimental setup with machine, beamlines and experimental stations was of highest standard for some applications (*Andersen, L Johansson, Mårtensson 2 interviews*). Especially beamline 22 (a spectroscopy beamline) is said to have "made MAX-lab famous", producing results that "people didn't think were possible" (*Andersen interview*).

Although a national Swedish research facility, MAX-lab was in the late 80s a laboratory with a very informal and relaxed atmosphere and work mode, especially in comparison with today's synchrotron radiation laboratories:

"There were funny moments. We were alone at MAX-lab, Jesper [Andersen] and I, at night around 1988. Jesper had learned how to inject the ring, so I prepared clean surfaces while Jesper injected, so that when I was done preparing, we had a new beam. That was very different from how these synchrotron radiation facilities work today" (*A Nilsson interview*).

"The beamline [22] was ready some time in the fall, and we measured together with people from Uppsala. And so Christmas Break came, and New Year. And some time in March, I went to the US and to a conference at University of Oregon, and I held a lecture and showed results from the new beamline. And it was simply the printouts photocopied on transparent... long before PowerPoint. And when I was done, someone said 'show us the palladium spectra again' and I did and he said 'Is that really true, all it says? Look at the date.' And it said '1/1 16:30'. And that was true. I could operate MAX I, I could inject, so on New Years day when I woke up and didn't feel very well, you know how it can be, I decided to go to MAX-lab and do some work. I think this captures some of the atmosphere" (*Andersen interview*).

These anecdotes are telling, especially when used for comparison with the next generation MAX-lab, that was already under planning when MAX I delivered its first photons in 1986. MAX I had been a home made, small scale

project, developed through a series of circumstances and in small steps, far from 'big science':

"No, I wouldn't call it big science at that time. Perhaps in a visionary sense among some of us, but for the most part, it was a continuation of a Swedish spectroscopy tradition, with new possibilities and perhaps somewhat more expensive" (*Flodström interview*).

MAX II, or as its embryonic predecessor concepts were called, 'Big MAX' and 'Super-MAX', was a significantly larger project. It would require a full-fledged facility initiative and a political process involving huge amounts of money compared to the first MAX project.

The new MAX

With MAX I, the accelerator physics group in Lund had shown its capacity to design and construct a machine that a Swedish national user community could make use of. But scientific and political developments, not least on the European scene where the ESRF (European Synchrotron Radiation Facility, see chapter 6) loomed large as the future promise for large parts of physics, chemistry and biology, had taken synchrotron radiation one step further from 'esoteric endeavor' to 'mainstream activity', with repercussions also in Sweden.

The people at MAX-lab had started to work on a new, larger MAX accelerator already in 1985, before MAX I was running and before it had hosted experiments (Forkman 2001, p 130). In its first version a Nordic dedicated synchrotron radiation source was envisioned, called 'SuperMAX' and built around a "technically very advanced" accelerator with an energy of 2 GeV or more (Forkman 2001, p 158). The SuperMAX proposal was submitted to the Natural Science Research Council in 1986, at about the same time as the discussions on possible Swedish participation in ESRF was at its height. When the decision arrived that Sweden was going to join together with Denmark, Finland and Norway within the Nordsync consortium,¹²⁴ it became clear that

124. The conclusion of the *NFR Working Group Report 1980* is that the projected ESRF facility will be a valuable resource for Swedish science and that it would be a "surprising departure from previous Swedish science policy" not to join (*NFR Working Group Report 1980*, pp 25-26). During the rest of the decade, the council acted positive towards the ESRF plans. But the costs involved were high, and a Nordic collaboration emerged as a possible strategy. After positive response from the Nordic Council of Ministers in 1986, the Nordsync collaboration was worked out in

the ESRF thereby would fill part of the growing demand for synchrotron radiation in the Swedish research community, especially that in the hard x-ray regime. The council concluded that SuperMAX was too ambitious a project to be sponsored, given the Swedish commitment to the ESRF (Forkman 2001, p 161). With the future ESRF primarily a source of hard x-rays, the research council's policy was that domestic developments of synchrotron radiation should stay in the VUV and soft x-ray regime, and so MAX-lab was told to redirect and lower its ambitions accordingly (Forkman 2001, pp 161-162; *M Eriksson 2 interview*).

A year later, the group in Lund returned with a second proposal for a 1 GeV accelerator at MAX-lab. Its ambitions were somewhat lower, but still swept most of the spectrum of synchrotron radiation utilities, and the scientific case included crystallography, lithography and x-ray spectroscopy (*MAX II first proposal 1987*). After a second negative response, a refinement of the concept resulted in the 'Big MAX' ('Stor-MAX') concept, which was turned into MAX II after further work to mobilize the scientific base for the project and counter the resistance in the council. However, the skepticism among some members of NFR was still severe (*Flodström interview*), and the procedure was all but easy:

"I think for MAX II, it was the third or fourth grant application that finally was approved. We really wore them down" (*M Eriksson 2 interview*).

As an answer to the instructions from the research council to rethink the spectral range and thus the prospected utility of the facility, the MAX-lab board had appointed an ad hoc committee, chaired by Per-Olof Nilsson, to provide roadmap recommendations for the laboratory for the 90s (Forkman 2001, pp 162-163). A conference was held in April 1989, chaired by Ingolf Lindau who was on sabbatical from Stanford and its synchrotron radiation project, and attracting about a hundred scientists from nearly all fields with prospective interest (*MAX-lab Activity Report 1989*, p 38). This breadth gave the work of the ad hoc committee a solid foundation and formed the basis for its recommendations to the MAX-lab board. Among these was that the existing MAX ring should be fully equipped with nine beamlines in 1992; that a third generation 1.5 GeV storage ring be built with at least 8 straight sections for insertion devices, with emphasis on VUV and soft x-rays applications; and that MAX I should be kept in use and serve as injector for the new ring (*MAX news 1*). In March 1989, the board of Lund University expressed

detail and became the proxy for Swedish membership in ESRF (*Nordsync preparatory meeting minutes 1986*).

its support and intent to contribute to the realization of the project, support that was crucial for MAX-lab (Forkman 2001, pp 163-165), and in August an application for a grant of 40 MSEK to cover for the construction of the accelerator was submitted to the National Council for Planning and Coordination of Research (Forskningsrådsnämnden, FRN) (*MAX II application 1989*). In the governmental research bill of 1990, the MAX II project was mentioned but no clear decision given. The government instead stated that they awaited the scientific evaluation of the project and presumed that the research council would – on basis of this evaluation – get back to them with proposals regarding details on the project and its financing (Forkman 2001, p 168). An international evaluation of the project was done in late 1989, in its report praising the achievements of the MAX-lab staff on MAX I, but also issuing warnings and expressing concerns over the rate of scientific and technical development at MAX-lab:

“The local accelerator team has done an excellent job on MAX I, on a very limited budget. [...] The team has accumulated very valuable expertise and has proved capable – through an informal, dedicated, University-type style of operation – of delivering good performance at low cost. [...] The price estimates in the proposal are based on the assumption that this style of operation can be carried over to the construction and operation phases of MAX II. It is the opinion of the Committee that this mode of operation will become more difficult as the sizes of the Laboratory and the User Community increase” (*MAX II Evaluation 1990*, p 9).

The conclusion in the report is, however, that the MAX II design concept is sound and that the project should be made a priority in the council:

“From a technological point of view the proposed machine should therefore be regarded as a very interesting step in the development of compact, inexpensive third generation light sources” (*Ibid.*, p 9).

“The MAX II concept represents an exciting step forward in lowcost storage ring design which we recommend strongly for construction” (*Ibid.*, p 12).

In appendices to the report, letters of intent from six researchers in various fields are presented,¹²⁵ together with a list of synchrotron radiation users in

125. These were Seppo Aksela (Electron Spectroscopy of Gases and Metal Vapours), Elisabeth Källne (Photoionisation of Free Atoms, Molecules and Ions), Anders Liljas (Protein Crystallography), Per-Olof Nilsson (Electronic Structure of NBE-grown Semiconductor

the Nordic Countries compiled by Per-Olof Nilsson from a questionnaire in March 1989, containing 54 names (*MAX II Evaluation 1990*). Scientifically, MAX II had gotten a go-ahead by this evaluation, but the political decision was still pending.

MAX II politics

The MAX II project was large in Swedish perspective, and never before had a national accelerator project of the size of MAX II made its way to the decision-making level in the council and the Ministry of Education. However, compared to other Swedish commitments such as CERN (especially CERN II, see chapter 2), the ESRF or the European Southern Observatory (ESO), MAX II was not at all a big project. But the mentioned infrastructure projects are international collaborations in which Sweden participate with a member fee and which Swedish scientists use accordingly. MAX II was a domestic accelerator project, and it concerned not only a single discipline but involved a whole range of stakeholders in different scientific communities, who for some part had to be convinced about the usefulness of the MAX II facility to their specific discipline. Never before had a single research project in Sweden been inquired and assessed so carefully and by so many people as MAX II; and the multidisciplinary character of the project was certainly part of the reason for the hard and widespread scrutinizing (Forkman 2001, p 171).

By the beginning of the 90s, a global user community had established itself around synchrotron radiation, and several new labs were planned and built globally, not least the 'big three' including the ESRF in Grenoble. In spite of this, one of the main questions in the Swedish domestic debate on synchrotron radiation was still how broad the potential national user community was:

"I remember visiting NFR and giving a presentation and they asked me what I thought would be the [annual] number of users in ten years time, and I said maybe 800 or 1,000. And they laughed me down" (*Lindau 1 interview*).¹²⁶

Especially biologists and chemists initially had strong opinions against the

Systems), Joseph Nordgren (Soft x-ray Fluorescence Spectroscopy for Molecules and Solids), and Nils Mårtensson (a number of projects) (*MAX II Evaluation 1990*).

126. Approximately ten years after this episode, MAX-lab had around 500 annual users (*MAX-lab users document*).

project. In the council, representatives of these fields argued against MAX II, viewing it as a physics project and a physics machine and expressing the opinion that Sweden already paid for and contributed to a host of comparably large-scale physics projects¹²⁷ (*Gidefeldt interview*). Uppsala University chemists and biologists argued for the construction of a hard x-ray synchrotron radiation source in Uppsala (*Liljas interview*), while representatives from other universities were of the opinion that the project was too big for Sweden and that the money would be better spent at ESRF (*Flodström interview*). The reported developments in insertion device technology, indicating that MAX II could be made to produce also hard x-rays (see below), seem to have drowned in the debate.

The final resolving of the funding question of the MAX II is an episode significant for the Swedish research policy and funding system (see chapter 2). In May 1990, the research council gave the MAX II project its full support and proposing to FRN that it be funded principally at the level requested, namely with a grant of in total 40 MSEK distributed over five years according to a specific plan. The plan however suggested that only 2 MSEK for detailed design studies and preparations should be paid during the academic year 1990/91, and that the rest of the money be granted once the issue of where to locate the lab had been resolved (Forkman 2001, pp 172-177). It was clear that the council regarded real estate questions as external to their responsibilities, as well as the increased annual operation budget for the lab which was supposed to be covered for by Lund University or by a special commitment by the government, through the university (*Ibid.*, pp 172-173). This was only one of two times that the operation funds of the facility was mentioned during the process leading up to the go-ahead decision for the project. The question of funding for experimental equipment at MAX II was treated in a similar manner; in a letter to the government in June 1990 explaining the details of the proposal and their considerations, the Natural Science Research Council expressed hopes that the Wallenberg Foundation (KAW), Nordic collaboration partners, and the Technical Research Council (Teknikvetenskapliga Forskningsrådet, TFR) would contribute to the funding of beamlines and experimental stations. Only once more, at a meeting called for by the Lund University Vice Chancellor's Office in August 1990, were matters of operational costs discussed. Building and construction were clearly prioritized over the somewhat more remote questions of where the money should be found for beamlines, experimental equipment, scientists, user support and other operating costs (Forkman 2001, pp 174-175). History

127. Apart from the major commitment to CERN, two domestic accelerator laboratories for physics were funded by the council: the The Svedberg Lab in Uppsala and the Manne Siegbahn Institute, former Gustav Werner Institute, in Uppsala (*Gidefeldt interview*).

has since shown that while such matters have never been solved in a thorough and comprehensive way, it might not have been possible to get the MAX II project through the Council and arrive at a governmental decision had all the expected total costs been revealed and considered as the total council commitment it eventually became. On the other hand, not clearly resolving the issue of what costs should be covered by whom among the stakeholders involved led to a blurred financial situation for the laboratory, that reports and investigations continuously have remarked on and that still remains (see further below).

The decision to go ahead with the construction came in the governmental budget bill of January 1991, which stated that the project had the government's full support but that the funding should be made within the existing frameworks of the Swedish National Board of Universities and Colleges (Universitets- och Högskoleämbetet, UHÄ hereafter), the Natural Science Research Council (NFR) and the National Council for Planning and Coordination of Research (FRN) (Forkman 2001, p 176). The accelerator (41.1 MSEK) was paid in its entirety by FRN funding for expensive equipment, so-called "big science money" (Ibid., pp 180, 183). The Ministry of Education announced that it would cover the construction costs for a new building, amounting to 62 MSEK (*MAX news 1*), but the responsibility for the 'conventional facilities' (i.e. buildings, maintenance, electricity and such) was left entirely to Lund University.

This distributed funding profile is unusual compared to other similar laboratories abroad, not least that agencies and financiers made partial funding decisions without guarantees that other parts be commenced. The government and the research councils funded construction of accelerator and buildings without any decisions made regarding funds for beamlines and experimental equipment or long-term operating costs. In effect, this meant that over 100 million SEK was spent without assurances that the investment would be put to any use. No overall budget has ever been formalized for the running costs and capital investment of the laboratory, instead the financiers have continued to award individual grants for accelerators, beamlines, experimental equipment, and operating costs at the very same governmental laboratory, which is internationally a very unusual situation. Even once construction of MAX II was well under way, in 1993, financial and planning obstacles emerged in the shape of an anticipated increase of the operating budget not materializing. The situation was resolved by a "generous donation from LU" and "extraordinary funds" from the natural science research council (Forkman 2001, p 217).

On the other hand, some people involved look at the process from a completely different angle, and regard it remarkable that the project was funded

and realized at all. The earmarked council money for expensive equipment, under FRN control (Forkman 2001, p 180), and the support of the succession of vice-chancellors of Lund University (Ibid., p 248), are mentioned as especially important factors. A comparison can be made with the United States, where funding commitments by the federal government are not always completely trustworthy since they are in reality granted year by year (and can be subject to budgetary disputes in Washington, see chapter 4) whereas in Sweden funding commitments – once they are made – cannot be easily revoked (*Lindau 1 interview*). It is also argued that MAX-lab was lucky to have had ‘friends’ in the councils, i.e. influential advocates at key positions (Forkman 2001, p 161; *Gidefeldt, Lindau 1 interviews*).

Constructing MAX II

Sweden’s scientific rationale for investing in the MAX II upgrade of MAX-lab was almost exclusively physics experiments in the VUV and soft x-ray range of synchrotron radiation. Storage ring development was still subject to a sharp distinction between hard x-rays and VUV/soft x-rays (see chapter 3). The VUV/soft x-ray regime was cheaper and the ESRF, once it was taken into operation, would cover for Swedish researchers’ need for hard x-rays. Most importantly though, the VUV and soft x-ray regime corresponded to the needs of the existing domestic user community, built on strong surface and materials physics traditions in Linköping and Gothenburg and the Uppsala spectroscopy tradition (see further below). Thus the original plans for the MAX II facility, as formulated within MAX-lab as well as articulated towards the council, was entirely focused on that wavelength regime, however clearly aiming high:

“We did, in the original plans for MAX II, concentrate on VUV and soft x-rays, perhaps including a beamline for hard x-rays on which experiments could be prepared at the prospect of going to ESRF. That was the plan. [...] So there were no promises about hard x-rays in the original applications, and no promises of industrial applications. [...] But the ambition was very well defined, it was to get a light source that was internationally competitive. Second to none. There was no other standard for it than to become the best in the world”¹²⁸ (*Lindau 1 interview*).

128. With regard to industrial applications, contacts had been established with the pharmaceutical company Draco (see below), but the plans were only of speculative character.

According to the 1990 international evaluation committee, these ambitions were well matched by the design concept. The committees' opinion about the MAX II design was that it would be "superior to most" in the VUV and soft x-rays region (*MAX II Evaluation 1990*, p 8).

MAX II construction presented a whole new challenge to the accelerator group. While MAX I was a piece of "Lundian handicraft" (Forkman 2001, p 116), the task of constructing MAX II was "one of the most advanced technical undertakings done in Sweden [...] and the [accelerator] group now needed to rethink and think in industrial terms" (Forkman 2001, p 213). On the initiative of the research council the MAX-lab board appointed an international reference group for the MAX II project to assist the MAX-lab board, directorate and personnel in the construction and commissioning of accelerators, beamlines and eventually experimental stations (*Ibid.*). According to the director of MAX-lab during the years of MAX II construction, the project had a "head start" compared to its competitors: even though the ambition was to become "second to none", the ability to build on the scientific and technical base of the existing MAX-lab and the experiences with MAX I, and the existence of a strong and capable user community provided a combination of good circumstances. The main 'competitors' were the ALS in Berkeley and ELETTRA in Trieste, Italy. Compared to these, MAX-lab had the advantage of already having MAX I running well and producing good results, satisfying the user community and the financiers. Therefore, the construction and commissioning of MAX II did not have to be forced but the machine group could be given time to make it run smoothly before users were admitted (*Lindau 1 interview*). The ability to establish good relations with supply contractors¹²⁹ is also mentioned as an important factor in the process (Forkman 2001, p 214).

The first beamlines for MAX II were largely funded with a grant of 40 MSEK from the Wallenberg Foundation, awarded in 1992 and covering for front-ends, insertion devices and some instrumentation (*MAX news 2*). The MAX II beamlines were designed and conceptualized with heavy involvement from users, a strategy to guarantee that instruments met the needs of the user community and to tie specialist scientific and technical knowledge and skill to the beamlines, but also for economic reasons – external capital was needed to complete the funding of the beamlines. Every beamline at

129. Especially the Uppsala based company Scanditronix, who delivered the magnets, and the local mechanical manufacturer *Erik Olssons Mekaniska* in the small village Tollarp outside Kristianstad, just some fifty kilometers from Lund, who delivered the magnet tripods (Forkman 2001, p 213). Erik Olssons Mekaniska have subsequently been contracted for similar work at other synchrotron radiation sources, such as the Swiss Light Source (SLS) (*M Eriksson 1 interview*).

MAX-lab has in one way or the other has been initiated by external groups of users, and in some cases Participating Research Teams have been established on basis of this (see below) (*Nyholm 2 interview*).

On September 15, 1995, MAX II was inaugurated by the King of Sweden (*MAX news 8*). The first experimental data with MAX II radiation were collected in May 1997. Given the clear 'orders' of the research council ten years earlier that MAX-lab should not go into the hard x-ray regime but concentrate on VUV and soft x-rays, and given the ambitions of the MAX II team as expressed in the political process, it is ironic that the first result obtained was the mapping of a protein on a beamline utilizing radiation in the hard x-ray regime (*Liljas interview*; *MAX-lab Activity Report 1996*).

Hard x-rays at MAX-lab

The aforementioned 'ad hoc committee' of 1989 drawing up the future plans of MAX-lab had not only included accelerator physicists and physicist users of MAX I, but also representatives from other branches of synchrotron radiation applications, most notably the life sciences. The simplified but roughly accurate division of scientific disciplines making use of synchrotron radiation, between physical sciences and life sciences, correspond generally¹³⁰ to the division of the spectrum of synchrotron radiation between VUV/soft x-rays and hard x-rays that separated synchrotron radiation sources in two distinct accelerator designs until the mid 90s. As already mentioned, MAX II was entirely a VUV/soft x-rays source when on design stage, but this did not mean that scientists with interest in the other regime had reason to exclude themselves entirely; the earliest ambitions ('SuperMAX') encompassed also hard x-rays. Furthermore, speculations had emerged already in the mid to late 80s about the possibility of insertion device technologies enabling hard x-rays from a low energy ring.

Contacts had been established with local industry early on about possible utilization of MAX II, most notably with the pharmaceutical company Draco (a part of Astra and eventually AstraZeneca), and the discussions were concerned also with hard x-ray applications (*Lindau 1 interview*). The division of molecular biophysics within the Department of Chemistry had been founded in 1984 with the ambition that the future facilities at MAX-lab would be used

130. It is not absolute; there are life sciences applications in the soft x-ray range and physical science applications in the hard x-ray regime – these categorizations always suffer from lack of sharpness.

extensively in the work of the department (Sjöström 2007, p 342), and had had some involvement in the MAX II plans. The prospects of obtaining hard x-rays from MAX II with a strong wiggler, as reported by the MAX-lab accelerator group, deepened the involvement of these representatives from the life sciences (*Liljas interview*).

The innovative 'superconducting wiggler' and its capabilities of producing hard x-rays from the MAX II ring had been mentioned as feasible in the 1990 international evaluation of the MAX II project (*MAX II Evaluation 1990*, p 7). By imposing very high magnetic fields on the electrons (using superconducting magnets), the superconducting wiggler produces 'overtone' radiation, of a severely shorter wavelength than ordinary undulators and wigglers would be able to do with the electron energy of MAX II (*M Eriksson 2 interview*). With this innovative solution the MAX II ring, optimized for producing radiation in a regime way below hard x-rays, could expand into that region and potentially serve a number of utilities previously excluded. To make it possible, a scientific base needed to be mobilized, which took a few years. One of the letters of intent appended to the 1990 international evaluation of the MAX II proposal expressed a request for at least one crystallography beamline at MAX-lab, serving as a "home base" where Swedish researchers could prepare for efficient use of the planned ESRF facilities, and a complementary facility that could serve the basic needs, with ESRF envisioned as only available for "experiments with very demanding requirements" (*MAX II Evaluation 1990*, appendix p 5).

As soon as it became clear that the superconducting wiggler was practically feasible, planning started for a kind of pilot beamline for diffraction studies, including protein crystallography. The wish to establish this program as soon as possible made the group behind it go for the unusual solution to buy the blueprints of an existing beamline at another laboratory and construct an exact copy (*Cerenius interview*; *MAX news 24*). However, some political resistance against crystallography facilities at MAX-lab lingered also after feasibility had been proven. Individual representatives in the research council were allegedly against the pursuit of the program because it was seen as unnecessary – better sources optimized for hard x-rays existed elsewhere, for example at ESRF in Grenoble, and money would be wiser spent there to maximize returns (*Mårtensson 1 interview*).

The beamline became a success, at least in absolute numbers. In 2002, it accounted for over a third of the MAX-lab users ("over 200" out of a total of 589, *MAX-lab Background Material 2002*, ch 2 p 5), with its dual utility of inorganic chemistry (small molecule crystallography) on 1/3 of the time and structural biology (protein crystallography) on the rest (*MAX news 24*). The

program showed early on that the hard x-rays obtained from MAX II with the superconducting wiggler were indeed usable for structure determination of macromolecules, and this triggered a larger demand for crystallography facilities at MAX-lab. A refinement of the superconducting wiggler, developed by the MAX-lab accelerator group and named the 'MAX wiggler', had been put to use delivering hard x-rays to a beamline for materials science applications including EXAFS (*P-O Nilsson, Nyholm 3 interviews*). Another 'MAX wiggler' was used for a beamline entirely designed for crystallography, built with investments and involvement from AstraZeneca and the pharmaceutical company NovoNordisk.¹³¹ This project was initiated as soon as the original hard x-ray diffraction beamline started to get overbooked, with the clear aim of serving exclusively crystallography users, on five experimental stations built up successively as beamtime demand grew (*Liljas interview; MAX news 17*). The beamline is far from the performance and capabilities of the specialized crystallography beamlines at other sources, for example ESRF¹³² (*Ursby interview*), but it is a local or regional resource with smaller travel distances and somewhat better flexibility due to smallness and the local spirit, and thus some alleged comparative advantages:

"We can't compete with ESRF. We can't compete with what they build at DIAMOND, SOLEIL, the big guns. They have more resources, they have a better beam, they have better everything, I almost said. So we'll have to be as good as we can and at the same time try to compete with flexibility. We're the regional source, we've got users from Denmark and Sweden and it's easy for them to come here. And we can be flexible with scheduling. [...] And to be crass: we can compete with the fact that it is easier to get beamtime here than at other places" (*Cerenius interview*).

131. In addition to these, Danish academics are also involved. The funding is split between the Wallenberg Foundation, the Danish Biotechnological Instrument Centre (DABIC), Novo Nordisk, and AstraZeneca, the last two contributing 6 MSEK each and in exchange getting one day each a week of guaranteed beamtime, that they however also pay for (*MAX news 23, Liljas interview*).

132. MAX-lab automatically logs the number of datasets that are measured on the beamline, and in the spring of 2006 (totally 22 weeks of operation), a total of 422 datasets were collected. At some of the best beamlines at ESRF, a hundred datasets can be collected in one day (*Ursby interview*).

The 'ordinary' MAX II science

Although the life sciences have grown as part of the MAX-lab activity ever since MAX II was taken into operation, the laboratory is still for the most part considered a 'soft x-ray lab'. In part due to the differences in work mode and habits of the experimental groups (see chapter 7), and in part due to the physics tradition that constituted the whole user base for MAX-lab during its first ten years of operation, there are striking differences between the new group of users utilizing the hard x-ray beamlines and the users at the VUV/soft x-ray beamlines. The diffraction activities are sometimes called 'an island' at MAX-lab, although it makes up a significant part of the total number of users. But in other significant aspects, MAX-lab is dominated and characterized by the VUV/soft x-rays research (*Cerenius, M Eriksson 2, B Johansson, Nyholm 1, Ursby interviews*).

The construction and commissioning of the long anticipated and awaited beamlines for VUV and soft x-ray spectroscopy at MAX II of course continued in parallel with the opening of new opportunities by the superconducting wiggler. Three beamline types for spectroscopy experiments had emerged as "obvious" choices for the first MAX II beamlines (*Nyholm 2 interview*), because they corresponded to the user community that provided the 'original' scientific base for MAX II. One of them was an upgraded beamline from MAX I, rebuilt and moved in 1997. The other two originated in collaboration between MAX-lab and groups in Lund and Uppsala, respectively, both with involvement also from researchers at Linköping University, and at these two, Participating Research Teams have been established (see below).

Certain parts of Swedish condensed matter physics have reportedly strengthened capacity and performance in a mutual development with these three original physics beamlines at MAX II. In a 2004 international review of Swedish condensed matter physics, orchestrated by the research council, the mutual importance of Swedish research activities in condensed matter physics and the availability of synchrotron radiation is highlighted – with specific referral to MAX-lab. Synchrotron radiation is said to play "a major role in much of condensed matter physics" and MAX-lab, "an exceptionally successful and competitive facility", has "shaped a significant fraction of high-quality condensed matter physics research in Sweden", not least through the synergies established between MAX-lab and the existing strong spectroscopy tradition in Sweden (*VR Condensed Matter Review 2004*, pp 27-28). Particularly important are the relations between the Uppsala University physics department and MAX-lab (see below).

MAX-lab continues to manifest its prime identity as a 'soft x-ray lab', not least with the recent construction of a new VUV/soft x-ray ring, MAX III.

Though designed partly to serve as a prototype for the future MAX IV project (discussed below), it also fills a gap in the supply of high brightness VUV and soft x-ray radiation, and it is supposed to be moved to the suggested new MAX IV facility in the future. That MAX-lab invests in a new ring for VUV and soft x-rays is significant for the laboratory and a positive development, according to the VR 2002 national facilities review, because it "fills a gap" created by the tendency of new labs to focus on hard x-rays (*VR Facilities Review 2002*, p 43).

MAX III can be seen as a part of a continuous upgrading and refinement process at MAX-lab. It is argued that a laboratory under constant evolution and development can be continuously optimized, with adding and substituting of scientific programs and experimental equipment as they emerge, become attractive, and fade. The alleged advantage of such a process is that scientific and technological developments in synchrotron radiation can be incorporated while they happen, enabling for a more efficient use of available resources compared to the designing and conceptualizing a laboratory in full, with a complete set of beamlines and experimental stations (*Mårtensson 2 interview*). This is a general feature of synchrotron radiation laboratories, but it is perhaps especially visible at MAX-lab, as will be discussed later.

Just as most synchrotron radiation laboratories, MAX-lab has had a brief episode with x-ray lithography (see chapter 3). The beamline for this purpose was put into operation in February 1999 (*MAX news 17*), but soon taken out of use because of lacking scientific (and industrial) interest (*Nyholm 1 interview*). The nuclear physics program has been ongoing ever since MAX I was taken into operation, except for an interruption for upgrades of the program during the years 2001-2004 when also the new injector system for MAX II was installed, replacing MAX I as injector (*MAX-lab Activity Report 2003*, p 359; *2004*, p 389). Quite naturally, the nuclear physics program at MAX-lab has stood back a little compared to the synchrotron radiation activities. Already in a 1997 NFR review the nuclear physics program was given lower priority compared to the synchrotron radiation activities, in a national context (*NFR Facilities Review 1997*, p 28). Since 2004, MAX I has run roughly one third of its total time in nuclear physics mode.

One conclusion of the broader scientific development at MAX-lab since the go-ahead decision for MAX II in 1991 is that the laboratory has developed into a full scale synchrotron radiation facility, with beamlines and experimental equipment fairly well covering the whole spectrum of possible fields of utility. The annual number of users has grown steadily with particularly strong increases in the late 90s, when the new beamlines at MAX II came on track one by one. The development of the life sciences applications and the simultaneous strengthening of the traditional core competencies and sci-

	00/01	01/02	02/03	03/04	04/05
Physics	320	353	284	284	281
Chemistry	60	62	75	97	126
Life sciences	177	174	179	199	165
Total	557	589	538	580	572

Table 6: Disciplinary categorization of MAX-lab users, 2000-2005. The categorization is done on basis of users' own designation when they

register their stay as users during a beamtime period. The time periods are August to July, i.e. one period of running time and proposal round (*MAX-lab users document*).

entific programs are mentioned by many as factors that has put increased MAX-lab's visibility in broader scientific communities and strengthened the support for the facility among several different parts of the Swedish scientific community (*M Eriksson 2, Honeth, B Johansson, Mårtensson 1, Nyholm 2, Novella Piancastelli interviews*). In relation to the other Swedish national facilities, MAX-lab stands out as a user facility with a broad scope:

"MAX-lab is the only one of the four facilities which is a national facility in the sense of offering a platform or service for various research groups from different Swedish universities and abroad" (*NFR Facilities Review 1997, p 27*).

"The MAX laboratory has a clear mission as the swedish [sic] national synchrotron radiation light source, serving a large, dynamic, and growing community in the fields of physics, materials science, chemistry, and biology. It stands out among the four national laboratories as a source of basic infrastructure support for swedish [sic] science and technology broadly defined" (*VR Facilities Review 2002, p 12*).

External laboratory politics

MAX-lab became a 'national research facility' in 1981, through a decision by the Natural Science Research Council, a label that meant nothing in terms of organizational or financial status. The organizational ambiguities of the two Swedish national research facilities MAX-lab and the The Svedberg Laboratory (TSL) in Uppsala were subject to continuous criticism throughout the 80s (Forkman 2001, p 184-185), and when the MAX II project emerged the issue became even more pressing. The council's articulated opinion that large Swedish research infrastructures were beset with problems of incoherence

and lack of strategy (*NFR Facilities Report 1992*, p 5) led the government to initiate an investigation on the matter. The report suggested a clear definition of 'national research facility', including that its activities are regarded "of high international quality" and the use by national (and possible international) researcher has "sufficient scope" (*NFR Facilities Report 1992*, p 3).

The investigation pointed at a number of shortcomings in the organizational system. First of all, funding was regarded insufficient. Capital investment in the facilities was done by single grants directly from the government, from the 'expensive equipment framework' of the planning and coordination council or from private sources such as the Wallenberg Foundation. Operational costs were covered by a special 'earmarked' grant within the ordinary annual governmental grant to the host universities. Several disadvantages with this system were acknowledged, for example that the facilities' strategic long term planning was hindered by the money coming from different sources and that comprehensive overviews of the de facto overall costs for the facilities was hard to make. Operation expenses tied to ordinary university budgets meant limited opportunity for adjustments, leading to significant inflexibility. The report also concluded that the responsibility for the operation of national facilities was "vaguely formulated" and divided between many authorities, and that this had the effect that comprehensive quality assessment and conclusions about outcome in relation to costs were hindered (*NFR Facilities Report 1992*, p 9-11).

The report suggested that responsibility for operations be transferred to the Natural Science Research Council and that a special annual grant from the government to the council should cover for operations, "separate from the other grants to the research councils" (*Ibid.*, p 13) but with priorities between the four facilities¹³³ made within the council. A 1993 governmental bill implemented modified versions of these changes. The operational costs for the national research facilities were made the council's responsibility, but no special governmental grant was issued. The operational costs to the facilities were instead to be "weighed against" other funds under the control of the research councils (Forkman 2001, p 186), and so the bill was merely a reinforcement of the existing policy (see chapter 2): Investments in research infrastructure was to be made within the existing framework of governmental research financing and weighed against every other expense of the council.

A follow-up investigation was put in place in 1993 to make recommendations about the resource needs of the national facilities. It concluded that the

133. In addition to MAX-lab and TSL, two facilities had been established in 1989 and 1990; the Supercomputer Centre in Linköping and Onsala Space Observatory (OSO) in Gothenburg, respectively (*NFR Facilities Report 1992*, appendix 1, p 1).

main criteria facilities needed to fulfill in order to receive “good support” from the research councils were that (1) the activity at the facility should be in the international forefront and the facility should be attractive for internationally prominent researchers; (2) the facility should be extensively used by researchers from several universities and colleges in Sweden; and (3) the activity should be within the realms of high priority by NFR or another research council. The facilities should have program advisory committees consisting of highly qualified researchers with special competence in the facilities’ areas, and the facilities should be given “great freedom” to decide about the use of the funding they receive (*NFR Facilities Report 1993*, pp 3-4). The MAX-lab governmental regulation of 1994 largely followed the recommendations of this 1993 report, stating that the laboratory should be available to researchers from universities, colleges and other scientific institutions in Sweden, and that foreign researchers also should be eligible to do research at the laboratory (*MAX-lab governmental regulation*).

The national facilities have undergone reviews with a few year interval. In the 1997 review, all four received very good appraisal: judged by their scientific achievements, they were all excellent hosts of world-class activities in their respective fields.¹³⁴ But they were also expensive, and continuously facing challenges in their expansion and development (*NFR Facilities Review 1997*, pp 7, 11, 16, 23). The 2002 review did for the most part repeat the praise, but it also laid emphasis on the organizational and financial problems at the labs. Especially the resource scarcity, partly due to the fact that the national facilities are funded within the same envelope and that little room is left for negotiation over the size of that envelope, was discussed (*VR Facilities Review 2002*). The distributed funding model and its lack of coherence was heavily criticized:

“In part the financial problems originate from a serious structural problem in the swedish [sic] funding policy. The funding of investments in instrumentation and other infrastructure is usually decoupled from the support for operational costs. Scientists at the facilities cannot resist the lure of research opportunities presented by new investments, despite the obvious absence of operational support. To compensate, the National Facilities have developed a ‘shadow economy’, in which students and users provide technical and user support over and above their research activities. The

134. MAX-lab had made “outstanding scientific accomplishments” and was among the “world leaders” in the field; TSL was a “world class” laboratory and had a “strong and unique role”; Manne Siegbahn Institute (henceforth MSI) was “not only unique in Europe but also worldwide” and showed “outstanding” performance; and OSO was “uniformly excellent” and showed an “impressive demonstration of world class science” (*NFR Facilities Review 1997*, pp 7, 11, 16, 23).

panel thus recommends a funding model where there is a better match between operational and investment funds" (*VR Facilities Review 2002*, p 15).

The 'shadow economies' of the national facilities is brought up several times in the report, as a suggestive reminder that the council so far hadn't taken full responsibility for the investments and operations at the national facilities, despite their excellent performance. The particular 'shadow economy' at MAX-lab will be discussed in detail below.

The review of MAX-lab is especially positive, and highlights the active involvement of users in the facility's development (*VR Facilities Review 2002*, pp 12-13). The MAX-lab management is commended for their ability to run a diverse and constantly evolving research facility and balance the interests of different user groups (*Ibid.*, p 39), and the good relationship with Lund University is mentioned as an important factor of the success (*Ibid.*, pp 17, 28). The report concludes that MAX-lab is an internationally competitive synchrotron radiation facility (*Ibid.*, p 38), but in order for it not to fall back, both MAX-lab and the council have to adjust priorities and strengthen initiatives (*Ibid.*, p 44).

Apart from the ordinary task of evaluating the overall scientific performance of the facilities, the panel was charged with recommending funding profiles for the facilities for the coming 3+3 years, i.e. the details of the internal allocation of funds between the four labs, taking into account the prospects of possible future upgrades of the facilities and not excluding discontinuation of facilities or withdrawal of their status as 'national' (*VR Facilities Review 2002*, p 63). The answer to this charge was a recommendation to the council to reevaluate its priorities and concentrate funding on MAX-lab and Onsala Space Observatory (OSO), with MAX-lab as highest priority (*Ibid.*, p 16). Consequently, in September 2002 the research council decided to concentrate resources on MAX-lab and OSO and phase out funding for the two other national facilities, TSL in Uppsala and MSI in Stockholm, whose status as national facilities also was to be taken away by the government (*VR Annual Report 2002*, pp 7, 28; *L Eriksson interview*). This decision is a clear indication of elevation of MAX-lab's status in the council and on national level in the science policy system, and it was followed by an increase in the annual MAX-lab budget of more than 10 MSEK over three years. But the ambiguous organizational and financial conditions – including the reliance on informal structures and a 'shadow economy' – were not changed (see below).

MAX-lab's organizational status within the council remained largely unchanged for almost a decade after the 1993 restructuring, with the exception of the 2001 merger of the four research councils and the functions of

FRN into a new agency, the Swedish Research Council (Vetenskapsrådet, VR) (see chapter 2). The 2005 establishment of the Committee for Research Infrastructures (Kommittén för Forskningens Infrastruktur, KFI) within the council, acknowledging that research infrastructure is a different issue than the ordinary functions of a research council (*Karlsson interview*), was largely an internal council affair, and didn't change the fact that investments in research infrastructure has to be made out of the ordinary budget of the research council. The visibility of research infrastructure within the council's activities has however been increased with the continuously updated KFI strategy document, the *Swedish Research Council's Guide to Infrastructure*, and the ambition of the council is clearly to elevate research infrastructure to a higher level of importance, within their limits (*VR Research Strategy 2009-2012*, p 1).

But the council still exercises very little hands-on governing of the national facilities apart from deciding about laboratory operational budgets, a matter that is attributed to the general "principle" of the Swedish Research Council; it awards grants with no other instruction than to make the best out of it, and then expects a report back a few years later (*Karlsson interview*). The council decides on the size of the budget, but leaves it to the labs to work out the details. Quality control is carried out through larger reviews with a few years interval. Reflecting the practices of the whole Swedish science policy system (see chapter 2), this has created a system with considerable weaknesses, but arguably also some advantages.

The systemic weaknesses and the shadow economy

With its organizational belonging divided between the research council and Lund University, the performance and procedure of MAX-lab is largely defined and confined by the overall structure of the Swedish research policy system (see chapter 2). The historical trajectories of university funding and organization give little room for comprehensive strategic commitments to a comparably expensive 'special entity' like MAX-lab. Giving the responsibility for the national facilities exclusively to the research council makes attempted prioritization of MAX-lab subject to the ordinary competition for funds within the council structure, which rather follows the logic of research financing on a smaller scale, i.e. the issuing of individual research grants to scientists and groups at ordinary university institutions. In this sense the Swedish research policy system is weak: it lacks mechanisms and opportunity for strategic initiatives on larger scale.

A facility like MAX-lab is both large-scale and strategic; for a small country like Sweden research infrastructure of this size means a strategic commitment, as emphasized in chapter 2. Viewing MAX-lab as an institution, and its collected infrastructural assets and research activities as a whole, enables an evaluation of it as a Swedish commitment to a specific basic research activity, comparable to other activities. With such a perspective, MAX-lab's organizational and financial status becomes opaque and complicated, because of the dividing of laboratory funding between different agencies according to types of expenses. The funding of MAX-lab has never been 'clean' in this sense but always dependent of money from several separate sources, which has made it impossible to comprehensively assess the Swedish investment in or commitment to synchrotron radiation, compared to other areas or activities (*L Eriksson interview*). It has also led to underfunding because the different financers do not coordinate. For example, some scientists' highly qualitative work may be partly due to the existence of top-class instrumentation at MAX-lab, but the grants awarded to these scientists cannot be made to contribute to the covering for MAX-lab operations. The operating budget from the council has never been balanced with investments, expansion and the number of users. Thereby the willingness or eagerness of the council to support good science – which is their mission – has lead to the paradoxical situation of constant lack of sufficient resources for operation of MAX-lab.

According to a former research council administrative officer, the problem is acknowledged in the council but not perceived as their responsibility to resolve. Cutting the awarding of infrastructure grants or urging MAX-lab management to lower their ambitions is not in line with the purpose and procedures of a research council (*Gidefeldt interview*). One example shows that the priorities of MAX-lab management may well be part of the reason for the unbalanced financial situation. In 1997 the operating budget was judged insufficient for maintaining sustainable operations stability and quality given the range of investments made, and instead of NFR increasing that budget the Swedish Foundation for Strategic Research (Stiftelsen för Strategisk Forskning, SSF) awarded a grant to resolve the situation. There was, however, no clearly articulated demand that the money should be used to balance the operating budget, and instead it was used for further investment in instrumentation and experimental equipment (*L Eriksson interview*).

The facility reviews have acknowledged the various dimensions of these problems and note that simply increasing the operating budget would not solve the problem:

“The flow of money is complicated. [...] The steering group recommends that there is a more comprehensive reporting of the finances of each labo-

ratory in order to have a more accurate picture of the relationship between scientific activities, in particular the core scientific activities and the resources available” (*NFR Facilities Review 1997*, p 31).

“The mismatch between program aspirations and operating budgets appears to stem in part from the way that projects are funded in the Swedish system. [...] The result is a chronic shortfall in operating funds evident at every laboratory, roughly in proportion to the laboratories success in generating new program support. An increase in operating budgets can provide a short-term fix, but if this systemic problem is not resolved, then operating shortfalls will inevitably arise again in a few years. In the course of our review, the panel also became aware that the laboratories have, to a greater or lesser extent, evolved ‘shadow economies’, in which important infrastructure tasks have been shouldered by research scientists or students in order to sustain core programs in the absence of adequate operations budgets of the facility. As a result, the true cost of doing business cannot be accurately gauged from formal budget information. [...] The extent of the ‘shadow economy’ differs from facility to facility. It appears to be very extensive at MAX-lab, where researchers from outside institutions and universities now routinely carry out tasks, which should be assigned to facility staff, assistant staff and technicians” (*VR Facilities Review 2002*, pp 18-19).

Lack of adequate resources is a common theme of the talk of almost all laboratory directors and staff (and scientists in general¹³⁵). Complaints by MAX-lab directors, staff and users are no exception but rather very widespread and coherent, and sanctioned both in the council’s reviews and the meeting reports of the MAX-lab Scientific Advisory Committee (SAC). The committee repeatedly underlines the need for increased funding to secure stable operation, adequate levels of user support, and securing timely installation of new equipment (*MAX-lab SAC Reports 1998, 2001, 2003, 2006*). Some long time users (*B Johansson, L Johansson, Nordgren, Uhrberg interviews*) argue that there is a detectable pattern: Although the political motive for investments in research infrastructure is that it supposedly benefits the work of Swedish scientists, infrastructure investments are regularly made without adequate securing of the opportunities for Swedish researchers to make use of it:

“We fool them, we pretend that it doesn’t cost that much. The first shot is for free” (*Nordgren interview*).

135. Cf. “more is never enough” (Greenberg 2001, 2007, in chapter 1).

The newly formed Committee for Research Infrastructure within the research council has acknowledged the problem and called for a new strategy to include “the entire lifecycle process, from idea to phase-out” in planning for research infrastructures (*KFI Guide 2008*, pp 21-22, 24). However, the committee’s status within the council and the overall Swedish national science policy system does not permit it to be operative and exert direct influence over the facilities and projects it is supposed to coordinate, and therefore the governance situation for MAX-lab remains largely unchanged.

It is often claimed (Forkman 2001; *M Eriksson 1, Gidefeldt, Kvik, Mårtensson 1, P-O Nilsson interviews*) that MAX-lab in its present status would not exist without the large grants for equipment issued by the Knut And Alice Wallenberg Foundation (KAW). While not granting money for facilities’ operating costs, the contributions to Swedish research infrastructure by the foundation have been very important:

“Over the years, the Knut and Alice Wallenberg Foundation has been the *dominating source* of funds for investments in advanced equipment and infrastructures. Hence, the Foundation has played a *decisive role* in many of the infrastructures that Swedish researchers use and for advanced equipment at Swedish universities” (*KFI Guide 2008*, p 21, emphasis added).

At MAX-lab, the contribution from KAW has been significant over the years, amounting to over a third of the total sum of the grants awarded for capital investments at MAX-lab during the ten years that passed after completion of MAX II.¹³⁶

The flip coin of resource scarcity

The most urgent lack of money at MAX-lab is said to concern maintenance of beamlines and experimental stations and user support (*Mårtensson 1 interview*). The ESRF provides a striking comparison: at least three persons

136. Out of a total sum of 288,228 kSEK between 1997 and 2007, the grants from the Wallenberg Foundation amounted to 105,500 kSEK, which corresponds to 36,7%. The grants from the research councils (including FRN in the 90s) amounted to 154,018 kSEK (53,4%). Grants and investments from the Crafoord Foundation, the Danish Biotechnological Instrument Centre, SweGene, AstraZeneca and NovoNordisk together amounted to 28,710 kSEK (9,9%). Note that these are only the investments done with money from external grants; MAX-lab frequently invests in equipment also with money from the ordinary operations budget (*MAX-lab investments document; Nyholm 3 interview*).

work full-time on each beamline there, while at MAX-lab the beamlines are regularly run by one person who may be employed on 'external money', i.e. users' grants redirected to MAX-lab and used for employing post-docs on beamlines as a part of Participating Research Team arrangements (see below). Several beamline scientists (post-docs, PhD students and visiting scientists) at MAX-lab have a listed employment affiliation other than MAX-lab (*MAX-lab Activity Report 2005-2006*, p 5), suggesting that they are paid with money other than the MAX-lab operational budget. Concern has been expressed that instruments are sometimes not kept in sufficiently good shape due to insufficient maintenance, and that this may impair user operation (*Nordgren interview*). The 2002 VR facility review noted the inefficient user support and the unsatisfactory situation for beamline scientists, stating that "it would be good for the laboratory if the leading scientists in charge could find more time for their own research, for teaching at their university and for strategic planning of the future of the laboratory and of synchrotron radiation research in Sweden as a whole" (*VR Facilities Review 2002*, p 39).

There are however signs that MAX-lab is perhaps not only coping with the situation but in part manage to turn it into an advantage. Learning to operate under budget constraints may foster an inventive atmosphere and a 'can-do spirit' (cf. the synchrotron radiation program at SLAC, chapter 4). MAX-lab's constant lack of adequate resources and struggle to survive has forced effectiveness upon every part of the organization, which allegedly has been utilized in a beneficial way (*Mårtensson 1*, *Novella Piancastelli interviews*). The resource scarcity is also said to have created good relationships between the laboratory and its user community. Forced to handle over responsibility to external users, the laboratory management has invited them to participate in and contribute to long- and short-term planning and strategy work, which has created a great support for the lab and made it 'national' in a real sense (*M Eriksson 2 interview*).

The origins of the devoted MAX-lab user community trace back to the late 70s and the initial ambitions to develop the future MAX-lab facility into a national resource. The first 'users' committee' for synchrotron radiation at MAX was formed already in December 1978 (Forkman 2001, p 102), giving the users a 'voice' and a forum where their commitment and initiatives could be channeled into the MAX project. The user organization (Föreningen för Användare av Synkrotronljuset vid MAX-lab, FASM) earned influence at MAX-lab early on and was given representation in the MAX-lab board. The organization also took responsibility for the annual users' meeting, which it has organized ever since (*MAX news 21*). During the first years of MAX I

operation, the laboratory was far from a streamlined user facility, and a culture of devotion was established among some users, out of a mix of necessity and sheer enthusiasm: "They could measure on one electron, and they were happy" (*M Eriksson 2 interview*).

MAX-lab management and staff are very careful to emphasize the importance of the user community; it is called "the core of the laboratory" and the user involvement is said to make "the process of forming the visions much more advanced" (*MAX-lab Background Material 2002*, ch 4 p 1). The initiative in the technical and scientific development at MAX-lab has largely been in the hands of the users (*Mårtensson 1 interview*), who have gotten their wishes fulfilled through tight collaboration with laboratory staff:

"MAX-lab gained a very high reputation worldwide for its foresight by careful planning the radiation sources, the beamlines and the instrumentation together with highly competent user groups. As a result, the laboratory could offer novel instrumentation for cutting edge research in emerging fields of science in a timely fashion" (*VR Facilities Review 2002*, p 37).

Some users have a very tangible input in the laboratory, by the design and construction of instruments for the beamlines. Users from instrument-intensive fields can thereby both design, construct and use the optimal instrument for their experiments, and simultaneously contribute to the rest of the user community, a process that is made possible by the good relationship between these users and MAX-lab (*Novella Piancastelli interview*).

The Program Advisory Committee (PAC), whose ordinary role in reviewing beamtime proposals, is also involved in the procedure of beamline project initiatives. Typically, a beamline or instrument concept is formulated by a group of researchers or a consortium of different groups, in dialogue with MAX-lab management. Before a proposal is submitted to the council or the Wallenberg Foundation to undergo standard peer review, PAC gives a priority order of all the proposals related to MAX-lab (*MAX-lab Background Material 2002*, ch 2 p 7). The formal authority to decide this priority lies with the MAX-lab board, but in practice PAC works it out, and the board has reportedly never altered its decision. In most cases also the funding agencies follow the PAC recommendations, which renders this committee great influence. Their decisions are supposed to be guided by a considered opinion of what is ultimately best for MAX-lab and for the user community, and in special cases, when the instrument proposals are judged to affect the long-term strategy of the lab, the Scientific Advisory Committee (SAC) is also involved

in the process (*B Johansson interview*). The procedure of users' initiatives being negotiated with laboratory management and reviewed by the committees is supposed to guarantee quality; the collected expertise of PAC and SAC and the users is hard to match (*Mårtensson 2 interview*).

The devotion of users due to resource scarcity may also operate reversely. The complicated situation with the council and the incomprehensive funding profile, in combination with a user community maintaining a strong initiative, may lead to a situation where MAX-lab and its directors and personnel feel a greater responsibility and commitment towards the user community, who expect to do their world class science, than towards its formal principals, the council or the government (*Flodström interview*).

General administration and organization

The stories about the early MAX-lab users' situation (cf. a quote above, "measuring on one electron"), indicates MAX-lab's local embodiment of the process from 'esoteric endeavor' to 'mainstream activity', and the present MAX-lab organization bears traces of this development. A long time user and beamline responsible says "the organization or perhaps disorganization" of MAX-lab is something that has grown organically by itself (*Andersen interview*).

Although MAX-lab has grown in physical size, in number of users and employees, in breadth of user community, and in scientific output (and thereby importance), the organization has never gone through any complete overhaul or restructuring. The operating budget and the funding for capital investment has increased gradually and allowed for renewal and increments in the staffing and the procedures, but no complete assessment of the needs has ever been done. Some would say the MAX-lab management and staff has had enough taking care of its growing collection of instruments and user community to keep up with its own development, and hasn't had the opportunity to look in the rearview mirror or stop to think (*Fahlman, Lindau 1, Nordgren interviews*). The oversimplified interpretation would be that MAX-lab is simply underfunded and understaffed and would be able to solve most of its problems if only enough money be put in. This picture is, however, contested in various ways. It has been argued that ambiguities around definitions and measure of 'productivity' in science makes certain scientific activities 'uneconomical' in the sense that funding is stretched to the last penny without comprehensive overviews. According to one interpretation, MAX-lab shows a similar pattern, indicated by the use of the special grant in 1997

(see above) that was supposed to cover for operations but was used for further investments. Thus the solution to MAX-lab's resource scarcity might not be a mere increase of funding, because the money would then be used to exploit new opportunities, again stretched to the last penny (*Honeth interview*). This view compares itself well with the history of MAX-lab – nobody contests that the driving force always was to do the best and most exciting science possible, rather than creating an efficient organization.

MAX-lab started as a small-scale university project, and it is still a division within Lund University,¹³⁷ though its activities are extraordinary to the university in many respects (the large number of external users, the demand for specialized skills of technical maintenance, etc.). MAX-lab has been designated a 'special entity' (*särskild verksamhet*) within the Lund University organization, positioned outside the ordinary organizational structure and directly subordinated the Office of the Vice-chancellor. The university acts as employer of laboratory staff (*VR Facilities Review 2002*, p 7), and there are two categories of employees, the ones paid by the operations grant from the council and the ones paid by Lund University. The university employees at MAX-lab are associated with the faculty of natural sciences (the faculty structure is the ordinary domicile for people with academic positions). But as already mentioned, large groups of MAX-lab personnel are employed at other institutions and thereby on other budgets but work "more or less permanently" at MAX-lab (*MAX-lab background material 2002*, ch 2 p 19). The laboratory director is paid via the VR grant, as is one 'coordinator' position for research, divided between the three areas synchrotron radiation, nuclear physics, and accelerator physics (*Ullman interview*; *MAX-lab Activity Report 2005-2006*, pp 3-4). The machine director and deputy MAX-lab director is also professor of accelerator physics at the Lund University physics department and he is thus employed entirely by the university (*Ullman interview*). A professorship in synchrotron radiation instrumentation was established in 1997 and is a regular faculty position within the university but in practice a MAX-lab position (*Nyholm 1 interview*). These are mere examples of how the complicated organizational and financial arrangements around MAX-lab are decided and established from case to case with no centrally defined model either at the council or the university.

The fluid organization has imposed a flexibility and perceived atmosphere

137. Also the science at MAX-lab done by Swedish researchers (who account for approximately 55% of the users, see below) is for the absolute most part 'university science', as it is financed by university money or university-administered grants, and organized as university research. This is because the Swedish public research system is almost entirely dominated by universities (see chapter 2).

of understanding and agreement among the council, the university, the users, and other stakeholders:

“Sweden has evolved a rather unique and very positive model for university/national facility collaboration. The Review Panel noticed in particular the shared responsibility between the Swedish research council and the host university not only for funding and operating a particular facility, but also for its long term strategic planning” (*VR Facilities Review 2002*, p 11).

Another seemingly advantageous feature of the fluid organization at MAX-lab is the alleged lack of bureaucracy and the informal decision and communication channels that date back to when all MAX-lab staff fitted in a coffee room, some of which is said to linger on (*Andersen interview*). The informal networks of communication is also said to be a contributing factor in MAX-lab’s relative success, because the ability and willingness to collaborate between otherwise heavily compartmentalized ‘sections’ or ‘divisions’ of a laboratory (cf. ESRF, chapter 6) has lead to an ‘optimization’ of the whole lab:

“You reach technical solutions that optimizes the whole thing, from machine to beamline to end station. And you may not get the optimal machine, but you will get the optimal system as a whole” (*Andersen interview*).

The fluidity of the organization is said to cause people to focus less on work descriptions (if there are any) and more on getting things done and solving the most urgent problems (*Cerenius, Ursby interviews*). While entailing the risk of severe inefficiency and a neglect of things important, this may also make the personnel develop a broader or more complete set of skills. It may also enhance flexibility and make the interaction between users and beamline personnel more informal, from which both sides may gain (cf. the quote above about MAX-lab’s possible ability to compete with flexibility). Beamline scientists express devotion and joy in connection with this situation, that they to a large extent can feel that the instrumentation is ‘their own’, that they have greater responsibility and freedom, and that this improves their care and concern for the beamline (*Cerenius, Ursby interviews*). But there are disadvantages. Concerns have been raised over the apparently lower publishing rate of beamline scientists and post-docs, who are forced to devote most, if not all, of their time to user support and technical work (*MAX-lab Background Material 2002*, ch 3 p 1).

Quality control and follow-up are two areas that MAX-lab has neglected due to resource scarcity, work overload and lack of clearly defined responsi-

bilities. Although user visits and MAX-lab related publications are recorded and made public in the Activity Reports, the procedures of beamtime allocation and user visits have some flaws that in theory permits people to do experiments at MAX-lab and publish the results without the MAX-lab personnel being aware of it.

Beamtime allocation and users

The regular MAX-lab call for proposals is once every year, typically early spring. All proposals are scientifically and technically evaluated by the Program Advisory Committee (PAC), formally accepted or denied by the board, and finally scheduled by the coordinator for synchrotron radiation research.¹³⁸ No external referees are involved in the process, and the ranking of proposals is done by PAC during a two-day meeting that in practice is divided in two; one for hard x-rays and one for VUV/soft x-rays (*Nyholm 1 interview*).

The laboratory's long-term strategy is partly included in the process of beamtime allocation. Unusual projects, of a kind MAX-lab normally hasn't got much of but which is judged interesting for the future, may receive a better review, and projects that doesn't really "fit in the activity" at MAX-lab might be turned down (*Nyholm 1 interview*). Apart from scientific quality and technical feasibility, two factors are weighed into the PAC evaluation: the ambition to expand the user base, which might give completely new users a better treatment in the process despite proposals of slightly lower quality, and the ambition to achieve better gender equality (*B Johansson interview*). According to the chairman of the Program Advisory Committee, MAX-lab has a closer relationship with its users, because the user community is smaller, which might increase the transparency of the beamtime allocation process (*Ibid.*).

Some users express concerns over what they perceive as imperfections in the beamtime allocation process. The criteria according to which beamtime is awarded is not written down, which may cause uncertainty about the rules and conditions for evaluation of beamtime proposals and the criteria for granting beamtime, especially among new users (*Fahlman, Sörensen interviews*). Several suggestions, including the implementation of a referee system instead of the committee system of today, a feedback mechanism for

138. Except for the beamlines where beamtime slots are shorter and more continuous flexibility is desired (the crystallography beamlines), and for the beamlines with PRT arrangements, see below.

	00/01	01/02	02/03	03/04	04/05
Sweden	310	318	312	343	323
<i>non-university</i>	11	12	15	19	10
<i>Lund University</i>	70	87	74	75	77
<i>Uppsala University</i>	92	97	96	103	85
Other Nordic countries	107	97	93	108	99
Rest of the world	140	174	133	129	150
<i>non-university</i>	4	3	7	13	9
Physics	320	353	284	284	281
Chemistry	60	62	75	97	126
Life sciences	177	174	179	199	165
Total	557	589	538	580	572

Table 7: Synchrotron radiation user visits at MAX-lab, 2000-2005. The categorization is done on basis of users' own designation when they register their stay as users during a beamtime period. The

time periods are August to July, i.e. one period of running time and proposal round. The numbers are user visits, meaning that they do not count individual users but registered users at beamtime slots. One individual user could therefore account for more than one user visit in this table. The statistics after the academic year 2004/2005 cover the period July 2005-December 2006 and would give a skewed comparison, and are therefore not included (*MAX-lab users document*).

beamtime proposals so that applicants get motivations for being accepted or turned down, and a general increased transparency in the process, have been drafted in the MAX-lab users' association (*Fahlman interview*). There is no formal requirement to report publications of results obtained at MAX-lab, not even as a prerequisite for getting beamtime again. All users are encouraged to send in their publications, and the ones received (after a few reminders) are published in the Activity Report (*Nyholm 1 interview*).

There are three exceptions that shortcut the regular beamtime allocation process, the Participating Research Teams (see below), commercial users, and so-called 'director's time'. The latter is put in place to guarantee beamtime for the beamline scientists, who are usually post-docs or PhD students (*Nyholm 2 interview*). Commercial users who don't want to disclose the content of their projects to the Program Advisory Committee can buy time. Long-term arrangements with the pharmaceutical companies AstraZeneca and NovoNordisk are in place that guarantee slots of beamtime in exchange for investments made on one of the crystallography beamlines.

The users arriving at MAX-lab register themselves in the computer system, and this logging is the prime way for the lab to keep track on who the users of the lab are and where they come from. The numbers in table 7 are based on this registration process, which is mandatory but in fact not controlled. Users can choose disciplinary belonging between the categories 'physics', 'chemistry' and 'life sciences', and this provides the information for the official user

statistics provided by MAX-lab (*Nyholm 1 interview*).

The composition of the user community has changed as the scope of the laboratory activities have broadened and new disciplines have been included. In the first ten years, before start of MAX II operation, the largest user group belonged to the physics community. After the start of MAX II operation, especially the chemistry and biosciences communities have increased considerably (*MAX-lab Background Material 2002*, ch 2 p 10). The largest geographically defined user group is the users from Uppsala, surpassing also Lund University users in pure numbers (see table 7). Apart from a slight domination of Uppsala University and Lund University users (accounting for approximately 30%), the user community of MAX-lab is spread over the whole of Sweden, and with a fairly even distribution between Nordic and non-Nordic international users. Swedish users account for approximately 55% of the user visits during the denoted years. However, the total number of commercial user visits is comparably low, only once exceeding 5% of the total during the five years in question (5.5% in the academic year 2003/04) (*MAX-lab users document*).

MAX-lab provides no travel and lodging services to its users other than the information provided on the website. The exception is the possibility for users to get reimbursed through the *EC Transnational Access to Research Infrastructures* collaboration, which grants users economical support in connection with their visits to MAX-lab, by EU money. For Swedish users and users from outside the EU, no such reimbursement is available, and there is no similar service provided neither by MAX-lab nor through any other Swedish initiative (*MAX-lab Background Material 2002*, ch 2 p 7; *Nyholm 1 interview*). Swedish users are thus not eligible for this EC reimbursement program if they go to MAX-lab (in their home country) even though they might have to travel far to get there, and so in principle it would be economically more beneficial for some users to go to another synchrotron radiation laboratory abroad to do experiments (*Uhrberg interview*).

The Uppsala connection

As noted in the previous section, Uppsala University is the single most represented institution among the MAX-lab users. The Uppsala users come from a variety of departments institutions, but the Department of Physics dominates and since 1989, no other institution or university department has had more annual individual users than the Uppsala physics department (*MAX-lab Activity Reports 1987-2006*). It has been suggested that MAX-lab is by and

<i>Report</i>	<i>No.</i>	<i>Report</i>	<i>No.</i>
1987	8	1997	32
1988	6	1998	42
1989	15	1999	60
1990	21	2000	59
1991	18	2001	60
1992	23	2002	53
1993	21	2003	52
1994	24	2004	41
1995	21	2005-06	45
1996	29	2007	44

Table 8: MAX-lab users from the Uppsala University Physics Department, 1987-2007. Figures listed in the MAX-lab Activity Reports. It is sometimes not clearly stated in the Activity Reports what time periods they cover, thus the numbers are simply reported as listed in the reports (*MAX-lab Activity Reports 1987-2007*).

large a continuation of the Uppsala spectroscopy tradition that grew strong during the 60s and 70s and culminated with Kai Siegbahn's Nobel Prize in physics (1981) (cf. quote in an earlier section). During the 80s, when the MAX-lab user community was established, several Uppsala physicists developed experiments and did extensive work at MAX-lab, and the Department of Physics in Uppsala contributed heavily to the design and construction of experimental equipment for MAX II. The synergies are doubtless strong between these two Swedish nodes in physics instrumentation. But it started off with more pending attitudes.

The 1975 conference in Gothenburg, described in the initial sections of this chapter, had no participants from Uppsala, despite the obvious connection between the Uppsala photoelectron spectroscopy program and synchrotron radiation (*Nordic Conference Report 1975*, Appendix C). Instead of participating at the conference, Kai Siegbahn wrote a letter to the organizing committee, explaining his own efforts at a synchrotron in Bonn and between the lines questioning the ability of Swedish or Nordic researchers to develop a synchrotron radiation source worth the cost.¹³⁹ The claim is that Kai Siegbahn reacted with an envious guarding of his own preserves when the invitation to the conference arrived, even "prohibiting" other Uppsala physicists to participate in the conference (*P-O Nilsson interview*). Kai Siegbahn was reportedly of the opinion that monochromatic soft x-rays from a home source was

139. The letter is appended to the report of the conference. It ends with the comment that synchrotron radiation is expensive and the suggestion that a Swedish source would perhaps not be worth the cost: "The money and personnel involved in building such machines and their maintenance should definitely not be underestimated. A possible future Swedish accelerator of this kind must naturally be competitive in performance with other future machines under present consideration abroad. A recent trip to USA and some discussions I had then with specialists in the field indicated to me a serious deficit between previously presented estimated costs and actual costs in this context" (*Nordic Conference Report 1975*, Appendix D).

entirely sufficient for all spectroscopy that would be interesting to do, and that variable wavelength was “nonsense” (*Flodström interview*). Apparently Siegbahn later changed his mind, and contributed to the resolving of some political issues in the establishing of MAX-lab (Forkman 2001, p 109).

As the opportunities at MAX-lab and of synchrotron radiation in general grew, the Uppsala physicists got on the train. The strong instrument tradition in Uppsala was matched with the development at MAX-lab, and by the time MAX II was to be built, the competencies on spectroscopy instrumentation for synchrotron radiation use had developed within some groups at the Uppsala physics department to a degree that the obvious choice for groups responsible for two of the beamlines were groups from Uppsala (*Nyholm 2 interview*). The connections between the Uppsala instrumentation tradition and the expertise on the accelerator side in Lund “have given rise to synergies you’ll have a hard time finding in other places” (*Lindau 1 interview*, confirmed by *Mårtensson 2 interview*). The argument is that a synchrotron radiation laboratory in Uppsala would not have been successful: “Depending on the traditions, there would have been too many restraining factors”, but at the same time a synchrotron radiation laboratory in Lund without the Uppsala involvement would likely not have succeeded either (*Lindau 1 interview*). Today, the Department of Physics at Uppsala University consists solely of synchrotron radiation users and a group of theorists (other physics subdisciplines such as nuclear and particle physics reside in another department, the Department of Physics and Astronomy) (*Nordgren interview*). A 2007 review of the research activities at Uppsala University highlights the instrument-tradition at the Uppsala Department of Physics and mentions MAX-lab numerous times in connection with the activities at the department:

“A particular strength of this department is in the development of new instrumentation and its associated spectroscopic techniques and applications areas. [...] Included in this is a strong emphasis on the use of synchrotron radiation [...], with much research and development being done at the Swedish national facility MAX-lab, in which Uppsala is a key and crucial participant” (*UU Evaluation 2007*, p 280)

“The committee considers that the research is world-leading in several aspects of instrument and technique development. [...] These instruments have incorporated many creative state-of-the-art ideas, are meticulously engineered and built, and have led and will lead to new scientific results in-house, at MAX-lab and at other synchrotron radiation laboratories around the world. [...] The Department has been and will continue to be a key contributor to the development at MAX-lab” (*Ibid.*, pp 282-283).

The Participating Research Teams

In the early phases of MAX II construction, when it had become definitely clear that funding for experimental equipment had to be found outside of the money granted for MAX II construction, the first formally regulated Participating Research Teams (PRT) at MAX-lab were established. The idea had been used already at MAX I, but no formal agreements or contracts had been set up. A report from 1992, requested by the council and entitled *The liability situation for the MAX-lab beamlines (Ansvarsförhållandena vid strålrören på MAX-lab)*, acknowledged that the running and maintaining of beamlines at MAX-lab so far had been the responsibility of the head applicant of the grants that paid for the respective beamlines. The report urged MAX-lab and NFR to work out contracts determining and regulating the liability, at least for future beamlines (*Karlsson-Nilsson Report 1992*). Some kind of PRT solutions were probably necessary at the time of MAX II construction, because MAX-lab didn't have enough personnel to take on responsibility for all the beamlines (*Lindau 1 interview*), and the prime technical and scientific competence on the areas in Sweden laid outside the lab organization, with user groups. Two groups from Uppsala and one from Lund emerged as the natural candidates for taking care of three of the beamlines, and took responsibility for design and construction work as well as submitting proposals to the funding agency (*Nyholm 2 interview*). Two of the three beamlines got PRT contracts associated to them. The basic conditions for the PRT agreements, including the entitled 75% of the beamtime, were disclosed in the May 1996 issue of *MAX news*, a newsletter from MAX-lab (*MAX news 9*).

The contract for one of the PRT beamlines contains information on the general capabilities of the beamline, the financing plan and the schedule for construction and commissioning, responsibilities for maintenance and user support at the beamline, distribution of beamtime, and rules for the review and follow-up of the PRT activities. The PRT is responsible for construction and commissioning of the beamline and its experimental equipment, and shall do development and construction work in continuous collaboration with MAX-lab personnel. When time for upgrades and maintenance is deducted, the PRT is entitled to 75% of the beamtime, within which the PRT is in total control of the distribution of time. 25 of these 75% shall be used for collaborative projects with groups outside the PRT. The rest of the total time, 25%, is distributed by PAC according to normal procedure, but PAC is obliged to consult with the PRT before beamtime is allocated outside the PRT time.¹⁴⁰ The PRTs are entirely responsible for maintenance and user sup-

140. This is primarily for reasons of coordination, so that one project does not receive double

port, also on beamtime allocated by PAC outside the PRT time. The contract states that after 5 years, a thorough review of the PRT activities shall be made and the conditions for distribution of beamtime be discussed and possibly renegotiated (*MAX-lab PRT contract*). Interestingly, only a copy (unsigned) of the contract for one of the beamlines has been possible to retrieve, and the head of the other PRT states firmly that he has never signed any contract¹⁴¹ (*Andersen interview*). A PRT member on the other PRT beamline shows surprising unawareness of the contract and its terms:

“I know that the PRT itself is formally approved and there is a contract. But I have never seen it, I don’t know if our duties are specified in the contract. I must admit I haven’t seen it. [...] I trust the people who put me in the contract and I never asked to see it myself. [...] I never had any difficulties, never had the kind of difficulties which would push me to look for the contract. That never happened” (*Novella Piancastelli interview*).

Despite the MAX-lab director’s assertion that PAC would never accept if the PRT time research held lower quality than that of the research done by external groups (*Mårtensson 2 interview*), there has been some negligence in the past years with regard to the PRTs’ supposed activity report to PAC (*Nyholm 2 interview*). The 2004 inquiries of the PRTs by external referee committees resulted in a cutting of the total guaranteed time to 50% for one of the PRTs, a decision that was allegedly not a disapproval of the research activities but had more to do with the relative size of the team and their evaluated need of beamtime (*Nyholm 2 interview*).

The third beamline that came about in the beginning of MAX II operation was an upgraded and moved beamline from MAX I. No PRT arrangement exists on this beamline despite heavy involvement of external groups from Finland, Uppsala and Linköping. The Finland group has a contract with MAX-lab guaranteeing them a certain amount of beamtime,¹⁴² and a representative in the Program Advisory Committee who can influence the beamtime allocation process in their favor. However, it is also stated in their contract that the scientific evaluation should be the same for their proposals as for others (*Nyholm 2 interview*). For the rest of the MAX II beamlines, no PRTs exist, but the Program Advisory Committee is supposed to acknowledge any

beamtime slots (*Nyholm 2 interview*).

141. “I have never put my signature on a contract, that much I can say.” (“Jag har i alla fall inte satt min signatur på något kontrakt. Så mycket kan jag säga”) (*Andersen interview*).

142. A contract which is in fact mentioned in the Finnish government’s Ministry of Education’s roadmap to national-level research infrastructures as a Finnish “membership” in MAX-lab (*Finland Research Infrastructures Roadmap 2009*, p 12).

investments in money and workforce done by external groups when judging their proposals. A plan to create a PRT on one other beamline with heavy involvement from an external group was reportedly prevented by MAX-lab management, who are said to have gotten second thoughts about the system after some time and thus were reluctant to make new arrangements of the sort (*P-O Nilsson interview*). It is the expressed opinion of the MAX-lab director that if a lab has the sufficient resources, it is better not to have any PRTs at all (*Mårtensson 2 interview*). A PRT head asserts that the workload associated with maintaining a beamline as a good user facility is unreasonable for a single group, and that though they have the knowledge and competence, their resources should go to research instead of maintenance of instruments (*Nordgren interview*).

Other criticism is targeted on a lack of transparency, that the PRT arrangements need to be better regulated and structured, so that other users know exactly what special arrangements regarding beamtime allocation is in place (*Fahlman, Sörensen interview*). The view that there is a lack of transparency is contested by other users (e.g. *Novella Piancastelli interview*), and by the coordinator for synchrotron radiation research: "The people who apply for beamtime here knows the rules [for PRTs and such], I dare say" (*Nyholm 2 interview*). The issue is doubtlessly controversial,¹⁴³ but at the time when the PRTs at MAX-lab were established, they were necessary. The investments of time and effort of the teams have proven to be a vital part of MAX-lab. "The PRTs have paid with real money for the operation of MAX-lab. That is the reality" (*Mårtensson 2 interview*).

Never intended to be – but evidently there

There are numerous suggestions in the material, both the general accounts on Swedish science policy (see chapter 2) and the written and oral testimony about the specific case of MAX-lab, that this laboratory came into being and developed into its present status *despite* political and economical conditions rather than because of them.

143. At the yearly meeting of the users' organization 26 September 2006, hardly anyone commented on a proposal from the users' executive committee to push the agenda of increased transparency in the PRT system (*Fahlman interview*). An interesting detail is that the chairman of the MAX-lab board reacted with surprise when asked about the PRT arrangements and claimed to know nothing about these and their exclusive investments and beamtime deals, although the PRT agreements are supposed to be made between the PRT team and the board (*Skogö interview*).

Compared to national synchrotron radiation laboratories in most other countries, very little central initiative has accompanied MAX-lab through its thirty-year history. MAX-lab was the product of the stubbornness, vision and luck of a few individuals in various locations of the system rather than comprehensive efforts on behalf of governmental research policy agencies or from Lund University. Though it is claimed that the people behind the 1978 grant application had a national facility in mind, they did most certainly not envision a laboratory that in twenty years time would run two storage rings with fifteen beamlines, some of which were state of the art in certain spectroscopic applications and some of which produced hard x-rays for crystallographic measurements. Speculation holds that such vision, if formulated, would have provoked resistance enough in the research council and elsewhere in the Swedish scientific community to make impossible the realization of it. MAX-lab's eventual success is often interpreted as being just as much enabled by the lack of grandiose plans as created by the ingenuity of key persons (*Eriksson 2, Flodström, Gidefeldt, Lindau 1, Mårtensson 2, P-O Nilsson interviews*).

Two scientific or technological preconditions stand at the center of this, combining inventiveness with prudence and certain political sensitivity. The accelerator group "gave MAX-lab good accelerator solutions right from the start" (*Mårtensson 1 interview*), a prerequisite for future success. The innovative solutions and the good contacts with local manufacturing industry made the accelerator systems at MAX-lab very cost effective. In international comparison, the MAX-lab accelerators are extraordinarily inexpensive.¹⁴⁴ The 2002 VR national facilities evaluation panel call the MAX-lab machine group "truly imaginative accelerator physicists" three times in their report (*VR Facilities Review 2002*, pp 13, 40, 41) and write that they "impress the world by their far-sighted planning of new components of the facility and the cost efficient realisation of these projects" (*Ibid.*, p 41). The chief MAX-lab accelerator designer and constructor from the start, Mikael Eriksson, is often credited with an enormous personal importance for MAX-lab's existence (*Andersen, Flodström, Gidefeldt, B Johansson, Lindau 1, Mårtensson 1, P-O Nilsson, Nordgren interviews*).

The second scientific asset was the collective strong tradition in fields that utilize synchrotron radiation and instrument development in general (*Mårtensson 1 interview*). Swedish researchers in spectroscopy and in struc-

144. A telling anecdote illustrates this well: "Jean-Louis Laclare was the one who built ESRF, and he was like a father for the synchrotron radiation community. He called me into his office and said 'you can't write anything about what your stuff costs, because that will put your colleagues in a tricky position. So stop doing that.' And I realized that he was right. Why should I make others suffer just in order to tell people how good we were?" (*M Eriksson 2 interview*).

tural studies had spent time at synchrotron radiation laboratories abroad in the 70s and brought home valuable experience. The strong Swedish (foremost Uppsala) instrument development tradition fertilized the MAX-lab environment and continued to develop into synchrotron radiation applications, in close collaboration with the machine group and the leading users (*L Johansson, Mårtensson 1, Uhrberg interviews*). Close collaboration between all actors in the lab made possible an optimization of MAX-lab as a whole. Though not delivering the world's best photon beam or facilitating many state of the art beamlines, MAX-lab preformed very well as a complete experimental system (*Andersen, Lindau 1, Mårtensson 1 interviews*).

The political effort, to pilot the early MAX project through the university bureaucracy and inflexible structures, and to later gain the support and the credibility to draft the MAX II project through the research council, was also reportedly the work of strong individuals. With time, the MAX-lab group developed skills in convincing the council and were capable of showing their qualities. Very much was a struggle, and the first MAX-lab director Bengt Forkman is considered especially important in the early process:

“It was his stubbornness, to hang onto it, to not take no for an answer from the university, from the research council, not take no for an answer from anyone, just keep on going and keep on going. So that they could really build something new out of the remnants of the nuclear physics and the LUSY accelerator. That was his great accomplishment, to build up a solid base for it all” (*Lindau 1 interview*).

Some argue (*M Eriksson 1, P-O Nilsson interviews*) that the accelerator project MAX I would never have survived on its own, with only a nuclear physics program, but that it was ‘rescued’ by synchrotron radiation. The nuclear physicists reportedly realized this and thereby enabled the synchrotron radiation program to ‘parasite’ on their accelerator project (*Flodström interview*). The work of carrying MAX II through in the council has been discussed above, under the headline ‘MAX II politics’, and it is clear that the ‘luck’ of MAX-lab of having friends and supporters in the council organization (as expressed by Forkman 2001, p 161) was an important factor in the relative political success of MAX II. When the Super MAX and Big MAX concepts eventually transformed into the final MAX II application and reached the decision-making bodies, the process to fund the accelerator was very quick (*Lindau 1, Mårtensson 1, P-O Nilsson interviews*).

The extensive user input in all scientific and technical development at MAX-lab is often mentioned as a factor of success, but perhaps it should in-

stead be viewed as an arrangement forced upon the laboratory organization by the fact that only the MAX II ring (with housing) got funded from the start and beamlines and experimental equipment required separate financing from other sources. It did, however, impose great responsibilities on the user community and with all its potential faults, a system with heavy user involvement in the initiation of new projects is by definition the most user-oriented. Every beamline project on MAX II was evaluated as separate applications, which some argue has enabled a continuous optimization of the laboratory and its scientific activity. This in turn created a system of continuous feedback through which the council, MAX-lab and the users were in constant 'triadialogue' over projects and the development of the laboratory. It is argued that the extensive user input has secured that everything built has been used to a maximum, because nothing has been developed in terms of instrumentation that wasn't originally asked for by the users (*Mårtensson interview*).

MAX-lab's position within Lund University has been continuously strengthened as the scientific visibility of the lab has increased. The scientific contributions to MAX-lab were practically inexistent at the beginning. Both the LUSY machine and the future chief accelerator constructor came from Stockholm, and the synchrotron radiation applications had no strong scientific base in Lund. Later, university initiatives have created new programs within both physics and chemistry in close collaboration with the science at MAX-lab. The political support from the central university management was, however, strong already in the 80s, especially in the process leading up to MAX II and the politics within the council (Forkman 2001, p 248). The favorable relationship between MAX-lab and Lund University has been repeatedly acknowledged by evaluation panels, as "vital for the success of MAX-lab" (*NFR Facilities Review 1997*, p 8; similarly expressed in *VR Facilities Review 2002*, p 17).

The next 'Big MAX'

Just as Super MAX emerged on drawing boards already in 1985, before MAX I was taken into operation, the accelerator group started conceptualizing the next generation MAX-lab already at the time when MAX II was commissioned:

"The thoughts of MAX IV came after we finished MAX II. It was more like a game really, it wasn't a real project but rather something academic, what

if we let go of all economic restrictions and just design a dream machine. And suddenly we realized that it wasn't unreasonable. Let's do it! Let's try and make a real project!" (*M Eriksson 2 interview*).

MAX IV emerged as the next step of MAX-lab during the last years of the 90s and beginning of the 2000s. It was first publicly described in 2004, in the brochure *MAX IV – Our Future Light Source*, which was used as background material for a workshop in Lund in September 2004 about the scientific case of MAX IV ('a three-day brainstorming'). This workshop resulted in the *Conceptual Design Report (CDR)* that was published in early 2006.

The MAX III project, mentioned before, was part of the planning for MAX IV, as a prototype for some accelerator concepts, and it showed that the novel design parameters worked in practice (*M Eriksson 2 interview*). According to most standards, the 2006 MAX IV CDR brandished a synchrotron radiation source with performance and capabilities beyond comparison at a cost well below expectations.

MAX IV is projected to be built on green field in the north east of Lund, close to the expected location of another even larger infrastructural project, the *European Spallation Source (ESS)*. The original design of MAX IV contained a dual storage ring concept, where one VUV/soft x-rays ring and one hard x-rays ring were to be placed on top of each other in the accelerator tunnel, with MAX III moved from its present location to the new facility. The linac injector system included in the concept was designed to enable a future expansion to a free electron laser. The CDR was evaluated in two steps by expert panels, on charge by VR. First, an evaluation of the technical concept was done in November 2005, to answer a big question within the council: how could the design of MAX IV could give such great brilliance, low emittance and otherwise exceptional performance compared to the sources being built today? (*Karlsson interview*). The technical evaluation report states that the design is "sound" and "congratulates MAX-lab on the innovative design concept" (*VR MAX IV Technical Evaluation 2006*, p 6). "Drawing on highly successful development of third-generation synchrotron radiation sources world-wide, the proposed set of rings offers a source an order of magnitude brighter over an unprecedented spectral range" (*Ibid.*, p 10). The second part of the evaluation concerned the scientific case, and it was done in October 2006. In its summary, the evaluation panel strongly endorsed the project:

"The Evaluation Panel is unanimous in its conclusion that the scientific case for a MAX IV facility is very strong, representing a site for intrascientific challenges as well as interscientific advances and an important re-

source for upgraded industrial research in the Nordic and possibly Baltic countries. The Evaluation Panel is clear in its recommendation to the Swedish Research Council: MAX IV should be funded to the level requested, and the funding should commence as soon as possible" (VR MAX IV *Scientific Evaluation 2006*, p 6).

Though the panel judges MAX IV a reasonable investment and a natural next development for the Nordic community in synchrotron radiation, saying that if it is not built, "it is inevitable that the Nordic research community will be negatively affected" (VR MAX IV *Scientific Evaluation 2006*, p 20), the only effect the report had was the council alerting the government of the plans and the evaluations that had been made. The government reacted by handling the issue back to VR in late 2007, with a charge to the council to "proceed in investigating the technical and economic conditions" (VR MAX IV *Report 2008*, p 4). The result of this investigation was presented in a report in June 2008, in which it was made clear that decision (from the government) to start construction of MAX IV needs to be taken "in the near future" in order to make use of the potential and the head start the project has got in the international competition, and in order to make use of the competence within today's laboratory that otherwise could be drawn away by other accelerator projects (VR MAX IV *Report 2008*, p 3).

At a second workshop about the MAX IV scientific case in the fall of 2007, where a large number of users and potential users of the MAX IV facility convened to work out requests and specifics of beamlines and experimental stations, it was made clear that the demand for hard x-rays compared to VUV and soft x-rays was far greater than previously anticipated. As a consequence, the MAX IV design was revised so that the ring for hard x-rays is to be larger and the VUV/soft x-rays ring would be replaced by an upgraded and moved MAX II ring (MAX-lab SAC *Report 2007*, VR MAX IV *Report 2008*). With the design upgrade, the performance parameters of the facility were improved even further, so that the projected performance of the hard x-rays ring now "approaches the theoretical limit" for storage rings (especially with regard to emittance), which means that it will be among the world-leading "for a long time" (VR MAX IV *Report 2008*, p 7). The latest cost estimations hold that total construction costs of the facility will be about 2.7 billion SEK (\approx 270 MEuro), and that annual operational costs will be about three times as much as the current laboratory, which is approximately 85 million SEK (VR MAX IV *Report 2008*, p 8).

The scientific support for MAX IV is broad. In the 2008 VR report, some prominent Swedish institutions state their commitment to MAX IV. They

include the Chalmers University of Technology in Gothenburg, who propose a long-term commitment including the establishing of a 'Chalmers – Campus Lund' at MAX IV, the Karolinska Institute in Stockholm (with their over 70 researchers in structural biology for whom x-ray crystallography is an important experimental method), the Swedish Museum of Natural History, which reports a rapidly growing interest in micro- and nanotomographic methods for geology, paleontology, zoology and botany, and of course Uppsala University with its strong spectroscopy tradition and the continuing of this tradition including the "strategic commitments" of establishing centers for instrument development, spectroscopy and materials science (*VR MAX IV Report 2008*, pp 14-15). Also foreign initiatives are presented in the report. Representatives of synchrotron radiation users in the Nordic and Baltic countries have established the Nordic/Baltic Synchrotron Radiation Initiative (NSRI) who evaluates and plans for future Nordic and Baltic use of synchrotron radiation facilities, and MAX IV is considered a cornerstone in their initiative (*VR MAX IV Report 2008*, p 16). Also, an Indian(!) group has expressed their interest, including the proposal of building a beamline of their own (*Ibid.*, p 18).

According to most people involved, MAX IV is a kind of natural next step for Swedish synchrotron radiation science. One interpretation holds that a Swedish decision not to build MAX IV would mean an active discontinuation of synchrotron radiation in Sweden, though continuously over a long period of time (*Mårtensson 2, Uhrberg interviews*). It has been expressed that MAX IV is the real test of ability to handle issues of large scale infrastructure on behalf of the Swedish science policy system; MAX-lab so far having developed step-by-step without ever a comprehensive, all-inclusive, large initiative taken from governmental level. So far however, the system seems to be reacting according to known patterns.

History repeats itself

The perceived good prospects of the realization of the MAX IV plans in Lund is a pawn in the game to get the European Spallation Source (ESS)¹⁴⁵ located

145. The ESS is a concept of a new neutron source for advanced study of various materials. In 1999, the OECD Global Science Forum suggested that Europe, Japan and the United States construct one such source each. Sweden and Lund has been a candidate for hosting the facility since 2002 (*Larsson ESS Report 2005*, pp 14-16), with articulated governmental support since 2006. The government has pledged to contribute 30% of the funding for construction (which is estimated to be 1,3 billion Euro) and 10% of the running costs should it be built in Lund (*Swedish governmen-*

to Lund. Government representatives and Lund University officials argue that MAX IV would strengthen the case for Lund in the competition for ESS (e.g. *Larsson ESS Report 2005*; *VR MAX IV Report 2008*), and that there are several possible synergies between two complementary facilities in overlapping areas of research. Given what the history of MAX-lab can teach about the generally weak and diversified Swedish science policy system, it is remarkable that two science infrastructure projects, both of enormous size and scope in Swedish comparison, are proposed at the same time and furthermore both planned for localization in Lund. The costs of constructing and running these facilities surpass every publicly funded basic science project in Swedish history, and the people involved are most certainly correct in their questioning of the ability of the Swedish science policy system to handle, fund and organize these two projects, not least simultaneously.

In the 2006 evaluation of the MAX IV scientific case, a warning is issued in connection with the proposed budget for the facility, that “seems low” for the size and level of complexity of the facility (*VR MAX IV Scientific Evaluation 2006*, p 23). It is clear from the evaluation that the suggested MAX IV laboratory requires a greater and more comprehensive financing profile and stringent organizational structure compared to the existing MAX-lab. This is also acknowledged by MAX-lab management and users, and other commentators (*M Eriksson 2, Mårtensson 2, Fahlman, Flodström, Honeth, B Johansson, Lindau 1, Nyholm 2, Nordgren, Skogö interviews*). The similarities with the warnings issued in connection with the MAX II upgrade (see above) are striking; international experts commend the innovativeness and efficiency of the small and informal organization of the existing laboratory but states explicitly that it won’t suffice at the projected next generation MAX-lab. However, the Swedish Research Council seems to be of a different opinion. In its 2008 report on the MAX IV project, the council envisions a funding and operation of the MAX IV facility in a manner similar to that of the existing MAX-lab. It is said that the “accelerator, storage ring and other permanent facilities” (a projected total of 947 million SEK out of 2,7 billion) should be “largely financed directly through new governmental grants” but for beamlines and experimental equipment (a projected total of 733 million SEK) it seems the council is prepared to rely on user groups to apply for funding for the equipment separately from the central MAX IV budget, just as the case of MAX II. In the report, the council even expresses the hope that beamlines and experi-

tal research bill 2008, p 194), but at the time of writing, no political agreements have been made and a lot of design and development work remains before a construction start is possible. The American counterpart, the Spallation Neutron Source (SNS) at Oak Ridge National Laboratory, opened in 2006 (*DOE National Labs Brochure 2008*, p 15). In Japan, a similar facility, the J-Parc, is planned to start operation in 2009 (Ikeda 2009, p 1).

mental equipment to a significant part will be paid for by research financiers in other countries, along the lines of a 'fair return' system¹⁴⁶ (VR MAX IV Report 2008, pp 9-11).

The speculations around a 'fair return' system builds on the fact that the present MAX-lab is used by about 20% by the other Nordic and Baltic countries, and if the same share of the use could be anticipated for MAX IV, the other Nordic and Baltic countries could be expected to contribute with operating costs of about 20%, namely 55 million SEK (Ibid., pp 10-11). The authors of the report seemingly neglect the fact that such a discussion, however speculative, raises difficult questions about fair return policy. If the other Nordic and Baltic countries are invited to share the operating costs of the facility to a degree or percentage that roughly corresponds to their share of the use of the facility, the question will immediately arise about the policy for foreign users from countries for whom it has never been of any interest to contribute financially to the MAX IV facility. At ESRF, where 'fair return' is extensively used, beamtime is only in exceptional cases awarded to groups outside the member countries (see chapter 6), but the ESRF has 17 member countries and covers well the European scientific base for the facility. Running MAX IV as a facility exclusively available to Nordic and Baltic groups may be an option, but it runs against the philosophy of most synchrotron radiation facilities. Furthermore, if shares of beamtime is too closely associated with budget contribution percentages, it will inevitably be seen by some as payment for beamtime, raise questions in the scientific community and possibly be seen as a hijacking of the organized peer review system. Furthermore, the 2008 VR report does only treat the operational costs as subject to fair return policy and says nothing about similar share of construction costs. Remarkably, despite the criticism and concern voiced constantly by evaluation panels ever since the first assessment of the MAX I project in 1978, that MAX-lab needs a more comprehensive funding and governance model, the council and the government continue to view the laboratory only as a project among many.

A decision of sorts for the realization of the MAX IV project was made in April 2009, when representatives of Lund University, the Swedish Research Council, the Swedish Governmental Agency for Innovation Systems (VINNOVA) and the Skåne Regional Council (Region Skåne) signed a 'declaration of intent' for the establishment of the facility. This agreement does however not mean that the facility is fully financed; only 750 million SEK out of the estimated total of 2.7 billion is pledged by the partners. The declaration states that the four partners will work to "secure funding" for the facility, that

146. For laboratories organized as international collaborations, 'fair return' (or 'Juste Retour') systems are sometimes put in place. They regulate overall distribution of beamtime between member countries on basis of the budget contributions made by these countries. See further chapter 6.

beamlines and experimental stations are supposed to be funded by “other partners” including research groups with their own individual grants. There is no mention of how the operational costs are supposed to be covered and by whom (*MAX IV declaration of intent 2009*).

Conclusions

The most recent developments at MAX-lab, accounted for in the last sections, are sequel examples of the laboratory’s standing in Swedish science and science policy. Continuously hailed in evaluations and reports and continuously expanding its ambitions and capacity, it lives on piecemeal funding and a passive governance strategy from its steward agency and principal financer, ultimately grounded in the limited ability of the Swedish science policy system to make discontinuous priorities and commitments. This is also the backdrop to the main conclusion to be drawn from this chapter: MAX-lab has come into being *despite* rather than *because of* the science policy system in which it operates, and its ability to develop and search out for new opportunities in this seemingly unfriendly context has become its prime virtue.

This conclusive observation has significant importance for the overall argument of the thesis. The evolutionary process through which MAX-lab has come into being and developed is an example of the pragmatic and inventive nature of synchrotron radiation laboratories that signify them and arguably make them apt to the changed dynamics of science and science policy argued for in previous chapters. Most – if not all – synchrotron radiation laboratories show these characteristics; as noted in chapter 3 and elsewhere synchrotron radiation labs are characterized by being scientifically and technologically ‘generic’, ‘modular’ and ‘substitutional’. This is fundamentally due to their basic technical setup with a number of replaceable and renewable individual beamlines and experimental stations, and it can be utilized for continuous ‘optimization’ of the laboratory and its experimental program in accordance with trends and drifts in the sciences, in politics and in society at large. In MAX-lab’s case it is both especially visible and its cause particularly evident. The corresponding funding and governance structure has by and large made MAX-lab embrace this feature as a strategy and a means to make constructive use of the situation.

The chief conclusion is thus that a small country like Sweden, despite shortcomings in its funding and policy system and despite the lack of overarching initiatives, may be able to create and develop valuable infrastructures that take part in a strengthening of domestic science on several fronts and be-

comes internationally competitive through a self-renewal process. The conclusion is possible to generalize to other small countries. In the last chapter, it is discussed in a broader context.

The process described is instigated and sustained by one type of *scientific entrepreneurs* – when contextual and structural mechanisms lack or are insufficient the work of scientific entrepreneurs may fill the gap.¹⁴⁷ In this particular case they are found both inhouse, as key individuals driving technical and scientific developments and promoting them in political spheres, and in the user community, which is frequently highlighted as particularly influential and important for MAX-lab. Furthermore, the apparent inability of the political system to respond to the scientific and technological developments is an interesting example of the duality of the bottom-up scientific and top-down political governance structures of synchrotron radiation laboratories (as noted in chapter 4), also because it seems to foster the kind of scientific entrepreneurs that emerge as important in this case study.

However, the reason for MAX-lab's existence cannot be entirely attributed to the work of ingenious scientific entrepreneurs, although they are indeed noticeable in the story. And the weaknesses of the Swedish science policy system – though certainly providing room for scientific entrepreneurs to put their ambitions to use – cannot be the sole structural or contextual explanation, neither for MAX-lab's existence, nor its alleged accomplishments. First of all, there is a science policy prehistory. The resource concentration at CERN in the early 70s has been noted in chapter 2 as causing the closure of national accelerator programs in Europe. Sweden was no exception, and it is clear that the shutting down of the LUSY project and its surrounding research program was what made domestic accelerator construction competence available to synchrotron radiation and ultimately led to the establishment of MAX-lab.

Second, the favorable scientific base in Sweden, in the shape of a strong spectroscopy tradition with instrument design and construction at its core, a well-established scientific field with world-leading capabilities, certainly fertilized the MAX-lab scientific environment. Within it, scientific entrepreneurs are possible to identify, most notably in the 90s flourishing of the synergies between the Uppsala physics department and MAX-lab and the establishment of Participating Research Teams. There are also contextual implications; the spectroscopy tradition in Uppsala was not only a fertilizer of the scientific

147. This is not to say that scientific entrepreneurs only exist or play noticeable roles when structural mechanisms lack or are insufficient. Scientific entrepreneurs take on different roles in different contexts, as shown also in the two other case studies, but their apparent role in the creation and development of MAX-lab is important enough for the general conclusions of this chapter to point it out explicitly.

program and the technical development at MAX-lab, it is also said to have benefited greatly from its involvement. It might therefore be viewed as a particular example of the collectivization and sophistication trends in science, as interpreted in the analytical framework of this thesis. Spectroscopy was already 'sophisticated' by its very existence – as a scientific area it is extraordinarily technology-intensive – but its merger with synchrotron radiation is an example of the fundamental collectivization force of modern scientific infrastructure to attract and collect disciplines and make them dependent of large-scale laboratory resources. It is perhaps not the most telling or spectacular example since it concerns a discipline already technically sophisticated – the life sciences disciplines involved are certainly more manifest examples – but it shows how these trends are at work on different levels.

In this chapter, another particular example of the emergence and establishment of synchrotron radiation in science and science policy has been presented. The main conclusion is the ability that seems to have come with this gradual development to pragmatically search out new and alternative routes and areas for developments. Thus the case of MAX-lab as presented in this chapter is another example of the dynamics and flexibility of synchrotron radiation as a (potentially) large-scale research infrastructure asset. In this case the focus is not its dynamic development in contrast and connection with high energy physics, but the opportunity it provides also to smaller countries – and even those with significant weaknesses regarding large-scale strategic commitments – to equip themselves with strategically important research infrastructures. It provides a second detailed example of the governance duality of bottom-up, scientific progress and top-down, political steering, or perhaps in this case the lack thereof. In the last case study, the center of attention is radically shifted but the story is continued, as it shows the efforts to institutionalize a dynamic and flexible scientific and technological enterprise in a large scale project with deep marks of big politics.

Bureaucracy and power at the European Synchrotron Radiation Facility

A European project

The European Synchrotron Radiation Facility (ESRF) is a joint European laboratory located in Grenoble, France and collectively owned by 17 member countries. It is built on a multilateral agreement between the governments of the 17 member countries, establishing the facility as a French private company (*société civile*), owned by the organizations through which the countries are members. The laboratory operates on a budget of annual contributions from the member organizations, decided in advance and corresponding to their shares in the company. The laboratory has the mission of providing synchrotron radiation, instrumentation to use it and sufficient scientific and technical support to scientists from the member countries. At present, over 40 independent experimental stations are provided to users, ranging over most scientific and technical areas associated with synchrotron radiation, and several of them are considered world leading and state-of-the-art in the synchrotron radiation community.

The main reason for the choice of ESRF as a case in this thesis is its contrast to the other cases and to most synchrotron radiation laboratories in the world. The ESRF was conceptualized, designed and built through a comprehensive effort, well-defined from the start and fully funded by the member countries according to rigorously negotiated agreements. The goal to take and maintain the lead among the world's synchrotron radiation laboratories was clearly articulated right from the start and is said to have guided the operation of the laboratory in all aspects since its opening in 1994. The ESRF stands out also organizationally and politically, as the result of a lengthy and complicated political process to make European governments collaborate towards a common goal and overcoming issues of national prestige and protectionism.

The 50s and 60s had seen the creation of quite a few European intergovernmental collaborative projects in science, such as CERN, JRC (Joint Research Centre, primarily for nuclear physics) and ESRO (European Space Research Organization) (Krige 2003, p 899). The European countries, however, lacked

European Synchrotron Radiation Facility, inaugurated 1994

Approximate number of staff: 600

Approximate number of annual external users: 6,000

Approximate number of operational beamlines: 40

Funded by the 12 member countries and 7 associate countries.

Table 9: ESRF basic facts
(*ESRF Highlights 2008*)

a forum where collaborations and joint interests in basic research could be discussed and the common good for European science could be paid attention to without being tied to a specific field or facility project. This gap was at least in part filled in 1974 with the formation of the European Science Foundation (Grande and Peschke 1999, p 46). Aiming at the establishment of tighter and better links between the European scientific communities by “advancement of cooperation in basic research, the promotion of mobility of research workers, access to a free flow of ideas and information, better exploitation of existing unique research facilities, as well as the support of joint projects and the provision of expensive, specialised services” (Maciotti 1975, p 301), the ESF became the natural ground for the first initiatives to a collaborative effort in the field of synchrotron radiation on European level.

In October 1975, Professor W.R.S. Garton at the Imperial College, London, suggested to the ESF to take on as part of its mission to facilitate and organize European collaboration in synchrotron radiation. Garton expressed concerns over the risk of duplication of efforts in the field in different European countries and a desire to make synchrotron radiation available to European scientists whose home country did not own a source (Schmied 1990a, p 18). The ESF Executive Council decided to set up a ‘working group’ on synchrotron radiation, consisting of 14 scientists from 7 European countries, which began the work to create a community of European synchrotron radiation researchers (Schmied 1990a, p 18). The group met twice during the years 1976 and 1977, but it was soon realized that the means at their disposal were insufficient for promoting any initiatives beyond multilateral exchange of information and acknowledgements of the divergence of the various national interests in the field. A smaller working group was formed in 1977 with the explicit task of prepare a report on the future of synchrotron radiation in Europe, inspired by a report on synchrotron radiation in the US (*NAS Assessment Report 1976*) that recommended the construction of a new ‘national’ US facility and the upgrading of existing ones. The report of the ESF working group, entitled *Synchrotron Radiation – a Perspective view for Europe*, was published in the fall of 1977 and stated explicitly that neither the qualitative nor quantitative demand of European scientists for synchrotron radiation beamtime could be met with the national sources in operation and planned at the time. The re-

port concluded that the creation of a pan-European facility with state-of-the-art beam and instrumentation characteristics was indeed desirable:

“The demand for synchrotron radiation as a tool of fundamental research in physics, chemistry and biology is rapidly growing. [...] The present situation in Europe with regard to access to synchrotron radiation sources is unsatisfactory. [...] Action of two sorts is therefore recommended to ensure the continuation and future development of the high standard European activities in this field: one concerns better use of existing facilities until a new dedicated source is available, and the other concerns the construction of a European synchrotron radiation source so that requirements, which cannot be met by the existing national sources from the mid eighties onwards, can be satisfied” (*ESF Perspective Study 1977*, p 66).

Conceptualizing

The original working group adopted the *Perspective* report in October 1977 and recommended the ESF to set up an enlarged ‘ad hoc committee’ to start working on the details of the proposed facility. It was agreed in the ESF Executive Council and General Assembly to form this ad hoc committee out of the existing working group, and charge it with the task of carrying out a ‘feasibility study’ for the project, that by this time had gotten the name *ESRF – the European Synchrotron Radiation Facility*. ‘Machine’ and ‘instruments’ subgroups were also formed to do specialized studies in their respective realms (Schmied 1990a, p 20). During the following years, the efforts to form a common European synchrotron radiation community were intensified, gathering scientists from the European scientific communities at a number of workshops and a continuous production of brochures and newsletters (*Witte interview*). An intensive debate over the fundamental characteristics of the ESRF machine took place during the first year of the ad hoc committee’s work, along the lines of the classic divide between VUV/soft x-ray and hard x-ray machines (see chapters 3, 4 and 5). With very few exceptions the existing machines in Europe at the time were VUV/soft x-ray machines, and hence the scientific community that had started to mobilize around the ESRF plans had their main interest and competence in the VUV and soft x-ray region. In the debate, it was anticipated that if the new facility was to become merely a ‘big VUV/soft x-ray machine’, the support for a large and expensive (and politically strenuous) European project would fail, since designs for VUV/soft x-ray machines and the means to build them was

well in the reach of any single European country (cf. MAX-lab, chapter 5). On the other hand, since the participating scientific communities from the European countries had their most experience in VUV and soft x-rays and therefore were most interested in corresponding scientific utilities, the valuable support of established scientists and groups would perhaps be lost if the project was given exclusive focus on the hard x-ray regime (Schmied 1990a, pp 20-21; *Witte talk 2001*). This debate and the arguments drafted have some implications for the political process accompanying the creation of the facility, also on long term.

At a two-week workshop in Aussois, France in May 1979, gathering 68 scientists from all over Europe, details of a design of the ESRF was drafted and formulated in a four-volume document (the *ESF Feasibility Study 1979*), in all 584 pages of printed text. In this study, the facility was outlined as a 'complete' synchrotron radiation laboratory, not limited to the x-ray regime but covering the whole available spectrum. A number of prospective experiments in the fields of physics, materials science, chemistry and life sciences were listed, together with specifications of desirable machine and instrumentation parameters for carrying out these experiments, described in "considerable detail" (Schmied 1990a, pp 21-22). The ad hoc committee approved the document in the summer of 1979 along with a recommendation to the ESF to "find means of turning this exciting proposal into reality" (*Ibid.*, p 22). At the meeting of the general assembly of the ESF on 6 and 7 November 1979, the chairman of the ad hoc committee Yves Farge presented the Feasibility Study and its conclusions. Though the representatives of the ESF member organizations were "deeply interested" (*Ibid.*, p 22) in the project, they had no mandate to make decisions about its realization. Instead Friedrich Schneider, a former secretary of the ESF, was given the task of moving the project up to the governmental level by directly approach governmental representatives (*Ibid.*, p 22). The machine design proposed in the Feasibility Study was focused on bending magnets as the principal sources of radiation at the laboratory, with insertion devices as only complementary (*ESF Feasibility Study 1979*, p 14; *Witte talk 2001*).

In these first four or five years of activity under the ESF umbrella, the efforts were primarily concentrated on the gathering and formation of a synchrotron radiation community to constitute a solid scientific base of the project. Once a machine and instrumentation design had been worked out in detail however and it became clear that the ESF as such had no authorization of carrying the project out in practice, the 'real' political process started. The mounting and maintaining of a scientific base for the project carried on, with developments and progress on the machine, instruments and experiments side. But the political procedures of locating the prospective facility

and negotiations over investment contributions took over as the most important for the realization of the project. As an element in the process, a so-called *Progress Committee* was formed in 1982, made up of delegations from Belgium, Denmark, France, Finland, Federal Republic of Germany, Ireland, Italy, Sweden and the United Kingdom. This committee was to be the forum for discussion and progress on the political level, with the ad hoc committee still working with the scientific case (Schmied 1990b, p 24).

High-level politics

As described in chapter 2, the procedures leading up to multilateral agreements for intergovernmental collaborations are complicated matters, governed by a lot of national prestige, stubbornness and self-interest, and when collaborations are to be set up external to everything already established, on political 'green field', the rules of engagement need to be worked out simultaneously with the process. Krige's (2003, p 904) argument that the desire to collaborate is almost exclusively motivated by self-interest, both on government level and within the scientific communities of the countries involved (see chapter 2), is confirmed by some of the processes of intergovernmental politics leading up to the creation of the ESRF. The expectations of return for investment among the various countries, and how it later plays out, is one of the prime interests in such processes, but general international politics clearly has a role to play, as described in chapter 2. The question of individual nations' commitment and gain greatly influenced and guided the negotiations and decision-making, not only in the planning and foundation phase but also in the running of the facility, and the 70s and 80s 'Franco-German entente' had an overarching role to play in the eventual resolving of the process (see chapter 2 and below).

The 'return for investment' logic played out especially in the site-selection procedure, a general pattern of European scientific collaborations (Krige 2003, p 904), and site selection and budget negotiations are processes that tend to go hand in hand. Among the representatives and delegations in the various committees around the ESRF project, two points of view existed regarding what exact principles should guide the site selection process. One was the so-called 'scientific' way of choosing a site for the facility, which included the assessment of all proposals for location according to 'objective' criteria, by the work of an expert referee committee. The other one, called a "crystallization process" (Schmied 1990b, p 26), relied on the political objectives of two (or a few) big countries to bilaterally (or multilaterally) agree on the construction

of the facility, committing to a joint investment of at least 50% of the costs, and also decide on a site. Other countries could then be invited to join. The countries that had made their own proposals for a site would have the choice to either leave the field, thereby revealing that their interest was not scientific but merely a wish to enjoy the advantages of hosting, or give in and sign up for the site proposed by the bigger countries. The site-selection for CERN II in the 60s (see chapter 2) is an example of a failure to carry out the 'scientific' alternative of process, and as we shall see, the 'scientific' procedure ultimately failed also in the ESRF case, and the 'crystallization' process had its way.

Possible sites for the facility had been proposed already in 1979 and 1980. Italy had offered Trieste as a location, proposing also to take 50%(!) of the investment costs should the facility be built there, but also Dortmund in Germany, Risø in Denmark (supported also by Sweden and Norway), and Daresbury in the UK were proposed sites (Dickson 1984a, p 392). The CERN site in Geneva also emerged as an alternative, with its potential cost reductions from gains of effectiveness and the anticipated synergy effects. CERN had recently decommissioned one accelerator (the one preceding the main CERN II machine) and the abandoned accelerator tunnel was suggested as a possible location for the ESRF machine. The cost savings and other gains from building the facility at a site already equipped with infrastructure and technical facilities were obvious, but the disadvantages were perceived as considerable. First of all, the accelerator tunnel in question had hosted a high energy physics machine, which meant that the space available for instrumentation around the ring was limited. Furthermore, the scientists and leaders of CERN were not "unanimously supporting" the expanding of the CERN mission to cover synchrotron radiation (Schmied 1990b, p 24). The perhaps most important argument however – and most interesting, in a wider perspective – against location at the CERN site was that many synchrotron radiation scientists "had not enjoyed the symbiosis with the high energy physics community" (Ibid., p 24), and were keen on establishing the emerging synchrotron radiation community as independent now that they had the hope of getting their own machine (Ibid., p 24; *Witte interview*). This was not only a psychological issue for the community; a collocation with the CERN facilities involved the use of the same injector as CERN's larger machines, which inevitably would mean subordination to the high energy physics program.¹⁴⁸ An ESRF built at the CERN site would also have to be constructed and commissioned in accordance with the CERN accelerator construction 'cycle', in this particular case meaning that it would partly rely on a go-ahead for the next

148. Cf. the synchrotron radiation program at Stanford, which was dependent on the SLAC linac for injection, also used to inject the high energy physics machines PEP and the SLC (chapter 4).

CERN accelerator (*Large Electron Positron Collider, LEP*), a decision that of course had its own logic and procedure, both scientifically and politically, entirely external to the already complicated decision-making process of the ESRF. In 1981 it was decided by the ad hoc committee not to go for the CERN location proposal. At the same time, France proposed Strasbourg as a site, a choice that had some logic to it, given that the ESF had its headquarters there (Schmied 1990b, p 24).

In 1982, several sites had been proposed but none decided upon. At the same time, the scientific development had taken several steps with the so-called *Als-Nielsen Report* (see below), and a growing impatience had spread in the groups working with the project. In October, the chairman of the ad hoc committee therefore proposed that a complete design be developed by a new working group, and should no decision have been made on the construction of the facility before the end of 1984, "the whole effort should be stopped" (Schmied 1990b, p 25). This group was named the *ESRP (European Synchrotron Radiation Project)* group and its work was located at CERN, which was now considered "neutral ground" again after the idea of locating the ESRF there had been shelved (Ibid., p 25). The group was charged with the task of preparing a "site independent project proposal" including a scientific case, specification of the instruments and beamlines, outline design of the machine, as well as outlines of buildings, services and other infrastructure. Part of the plan with this detailed mission was for the ESRP group to come up with a concept against which the proposed sites so far could be judged (i.e. pursuing the 'scientific' way of site-selection) (Ibid., p 25). In early 1984, the group received a letter from the director of the ILL (*Institute Laue-Langevin*) in Grenoble, suggesting the co-location of the ESRF with ILL (Haensel 1988, p 72). Local pork-barrel politics in the Isère region of Grenoble, including personal favors between the governor of the region and the French president Mitterand, is claimed to have influenced the choice of the French government to switch Strasbourg for Grenoble as their proposed site for the facility (Witte interview).

The Franco-German entente

At the end of the 70s, the Federal Republic of Germany was not only economically and politically a dominant force in (Western) Europe but also a prominent nation in physics, including both high energy physics (and thus accelerator development) and the emerging field of synchrotron radiation

research.¹⁴⁹ Several international and German domestic infrastructure projects were on proposal stage and competed for funds. In 1980, the German Minister of Research and Technology, V. Hauff, appointed an expert committee to investigate these projects and rank them in order of importance for Germany, an early 'roadmap' for scientific infrastructure.¹⁵⁰ As it turned out, the ESRF was regarded only number eight in importance among ten big projects, with the next accelerators at both CERN and DESY above it,¹⁵¹ as well as the proposed neutron spallation source in Jülich near Cologne (Dickson 1984a, p 392; Schmied 1990b, p 23). That one of Europe's key players in synchrotron radiation officially placed its priorities elsewhere than the proposed joint commitment in synchrotron radiation was obviously a fallback for the whole ESRF project. But the ad hoc committee and the progress group continued working on refinements of the designs and advocating the 'scientific' site selection process to be carried out.

In order to continue the establishing and formation of a scientific base of the project, meetings and conferences were organized in different countries around a variety of topics. Modifications of the design of the machine, as well as updates of the scientific case, were presented and discussed. In particular a 1982 report edited by J. Als-Nielsen had great importance for the scientific development and possibly also the political, containing a proposal to change the machine design to an all insertion device machine (Schmied 1990b, pp 24-25). The report made the refined design cutting-edge since its investigations showed new possible applications of radiation from undulators and the overall enhancement of performance quality of the machine and instrumentation that would be possible with an all insertion device design. These scientific and technical improvements put the concept of creating a facility with unprecedented performance parameters – in short a machine surpassing the existing European sources with great margin – into a concrete design proposal, and showed how it could be done in practice. It had been expressed already in the 1977 Perspective Study that the eventual design parameters should go beyond existing projects (*ESF Perspective Study 1977*, p 9). During

149. By 1980, DESY had its original electron synchrotron in operation, partly for light production (as mentioned in the beginning) and a synchrotron radiation program in operation on its DORIS storage ring. The storage ring for synchrotron radiation BESSY in Berlin was under construction and plans existed to make use of the newly completed PETRA ring at DESY (Winick 1980, p 30).

150. Most (Western) European countries today have such roadmaps, as does Europe as a whole through ESFRI (European Strategy Forum on Research Infrastructure) and of course the United States through the Department of Energy and the National Labs (e.g. *ESFRI Roadmap Report 2006*; *DOE Facilities 20-Year Outlook 2003*; *KFI Guide 2008*; *Finland Research Infrastructures Roadmap 2009*).

151. A fact that also gives a hint of the status high energy physics held at the time, cf. chapter 2.

the following years, a slow but clear shift in the motivation, purpose and goals of the ESRF project took place. The debate over wavelength range (see above) can be seen as a prologue to this, as one of the arguments of the discussion contained a concern that the ESRF, if it were to be merely a 'big VUV/soft x-ray machine', would not provide enough new and advanced opportunities for the European scientific communities to motivate an expensive and politically complicated effort by the countries involved. A switch to an all insertion device machine design, however, paved the way for the committees and teams to think anew, and the Als-Nielsen report pointed clearly at opportunities of doing experiments previously impossible.

On 26 October 1984, the French and German delegations of the Progress Committee declared that they had reached agreement to build the ESRF in Grenoble, and to share the main part of the construction costs of the facility. Other countries were invited to join. Only a few weeks earlier, the ad hoc committee had adopted a resolution recommending that Grenoble was not chosen as a location, instead advocating search for a site to fulfill certain predefined conditions (Schmied 1990b, p 26). Obviously, while the pursuit of the so-called 'scientific' way of choosing a site had been going on for quite some years without even coming close to a conclusion, the 'crystallization' process – working according to political rather than scientific logic – had had its way and eventually produced the necessary bilateral agreement. Only speculations exist over what may have made Germany change its attitude from regarding the ESRF only ranked eight in importance among proposed scientific infrastructure projects in 1980 to coming in as the next biggest investor in the facility in 1984 (after France, who pays a somewhat larger share because it is the host country, see below).

The importance of the overall (Western) European political context should probably not be underestimated. The Franco-German entente that had started to lead Europe on the road towards the Maastricht Treaty, the European Monetary Union and the deepened European Union collaboration of the 2000s, had already established itself as the all-pervading force of European integration (see chapter 2). Building on the postwar voluntary subordination of the Federal Republic of Germany (Cruise O'Brien 1997, p 80; Middlemas 1995, p 117) and the joint determination of the two countries to emerge out of the financial stagnation through the creation of the "real Europe" (Middlemas 1995, p 88), and with Italy and the United Kingdom turning inwards or, in the case of the latter, glancing over the Atlantic Ocean (Ibid., pp 74-75, 80, 89-90), this Franco-German entente was the alliance with which it seem every European collaborative effort in the 70s and forward would stand or fall (see chapter 2 and Judt 2005, p 529; Middlemas 1995, pp 323-329). In the European Community, the initiatives originating from the work of Étienne

Davignon (see chapter 2) had also laid ground for a European collaborative scientific and technological framework, in which the political decision to establish the ESRF was well-fitted, especially given the renewed ambitions to create a world-leading facility.

The ESRP group presented their draft report – approved by 59 scientists at an ESRP meeting – on the October 26 meeting of the Progress Committee. This report expressed the firm view that the ESRF most important of all should be a better source with better instrumentation than was currently available at the national sources, making entirely new experiments possible (Schmied 1990b, p 26; *Witte talk 2001*). All of the previous reports had been clear on the point that the ESRF not only should expand the number of beamlines available to European scientists but also expand the experimental opportunities beyond what the existing facilities in Europe could offer. By this report, however, the aim of taking the world lead was clearly articulated. It is reasonable to suggest that the shift in aim and scope of the ESRF also had some part in the change of attitude in Germany (*Witte interview*). Speculation has it that Germany's lukewarm attitude to the ESRF in 1980 was in part because of the lack of clear advantages of a European synchrotron radiation source compared to Germany's existing programs on DESY in Hamburg and BESSY in Berlin. With the Als-Nielsen report and the work of the ESRP however, it became clear that not only was the ambition to create a laboratory with a state of the art light source and accompanying world class instrumentation, it was also technically feasible and included a strong scientific case. The interest in synchrotron radiation was growing among the German scientific community and with it grew the support for a state-of-the-art facility (Dickson 1984a, p 392).

The Franco-German agreement over ESRF location and financing of course had a particular set of circumstances and included bargains of its own. Both countries had interest in hosting the European Transonic Wind Tunnel (ETW), a test facility for aircraft. The German lobby for the "application oriented" ETW seem to have been stronger than the domestic ESRF lobby (*Witte talk*). The plans at the federal research center in Jülich, Germany, to build a neutron spallation source had made the Jülich candidature for ESRF (see above) only "half-heartedly" supported on site (*Ibid.*). The October 1984 agreement between France and Germany is said to have entailed the pledge to build the ETW in Cologne while giving the ESRF to France (Papon 2004, p 64). Further strengthening the deal for Germany's part was the fact that the French proposal at the time still was to build the ESRF in Strasbourg, a good site from a German perspective as Strasbourg is situated practically 'at' the German border and so a location there would allow German ESRF scientists to live in Germany, thereby avoiding all complicating features of mov-

ing across borders, such as tax rules, schools and other services, and general cultural barriers (*Witte interview*).

As mentioned, the negotiations were somewhat influenced also by local politics. In addition to the pork barrel politics deal between the governor of Isère Louis Mermaz and president Mitterand, the director of the Commissariat à l'Energie Atomique (CEA) Institut de la Recherche Fondamentale, Jules Horowitz, was active behind the stage. The CEA, which was to become one of the French member organizations of the ESRF,¹⁵² was not represented in Strasbourg but well-established in Grenoble, both with their CENG nuclear studies center (Centre d'Études Nucléaires de Grenoble) and through their involvement in ILL, and Horowitz was in favor of collocation. It is said that Horowitz also wanted the future ESRF to have a similar legal status and organization as the ILL, something he tried to ensure by making the two institutes locate side by side. Jules Horowitz, later to become the first chairman of the ESRF council, "had access to high decision levels" (*Witte talk 2001*; confirmed by Dickson 1984b, pp 391-392).

Though confirming an already established collaborative pattern of Franco-German relations, the bilateral agreement of October 1984 took many of the prospective collaborating countries with surprise, and caused some resentment. Especially the Italian delegation to the Progress Committee, who still argued strongly for the Trieste site, and the Nordic countries, who reportedly felt run over by the larger countries, reacted strongly (Dickson 1984b, p 1295). A lot of work had been put into the systematic effort to evaluate all the proposed sites, and members of the Progress Committee who advocated the 'scientific' way of site-selection showed some indignation. The Progress Committee decided to carry on with the comparative evaluation of the different sites still under discussion, in order to "calm the spirits" (*Witte talk 2001*). However, no particular site emerged as preferable from this process, and as the months passed, the Franco-German proposal was gradually accepted and became the foundation for future work (*Ibid.*).

The foundation phase

By December 1985, the Grenoble proposal had matured sufficiently in the hallways of the European synchrotron radiation community, and the political process had come far enough for some countries to join. The signing

152. The member countries are represented in the ESRF through the *Contracting Parties*, domestic organizations of the countries' choice. (see below).

	<i>Constr.</i>	<i>Oper.</i>
France	33%	27.5%
Federal Republic of Germany	23%	25.5%
Italy	14%	15%
United Kingdom	12%	14%
Benesync (Belgium, the Netherlands)	6%	6%
Spain	4%	4%
Nordsync (Dk, Fi, No, Swe)	4%	4%
Switzerland	4%	4%

Table 10: ESRF member countries' budget shares, divided on construction and operating budgets. The French contribution to the construction budget included a site premium of 10% as well as the site in Grenoble, ready to build on, free of charge (*ESRF Convention*).

of the Memorandum of Understanding by France, the Federal Republic of Germany, Italy, Spain, and the United Kingdom marked the start of the so-called 'Foundation Phase' of the ESRF in Grenoble (Haensel 1988, p 68). The provisional council, represented by these five countries, as well as machine and science advisory committees, were installed. The detailed design work of the accelerator, instrumentation, buildings and infrastructure started, as did negotiations over the future organization, finances and rules and procedures of the international collaboration. The ESRF team, starting its work in April 1986 with Ruprecht Haensel as director general (Haensel 1988, p 68) was given the task by the provisional council to refine the machine design, prepare the construction drawings, revise the experimental programme, and make cost estimations for construction and operation. The first result of their work was the *Foundation Phase Report* (usually called and henceforth referred to as the *Red Book*), submitted to the provisional council in February 1987 and containing detailed descriptions of everything from buildings and peripheral infrastructure to beamline and instrumentation design specifications, including qualified cost estimations (*ESRF Red Book 1987*). Worth noting for the following is one passage in the chapter about design specifications of the ring and the beamlines, clearly stating the aim of launching a world-leading initiative in synchrotron radiation:

"The basic purpose of ESRF is not to offer what is already available at other Synchrotron Radiation sources but to largely complement and extend the present possibilities. [...] Therefore the two immediate objectives are to make perform experiments which are not feasible with present sources [and] to make an adequate number of state of the art beam lines available to the scientific community" (*ESRF Red Book 1987*, p 39).

The Red Book also offered outlines of prospective beamlines, developed partly through a series of workshops with European scientists. The overall mes-

sage in the Red Book was clear: every performance parameter for the ring, the beamlines, and the individual experimental equipments within control should be optimized in order to make the conditions for *the experiment* as fruitful and promising as possible. Among the things particularly mentioned were the demand for symbiosis between insertion devices and beamlines in the planning and design of experimental equipment, and special emphasis on beam lifetime, stability and reliability for the machine operation. The desire to keep a strong inhouse R&D program in order to keep the facility in the forefront was also clearly expressed:

“[I]n parallel with routine operation, there must be an intense programme of continuous improvement and development. *People and money will be needed* for such a programme.” (ESRF Red Book 1987, p 589, emphasis added).

The estimate in the *Red Book* was that ‘routine operation’ of the machine, with the first fifteen beamlines, could begin at month 66 after the construction start, i.e. after a period of 5 1/2 years.

The political process continued to have its own way. The *Red Book* was adopted by the provisional council in September 1987 as the official planning document of the facility. However, the 1985 Memorandum of Understanding had a two-year time limit, and therefore a new political agreement had to be reached before the end of 1987. The legal documents had not yet been fully finalized, and most importantly, the negotiations over the distribution of budget contributions among the future member states had not been closed (Witte talk 2001). The United Kingdom in particular caused some difficulties in this process. The British Science and Engineering Research Council (SERC) was at the time responsible for Britain’s efforts and memberships in facilities of this kind, and had already the costs of the British neutron spallation source ISIS and synchrotron radiation source at Daresbury within their budget, as well as the British membership in the ILL. Expected to also squeeze in the contribution to the ESRF, the SERC expressed its hesitation towards participation. As one of the big countries in Europe and with a strong domestic program in synchrotron radiation (Witte talk 2001), the United Kingdom was expected to “contribute considerably” to the ESRF (Witte interview). The British delegation argued that under present circumstances, they could only afford to pay 7% of the ESRF construction and operational costs, a figure that after some pressure was raised to 10%. The two research ministers of France and Germany however presented Britain with an ultimatum:

“[T]he research ministers of these two countries, Jacques Valade and Heinz

Riesenhuber, said 'no': if Britain cannot pay a share more commensurate with the strength of its scientific community, it should not be allowed to join at all" (*Witte talk 2001*).

The UK government and its research council finally agreed to participate at 14%, a figure still seen as too low and only reluctantly accepted by the other partners (*Ibid.*). It was generally anticipated that the contracting party countries should join at a percentage at least roughly reflecting the strength of their domestic scientific community in the fields concerned, in order to ensure approximate balance in the 'fair return' of the contributions. Bluntly speaking, the countries were expected to agree to pay a share of the construction and operational costs corresponding in size to the share of the overall delivered experimental time they expected their scientists to make use of. Though this 'fair return' (or 'Juste Retour') policy is not mentioned neither in the 70s ESRF documents (the Perspective and Feasibility Studies) nor in the *Red Book*, it is clear that the 'fair return' policy played a significant role in the negotiations and decision making of the foundation phase. The concept is mentioned in a 1980 report from the Swedish Natural Science Research Council (*NFR Working Group Report 1980*, p 27), together with expressed expectations that it be put into practice for the ESRF.

The United Kingdom's slightly imbalanced contribution percentage to the ESRF budget is not the only anomaly in the context. When the *ESRF Convention* was signed in Paris on December 16, 1988 by all member states (except the Netherlands, which joined later through agreement with Belgium within the 'Benesyne' consortium), the shares were distributed as listed in table 10. These shares correspond surprisingly poorly with both Gross Domestic Products of the countries and rough estimates of their scientific communities' sizes, two general measures according to which figures like these are commonly weighed. Particularly surprising are (along with the low share of the United Kingdom) the considerably high level of contribution from Italy, and the modest figure of the four Nordic countries (*Stirling, Witte interviews*). It has been clearly shown during the almost 15 years of ESRF operation that the Italian contribution is far too high compared to their use of the facility, and that the reverse is true for the Nordic Countries, who constantly over-use beamtime by a factor of approximately 1.5. It is often said that this should have been anticipated or even was anticipated, without further interest, in the budgetary negotiations of the foundation phase (*Birberg, Mason, Rodriguez Castellano, Stirling, Witte interviews*). Regarding available explanations, however, the sources are insufficient and fragmentary. Stories exist of ministers of Italy being lured into a large share by 'sweet talk' from the

French president Mitterand¹⁵³ (*Witte interview*), but it is also reasonable to suggest that the Italian offer from 1980 and on to pay a (very generous) share of 50% for the facility if it be located to Trieste may have put them in a tight position at the negotiating table. For the Nordic countries, it is said that at the end of the negotiation process when only a few percentages lacked, generosity from the bigger countries allowed the Nordic quartet in on only four percent (*Ibid.*). The full effects of these imbalances and the implementation of the 'fair return' concept is a feature of the politics and organization in and around the ESRF significant enough to be dealt with in detail further ahead. It may, however, be of some importance to note that the ESRF Convention, signed in 1988, contains clear statements about fair return policy and how it is supposed to apply, despite the fact that some countries obviously entered the collaboration at unbalanced figures, and that these figures require unanimity in the ESRF council to be in any way changed:

"If it appears to the Council that there is a lasting and significant imbalance between the proportional use made of the facility by the scientific community of a Contracting Party and the contribution of that Party's Members, then the Council may decide measures to limit that use, unless the Contracting Parties agree to an appropriate re-adjustment of the contribution rates" (*ESRF Convention*, p 6).

Establishment and construction

"The objects of the Company shall be, within the framework of the Convention: (a) to design, construct, operate, and develop, for the use of the scientific communities of the Contracting Parties, a synchrotron radiation source and associated instruments; (b) to support the use of the Facility by the scientific communities of the Contracting Parties; (c) to draw up and execute programmes of scientific research using synchrotron radiation; (d) to carry out any necessary research and development work in techniques using synchrotron radiation; (e) to carry out any task associated with the achievement of the foregoing objects" (*ESRF Statutes*, p 3).

153. Cf. the discussions on European politics in chapter 2; Italy's alleged willingness to show its strength in the ESRF budget shares negotiations fits well with the country's standing in Europe by the time, with continued economic crisis and political turmoil making the country a relatively weak partner in European affairs compared to its size and historical importance (Middlemas 1995, pp 80, 316-317).

The signing of the convention meant the end of the so-called 'foundation phase' and the start of construction of the facility in accordance with the details in the *Red Book*. On January 12, 1989, the ESRF was established as a French private company (*société civile*) with the contracting parties as shareholders, and construction work on site in Grenoble began. In order for the scientific case and the specifications of instruments to be updated and ready to put into practice, a workshop and first ESRF users' meeting was held later the same year. During this workshop, "two hundred selected members of the European scientific community" and the newly installed ESRF Scientific Advisory Committee (SAC) worked out a priority list of the first 18 public beamlines to be built (Kvick 1991, p 1310). In 1991 also the Netherlands signed the convention and the statutes and joined the collaboration formally through the 'Benesync' consortium with Belgium (*ESRF Convention*, p 10).

The establishment of the ESRF as a French private company had organizational and diplomatic reasons. The provisional ESRF council did not view establishing an international organization for running the lab an advantageous option, because it would require agreements on a higher diplomatic level than that of science ministries and research councils. An international organization like the one put in place for CERN is a completely new political and diplomatic entity that basically requires construction of a complete set of rules for every possible circumstance or occasion. The dissolving of such an organization – should the collaboration for any reason end – is also a complicated matter. The provisional council wanted to avoid legal ambiguities and risks of controversy over employees' status and tax rules, and so the creation of a private company seemed the most straightforward solution, relying on French law and regulations (*Krech interview*). Avoiding the creation of an intergovernmental organization meant full freedom for the individual participating countries to choose representation in the ESRF: governments could choose an institution or organization of their preference to act as shareholders in the company. This model was largely inspired by the ILL, which was judged comparably successful or unproblematic also with regard to the politics of the international agreements and collaborations (*Ibid.*).

Construction work started on site in April 1990. The scientific and technical case was solid after more than a decade of thorough planning, and with the stable funding provided, rapid advances could be made. The storage ring was commissioned in 1992 and the first preliminary experiments started in 1993 (Lieuvain 1994, p 1555). The aforementioned shift from the wish for a complementary source to a world-leading facility permeate the whole contents of the *Red Book*, which was the document laying the foundation for all work during the construction phase, including not only construction of infrastructure and laboratory facilities but also organization and patterns for budgets and

investment. Some of the measures taken early provided solid ground for the scientific and technical development of the facility for many years to come. For example, it was decided that new investments and refurbishments of instruments should be an annual budget post, just as stated in the *Red Book* (see above). Since the completion of all ESRF-funded beamlines in 1998, which is the point where the formally designated expenditure on construction reached zero, the capital expenditure has been around 20% of the annual budget (*ESRF Highlights 1994-2008*). The *Red Book* also stated that beamlines should be designed and constructed with regard to specific technologies,¹⁵⁴ that they should be state-of-the-art, and that an advanced and viable inhouse scientific program should be included in the budget and promoted on all levels of the organization. These particular strategic choices made in the *Red Book* were implemented during the foundation phase and have since been part of the ESRF organization and strategy. Most importantly, both the heavy focus on state-of-the-art beamlines and the ambition to keep a strong inhouse research program were given special room in the ESRF budget.

The ESRF has its own Participating Research Team (PRT) arrangements, on the so-called CRG (Collaborating Research Group) beamlines. They were first mentioned as a possibility (under the name 'Collaborative Research Teams') in the *Red Book* (*ESRF Red Book 1987*, p 6). The concept reportedly emerged after the decision to build all the 30 public beamlines on the insertion device ports of the ring, when the suggestion was made that the bending magnet ports be used for external groups from the contracting party countries. The details for the CRG arrangements are laid out in a specific document, adopted by the council in 1994. The CRGs are independent contractors, organizationally entirely separate from the ESRF and entitled to use 2/3 of scheduled beamtime for its own purposes. On the other 1/3 of the time, beamlines including all of its instrumentation and adequate user support shall be made available to the general ESRF users (*Patterson interview*). There are no formal regulations for how the CRG make use of their beamtime, although they normally have local review committees (*ESRF CRG Conditions*, p 6; *Patterson interview*). The first few CRG beamlines were approved by the ESRF council in 1991. When the facility opened for users in 1994, three CRG beamlines were already in operation (Kilvington 1998, p 32).

The reports and documents of the foundation and construction phases (*ESRF Red Book 1987*; *ESRF Convention*; *ESRF Statutes*) all expressed a strong focus on external users, and the establishment of a user administration was

154. Beamlines can be designed to optimize for the utility of a specific technique or to cover a certain disciplinary field. While the latter may provide flexibility and broad utilization, technology-specific beamlines are often the most advanced and state-of-the art. This is further discussed in chapter 7.

prioritized early. Largely modeled on the ILL counterpart (just as most of the ESRF organization and administration) as well as the input from the scientists and engineers that were recruited from other labs around the world to run the inhouse scientific program, the ambition of the user administration was to embody a kind of 'best practice' for taking care of the external users:

"We built on the experience that we all had from our different labs. I had one or two excellent people in the beginning who had their beamlines up early and we tested things through them while they were commissioning, so when we formally opened everything was running very smoothly" (*Mason interview*).

When the facility opened for external users in September 1994, the user office was already in place, with established practices on everything from beamtime application procedures over sample handling to travel reimbursements.

The ESRF was a laboratory of unprecedented size and breadth, and the expectations laid on it also lacked comparison. The promises of the official documents and the articulated policy during the foundation and construction phases was that the lab would provide synchrotron radiation in the hard x-ray regime of the highest quality yet seen, that it would run state-of-the-art instrumentation to make optimal use of this radiation, and that it would be primarily focused on accommodation of external users. The early effort to establish a functioning user administration was a crucial part in delivering according to these promises.

Completion and expansion

The Red Book had outlined that the long-term plan to provide over 30 public beamlines to users from the ESRF member countries was to be fulfilled during the first five years of user operation. During the workshops and meetings of the Scientific Advisory Committee of 1989 and 1990, the first eighteen were specified in detail. When the ESRF opened for experiments in September 1994, nine public beamlines were completed, accompanied by three CRG beamlines. In the following three years, six new beamlines were commissioned annually and in 1998 another three, making all thirty of the ESRF's public beamlines operational from January 1999, exactly according to plans (see table 11) (*ESRF Highlights 1994-2008*). In 2004, a beamline previously used for machine and beam diagnostics was made available and turned into

a public beamline. Another beamline for diagnostics was moved in 2007 and made an insertion device port available for a new “instrumentation and techniques test beamline” (*ESRF Highlights 2007*, p 131). The construction phase was completed in 1999 when all 32 ESRF-funded beamlines were ready-built and taken into operation, plus nine CRG beamlines.

As noted in previous chapters, continuous expansion and development is a natural feature of synchrotron radiation laboratories that stems from the diversity in activities and basic ‘substitutional’ character of these labs. The ESRF has since its opening had an annual budget post for capital investment amounting to 20% of the whole budget, which has guaranteed an extraordinary pace of development and refurbishment of accelerator system, beamlines, instrumentation, and other technology in the lab. For the first five years this expansion was mostly represented by the continuous completion of the public beamlines (see table 11), but also after 1999, new beamlines were taken into operation. In the years 2006 and 2007 no new beamlines were commissioned, on the other hand ESRF is on the verge of a major upgrade program that will mean significant developments on the machine side and practically every beamline, and thus new investments of great magnitude (see below).

Another indication of expansion is the growth in number of users, number of delivered beamtime shifts and number of beamtime applications. These figures are provided in some detail in the *ESRF Highlights* report, issued annually. The figures presented there are total numbers of requested and allocated shifts (one shift being eight hours of beamtime) and number of submitted and accepted proposals, all figures split into the categories of scientific areas that also make up the different proposal review committees. While these figures are both official and easily accessible, they are beset with some ambiguities. In the first five years of ESRF operation, the *Highlights* reports covered typically the period from August the previous year to July the present, with the exception of the first report that covered the period September 1994 (when experiments started) to December 1995. The following year’s report covered the period August 1995 to July 1996, causing an overlap in figures. Furthermore, in 1999 the ESRF started to issue the *Highlights* report in the winter, with the effect that the 1999 report covers the period of August 1998 to January 2000 (one and a half year). Thereafter (2000 and forward), the reports cover calendar years. Comparisons over the total period are, therefore, complicated. The categorization and taxonomy of scientific areas and experimental techniques in synchrotron radiation laboratories is generally problematic (see chapter 3 and further chapter 7). The proposal review committees correspond to eleven roughly defined scientific areas (see below), while the beamlines are sorted into seven groups in the organization chart, according to other, primarily

Year	Public beamlines		CRG beamlines	
	Added	Sum	Added	Sum
1994	9	9	3	3
1995	6	15	2	5
1996	6	21	0	5
1997	6	27	0	5
1998	3	30	3	8
1999	2	32	1	9
2000	2*	33	0	9
2001	-1**	32	1	10
2002	0	32	1	11
2003	0	32	2	13
2004	1	33	0	13
2005	1	34	1	14

Table 11: Number of completed ESRF beamlines per year, 1994-2005. (*) One of these is the so called Industry beamline (see below), i.e. it is not among the public beamlines. (**) In 2001, one public beamline was dismantled and replaced by a CRG beamline (*ESRF Highlights 1994-2007*; Kaprolat 2004, p 14).

technical, criteria. The two sets of groups are not translational, mainly because several techniques can be used in the same ‘discipline’ and most beamlines are used by researchers from different fields. Also with regard to the scientific areas present in the lab, there has been a clear growth. When the ESRF opened in 1994, six proposal review committees existed and they had a number of 1,021 proposals (for the period September 1994-December 1995) to process. In 2008, the number of committees was eleven and they processed 2,013 beamtime proposals, i.e. almost a doubling of both the figures (*ESRF Highlights 1994-2008*). The first four issues of *Highlights*, covering in total the period September 1994 to July 1998, also featured number of proposals submitted and accepted sorted in groups corresponding to the proposal review panels, apart from figures on requested and allocated shifts. From these, it is possible to deduce the average number of shifts applied for by an applicant and allocated to a single experiment within the different areas. This data is over ten years old and should therefore be used with some caution; however, they give a hint on differences between the typical amount of beamtime used by a single experiment within different areas. The average number of shifts applied for and allocated to a ‘life sciences’ experiment is roughly 8 and 6, respectively, whereas for example proposals in ‘chemistry’ on average apply for roughly 12.5 shifts and are allocated almost 12 per experiment. In ‘hard condensed matter’, the average proposal request 17.75 shifts and gets about 15.

Over the years, changes have also been done in the proposal and beamtime allocation processes, allowing for special arrangements benefitting both the group of researchers and the ESRF. So-called ‘Long Term Projects (LTPs)’ can be granted continuous beamtime access over three years, if the proposers can

make the case that their project benefits the ESRF and its user community (*Mason interview*). Typically, a LTP agreement also involves an investment in equipment or stationing personnel, such as a post-doc, at a beamline.

For macromolecular crystallography users, whose work differs from most other experimental activities in the laboratory in that the work is of routine character and can be done in just a few hours, another special arrangement has been made available, so-called 'block allocation' of beamtime. Originally, protein crystallography proposals followed the same procedures as every other project, so that one proposal was submitted per project, which in practice often meant one proposal per sample, a very tedious arrangement since most crystallography groups have many samples in line for measurement at once. This led to the submittal of over 200 proposals every scheduling period for very straightforward crystallography measurements and a lot of duplicate work in the proposal review committee for life sciences. The policy of block allocation was implemented as a solution, inviting groups of crystallographers or whole institutes or departments to submit collective proposals. The effect was fewer proposals to handle and greater flexibility for the research groups as they can distribute and administrate their allocated time internally, according to their own preference (*Mason 1 interview*). For crystallographers such level of scheduling freedom is especially advantageous as their process of growing protein samples largely determines their need for beamtime (see chapters 3 and 7).

The total number of *users* and *experiments*¹⁵⁵ is therefore the most useful in order to evaluate the growth in beamtime demand and delivery at ESRF, because these variables are independent of the exact beamtime allocation procedures and these figures are given per calendar year in the *Highlights* reports for the whole period back to 1994. The biggest increases in number of users and number of experiments occurred during the first years, when new beamlines were continuously commissioned, and between 1999 and 2000, when the block allocation policy for macromolecular crystallography was implemented. Another change with effects on the number of users and experiments was the 2000 extension of the scheduling period, i.e. the total amount of time for experiments, with one month (see table 13). In conclusion, there has been a continuous growth in nearly every aspect of user utilization of the facility: From 1995 to 2007, the annual number of users has grown from 1,149 to 6,222, and the number of experiments carried out has increased from 339 to 1,539 (*ESRF Highlights 1994-2008*).

155. Users is defined as number of user visits, i.e. the same one user will appear two (or several) times in these statistics if she has participated in two (or several) experimental runs during a calendar year. Experiments is defined as the number of allocated (and utilized) beamtime slots, i.e. applications granted beamtime and eventually also making use of it (*Mason 2 interview*).

1994	<i>Hard condensed matter, Soft condensed matter, Chemistry, Life sciences, Surfaces and interfaces and Methods and instrumentation</i> (6 groups)
1997	<i>Hard condensed matter split into Hard condensed matter: Structures and Hard condensed matter: Electronic and magnetic properties</i> (7 groups)
2000	<i>New category: Materials engineering & Environmental matters</i> (8 groups)
2002	<i>Life Sciences split into Medicine and Macromolecular crystallography</i> (9 groups)
2006	<i>Materials engineering & Environmental matters split into Applied materials and engineering and Environmental and cultural heritage. Hard condensed matter: Structures split into Hard condensed matter: Crystals and ordered structures and Hard condensed matter: Disordered systems</i> (11 groups)

Table 12: Changes of the proposal review committees at ESRF. (*ESRF Highlights 1994-2008*).

Year	No. of users	Change (%)	No. of experiments	Change (%)
1995	1,149		339	
1996	1,777	+54.66	518	+52.80
1997	2,376	+33.71	656	+26.64
1998	2,726	+14.73	766	+16.77
1999	3,361	+23.29	915	+19.45
2000	5,049	+50.22	1,146	+25.25
2001	4,358	-13.69	1,116	+2.62
2002	4,802	+10.19	1,165	+4.39
2003	5,140	+7.04	1,282	+10.04
2004	5,488	+6.77	1,355	+5.69
2005	5,565	+1.40	1,349	-0.44
2006	6,092	+9.47	1,510	+11.93
2007	6,222	+2.1	1,539	+1.92

Table 13: Applications and number of experiments at ESRF, 1995-2006. 1994 operation was not a full calendar year, and 1994 figures are thus excluded (*ESRF Highlights 1994-2008*).

Year	APS	ESRF	SPring-8
2000	512	1,028	442
2001	676	1,332	712
2002	712	1,336	853
2003	866	1,534	745
2004	1,076	1,689	863
2005	1,190	1,744	870
2006	1,161	1,818	813

Table 14: APS, ESRF and SPring-8 publications, 2000-2006 (*ESRF, APS and SPring-8 publication databases*).

Highest standards

Speculation holds that the success of the ESRF in higher political circles – which made possible the construction and commissioning of the laboratory with no delays and most importantly a comparably generous funding profile – was to a large portion due to the shift in the articulated mission of the facility. In the 1982 *Als-Nielsen Report* (see above), it was first formulated in concrete terms that the facility was to become the world's leading synchrotron radiation laboratory, and have the ambition to remain so. In subsequent documents, this message was repeated. It is reasonable to suggest that this well-articulated ambition was influential for the decision of the French and German governments to make their 1984 commitments to invest millions of Euros annually for at least two decades.

The aims were further specified in the *Red Book*, in which a variety of efforts to ensure that ESRF would take and maintain the lead were outlined and emphasized, efforts by which absolute priority was always to be given to the users and experiments, and by which the continuous world-leading position of the laboratory was to be ensured:

“An important design requirement is to provide maximum flexibility in matching the source parameters to those of the experiment. [...] Most experiments require several hours of stable beam time. For that reason, special emphasis has to be given to features such as: good beam lifetime, stability of the beam during operation, reliability. The facility which is described in the following chapters has been optimized to meet these objectives” (*ESRF Red Book 1987*, p 40).

“The ESRF is a pioneering venture. [...] New ideas and techniques will need to be developed so that users can exploit the machine potential to the full. The ESRF must plan to remain at the forefront of world research, well into the twenty first century” (*Ibid.*, p 589).

“One of the ESRF's major tasks will be to provide a service to users. To maintain this at top quality teams of about three scientists, one student and one technician per beam line, together with all the necessary technical infrastructure, must be provided” (*Ibid.*, p 589).

“In the long run, the main objectives of the ESRF must be to go beyond what can be envisaged today. This objective can only be achieved if active technical, theoretical and experimental research and development, both on machine side and experimental side, is also performed at the ESRF it-

self. On-site research groups should therefore be formed at a very early stage of the project in order to allow a continuous interaction between machine physicists and experimentalists already during the construction of the ring” (Ibid., p 40).

The unanimously expressed¹⁵⁶ aim of the ESRF today – strong focus on user support and the ambition to remain in a leading position internationally – largely echoes the above quotes from the Red Book. The main mission of the laboratory is expressed as follows by the director-general and the head of the users administration:

“Our mission is to supply the most reliable and most stable possible hard-x-ray beams and instrumentation to use that over a wide range of disciplines for the scientific community across Europe. We are a service institution, and there is a corollary to that. The corollary is that this implies that our service mission has to be accompanied by first class science carried out here by first class scientists. [...] So our mission is at the same time to be a first rate scientific laboratory for Europe but to provide first rate scientific facilities for European scientists” (*Stirling interview*).

“To do the best science possible. To help our users to do the best science possible, if you like. For me that is what is written above the door. It’s a very broad statement, but it’s what we do” (*Mason 2 interview*).

Originally the product of multilateral negotiation, and the manifestation of a multitude of national expectations and due investments, the ESRF is continuously evaluated according to a variety of standards, having to do with quality of the research performance, user support, breadth of the scientific program, and progress of the technical development. Therefore the stated mission to do “the best science possible” or perform as a “first rate scientific laboratory” is ambiguous, not only with respect to the classic problem of defining ‘quality’ in science but also in a sociological sense; the ESRF directors and staff have several masters to serve, all with their own demands.

Some but not all ESRF member countries have domestic synchrotron radiation sources.¹⁵⁷ Many of the countries with domestic sources have pursued

156. Almost without exception, the interviewed ESRF staff (see references) expressed the opinion that the main mission of ESRF is to be as good a user facility as possible and that every effort put into the facility is done with the ultimate end of gaining the users and the experimental program.

157. Four of the member countries had domestic sources when the ESRF opened: Germany,

a dual strategy to provide access to synchrotron radiation to their national scientific communities by emphasizing the complementarity of their domestic source to ESRF (cf. MAX-lab, chapter 5). For the countries with no domestic synchrotron radiation laboratory, however, the ESRF is explicitly supposed to cover for their whole national demand (*ESRF LTS 2006*, p 5), and these countries have slightly different requests regarding overall ESRF performance. The ambition to pursue cutting-edge scientific programs in some advanced fields fits well with the 'complementarity' approach, while countries whose entire synchrotron radiation effort lies with the ESRF expects the laboratory to provide facilities and opportunities for a broader base of sciences making use of synchrotron radiation and not only those that require state-of-the-art instrumentation. Therefore, the ambition of world lead – regardless of how that is defined – cannot be allowed to take over entirely; a significant part of the infrastructure must be designed and operated to serve broad scientific communities doing measurements of routine character and experiments with no particular cutting-edge demands. There are indications that this balance is kept fairly well, for example the oversubscription rate figures (see below). The average oversubscription rate is reportedly 2:1, and the fact that the most oversubscribed beamlines approach 5:1 suggest that some beamlines are rather in fact undersubscribed. This is an indication that both the cutting edge experiments and the routine measurements are accommodated, and possibly that the different needs among the member countries are fulfilled. Furthermore, the Collaborating Research Groups (CRG) beamlines play a certain balancing role among the member countries' different approaches, since they are operated by research groups from the member countries under no other conditions than that they provide 1/3 of the beamtime to the ESRFs external users and that their activities meet certain basic quality criteria (see above). CRGs at ESRF may therefore function intermediate solutions for countries with complementary demands for beamtime outside the very competitive ESRF beamtime allocation procedure.¹⁵⁸

The ESRF was built according to a clear policy of beamlines being specific with regard to technologies and experimental techniques. Building and main-

Italy, United Kingdom, and Sweden. Four member countries have since built, are building or have made decisions to build domestic sources: Denmark, France, Spain, and Switzerland. Four member states are without any domestic synchrotron radiation laboratory: Belgium, Finland, Netherlands, and Norway.

158. Although the existing Collaborating Research Groups are foremost from the larger member countries with domestic sources. The current 12 CRG beamlines are run by groups from Belgium/the Netherlands (1 beamline), France (3), Italy (1), Germany (1), Spain (2), Switzerland/Norway (1), and the UK (2) (*ESRF Highlights 2008*, p 139).

taining beamlines with world-unique capabilities is an articulated strategy of the ESRF, and the increasing competition from national sources in recent years has lead to this strategy being pushed even further (*Mason interview*). Every beamline has an individual scientific case and draws its credit from the extensive inhouse research program that is supposed to be maintained at every ESRF beamline. The beamlines, as well as their scientific programs, are reviewed every five years (*ESRF MTSP 2005*, p 5). Also operation of the storage ring, the different 'filling modes' (see chapter 3), is a special feature of the ESRF and provides some beamlines with capabilities inaccessible elsewhere (Elleaume 2004, p 17).

Continuous improvement

The ability to keep high standards with regard to machine and instrumentation is largely due to the annual capital investment budget post, that has enabled a continuous ambitious refurbishment program for all beamlines as well as investments in new infrastructure and instrumentation, on all areas (*ESRF LTS 2006*, p 2). On the machine side, the development has been remarkable given that the upgrades have been done without any long periods of shutdown. Brilliance has been continuously increased so that it is nowadays 2.5 orders of magnitude higher than in 1994 (Elleaume 2004, p 16). Between the years 1995 and 1997, the machine performance underwent a major enhancement when the current of the storage ring was doubled (David et al 1998, p 30). Furthermore, operation reliability has been increased dramatically by the implementation of a systematic effort to analyze all failures and carry out preventive maintenance (*Elleaume interview*).

On the beamlines' side, the annual capital investment budget post has meant continuous improvements and the ability to readjust according to changing demands in the scientific community. This ability has proven to be crucial for the development of the facility, since the demands with respect to beamlines and instruments have shifted considerably since the first 18 beamlines were outlined in the late 80s. The prime example is macromolecular crystallography, which was under-represented when the facility opened in 1994, with no dedicated beamline neither built nor planned but only an experimental station on a shared beamline (McSweeney 2004, p 8). When the increasing demand for macromolecular crystallography was recognized, new beamlines to fill the gap could be constructed with relatively short delay (Kvick 2004, p 4).

Today, three public beamlines with a total of seven individual experimental stations are dedicated to macromolecular crystallography (*ESRF Highlights 2008*, p 139). Specially optimized user accommodation procedures have been built up, with aforementioned ‘block allocation’ of beamtime and even ‘mail order’ measurements (see below).

Investment in new instrumentation has also enabled continuous incremental improvement of beamlines. A modular ‘open undulator beamline’ for testing of new scientific ideas and instrumentation was set up early on (Riekel 2004, p 10). To ensure the keeping of a holistic view on facility performance and upgrades of its capabilities, a decision was made early to set up an optics group and a dedicated test beamline for optics. Through this initiative, the ESRF has been able to lead part of the optics development themselves, and keeping it in close proximity to the other development and enhancement programs (Morawe and Freund 2004, p 7).

The importance of maintaining a strong inhouse scientific program to ensure the ability to provide a good user service and support was anticipated in the *Red Book* (see above). The same connection between inhouse research and development and accommodation of users’ needs is repeated by today’s ESRF officials (*Elleaume, Krech, Kvick, Larsen, Mason, Rodriguez Castellano, Stirling interviews*). In practice, the health of the inhouse program is guaranteed by the policy of having 20% of the inhouse scientists’ time earmarked for their own scientific projects and the development of ESRF science and instrumentation (*Kvick interview*).

To further ensure as smooth technical operation as possible in all the facility’s details, the ESRF has put in place a well-oiled organization with specialized maintenance and service groups for specific areas, both within the accelerator division and the experiments division of the organization. A separate ‘technical beamline support’ group operate across the board as a task force that is called in to solve urgent problems, regardless of location (*Comin interview*).

All these mechanisms and policies that are deliberately implemented to create as favorable conditions as possible for hosting science of high quality are ultimately made possible by solid and predictable funding. The stable funding profile, guaranteed by the 1988 signing of the *ESRF Convention* that was to be in force for no less than 20 years, has allowed for a long-term planning on both the scientific and technical side that most synchrotron radiation laboratories lack, due to national politics. Signing of the *Convention* meant a commitment over 19 years (from the signing in December 1988 to the end of 2007, when the *Convention* expired) to contribute financially to the facility

according to the negotiated level of shares (see table 10), amounting to several million Euros per year also for the smallest shareholders,¹⁵⁹ only possible to change by an unanimous decision in the Council (*ESRF Convention*, p 4).

The ESRF is repeatedly compared with the two other of the 'big three' synchrotron radiation facilities in the world, the Advanced Photon Source (APS) at Argonne National Laboratory in Illinois, and the SPring-8 in Japan, not least with regard to publication statistics. According to the director-general at ESRF, the ESRF outperforms both:

"For every two publications produced by the APS, the ESRF community produces three. We produce between 2 and 3 times as many publications as SPring-8. The budgets are broadly comparable. [...] These publications, our different libraries have looked at them together, so we all agree that they correspond to the same thing. It's not measuring elephants on one side and zebras on the other. So as far as we can tell, that is a fair thing. [...] If you divide it by the number of beamlines, we are the smallest of the three, APS is running at least the same number of beamlines as we are, and SPring-8 is running more. So that would even stretch it more" (*Stirling interview*).

The figures obtainable from the publications databases of the three labs confirms these claims roughly, as seen in table 14. According to directors and scientists at the ESRF, it is possible to assign this clear lead in number of publications to the collected strategies and efforts at the ESRF to enable users to make the best experiments possible at their beamlines, all of which has been and will be discussed further in this chapter (*Elleaume, Glatzel, Larsen, Mason, Stirling interviews*). One organizational difference between the APS and the ESRF commonly referred to, not only by ESRF staff but also by people in the community in general has been mentioned in chapter 4 and concerns the extensive presence of Collaborative Access Teams (CATs) at APS. As mentioned, when ESRF construction started, the facility was funded in its entirety, with 30 complete beamlines including all instrumentation and the hiring of adequate staff, and continuous development and improvement

159. The estimations in Annex 3 to the *Convention* is that the total costs of the facility during the construction phase I and II (covering the years 1988-1994 and 1994-1998 respectively), including operating costs for the years 1994-1998 would be approximately 3.6 billion French Franc (in January 1987 prices excluding taxes) (*ESRF Convention*, annex 3). Recalculated (according to the fixed rate set at January 1, 1999: 1 EUR = 6.55957 FF) only in order to give a rough estimate, this equaled about half a billion Euro, which meant a commitment from the holders of the smallest shares (for example Spain and Switzerland, 4% each) of over 20 million Euros only for the first ten years, i.e. over 2 million Euros per year.

was assured also financially, by the annual capital investment budget post. Some bending magnet ports were provided to external groups to equip and staff through so-called CRGs, but only as a complementary asset, surplus to requirements. The differences to the APS, entirely built on the capacity of the Collaborative Access Teams to equip and staff the beamlines, has allegedly created very different conditions for the collected scientific activities at the facilities. In comparison, the ESRF has had great advantages from the all-encompassing funding model whereas the APS have experienced severe problems (see further chapters 4 and 7) (*Witte interview*). The explanation for the differences compared to SPring-8 are more elusive, but they are partly ascribed to the larger industrial involvement – an estimated 20% of the total activity at SPring-8 is proprietary research (*SPring-8 Research Frontiers 2006*, p 201).

Some of the claimed success of the ESRF is reasonable to relate to the concept of the ‘Matthew Effect in science’ (see chapter 1), that there is a ‘cumulative advantage’ associated with credibility and good reputation in science. Success stories seem to replicate themselves and create a self-sustaining self-reinforcement. It is commonly agreed among users, scientists at other labs, and inhouse scientists, that the ESRF is a world-leading facility, perhaps *the* world leader. An overwhelmingly positive attitude, in some cases admiring, emerge from the collected interview material, coupled with a clear conception of the reasons for the success: extremely good user support, continuous investment in new instrumentation and upgrades, and reliable production of a stable and high-quality radiation beam. It is reasonable to suggest that the Matthew Effect plays out especially well in a global community of strong and personal networks, like synchrotron radiation. A facility like the ESRF, slowly but comfortably gaining a reputation of being able to deliver ‘world-class’ x-rays on ‘world-class’ instrumentation with ‘world-class’ scientific and technical support, as well as maintaining a ‘world-class’ inhouse research program, will arguably see that reputation multiply and reinforce itself. As more and more people seek employment opportunities and apply for beamtime at ESRF, competition will increase and the reputation of excellence will attract the greatest talents and the most prominent experiments, in due time guaranteeing the seizing and maintaining of the global leading role. The ESRF directors have made it their mission to keep the number one position in the world and advertise it. Numerous examples pointing at the strength of the ESRF in machine reliability, user support and inhouse research are brought up in interviews, along with claims of technical superiority and comparably high degree of completion of projects and experiments (*Elleauume, Krech, Larsen, Stirling interviews*). The ESRF is the photon metropolis – bigger, better and bolder than any of its competitors.

The politics of collaboration

The 12 countries that signed the *ESRF Convention* in 1988 and 1991 are members through proxy organizations of their choice, such as Italy who have chosen to let two national research institutes share their membership with the national research council, or Germany whose membership is managed entirely by the German domestic high energy physics and synchrotron radiation laboratory DESY. The countries are called the 'contracting parties' and their choice of proxy organizations does not affect their membership in ESRF but only their internal administration of the membership. Contracting parties and proxy organizations are listed along with their budget shares in table 15. No additional countries have been admitted as full members in the collaboration since the signing of the convention, but countries have been admitted as so-called 'scientific associates', with observer status in the council. The first scientific associate to enter into the collaboration was Portugal in 1988, followed by Israel (1999) and Austria (2002), all three entering with an annual budget contribution of 1% each of the total member countries' contributions. Scientific associates, and the three other partners Czech Republic, Hungary and Slovakia (0.55%, 0.25% and 0.25% respectively) thus add to the total budget without the original member countries' contributions being adjusted. The scientific associates' de facto contributions to the ESRF budget has since 2002 been roughly just below 5%, sale of beamtime around 3%, and other income, such as sale of equipment, around 2-6%. The largest share of the ESRF budget is thus still paid by the members (*ESRF Highlights 1994-2008*). The total sum of members' contributions have increased approximately 2% each year since the construction phase II ended (1999), and with an increasing amount of money coming in from scientific associates and sale of beamtime, the overall budget has increased in numbers every year during the past ten years, but considering inflation, it has approximately remained steady.¹⁶⁰

The council, to which each country appoints up to three delegates, is the highest governing body of the ESRF. The *ESRF Statutes* stipulate that the council shall meet at least twice a year, and that their meetings shall be closed. Stringent rules of procedure govern the work of council. For the ESRF to admit new members or scientific associates, or redistribute shares among the member countries, a unanimous decision is required, in this context defined as at least two-thirds of the capital and no counter-vote of any contracting party with all contracting parties having an opportunity to vote. Qualified majority, here meaning two-thirds of the capital, the number of unfavorable

160. The average inflation rate in the Euro zone since 2000 has been slightly above 2% (*Eurostat Yearbook 2008*, p 137).

votes not exceeding half of the contracting parties, is needed for council's election of its chairman (and vice-chairman), the medium term scientific programme, the annual budget, and the policy for the allocation of beam-time. Each contracting party (the countries, not the member organizations) has a single indivisible vote that the delegations designate to one of their delegates on basis of their internal decision. The Scientific Advisory Committee (SAC) and the Administrative and Finance Committee (AFC) are appointed by the council as reference groups. The role of SAC is that of a traditional science advisory committee; to guide and inform council in scientific matters and prepare decisions in council, and to approve and thereby give its support to medium- and long-term plans for the facility. The role of the AFC is to prepare decisions on administrative issues and to work out the details of the annual budget (*ESRF Statutes*, p 5; *Birberg, Rodriguez Castellano interviews*).

There are examples of matters that are never brought up at all because they are politically sensitive and threaten to cause deadlock situations in the council. The distribution of the shares among the member states – some of which are inconsistent with average utilization of the facility – can only be adjusted by the council's unanimous decision, which has lead to a stalemate. It is argued that the council generally tries to avoid even discussing changes of the contents of the *Convention*, since it is extremely complicated and time-consuming (*Stirling interview*). The differences between the member countries in routines for decision-making and the bureaucracy associated with it have the effect that the contents of the *Convention* and *Statutes* remain completely unchanged. The need for readjustment of the budget shares is reportedly often voiced by council delegations (*Birberg interview*), but qualified speculation holds that council tries to avoid going into such readjustment because of fear that the situation would not be resolved:

“There was always strong reluctance from the French side to reopen the Box of Pandora, because it was such a heavy and long negotiation to come to this set of percentages. It would be easy to say that Italy gives 2% to Nordsync, all the rest remains as they are, and then the main imbalance would have been cured. But everyone feared, or in particular the French feared, that then other countries would, Germany would, or the British would not accept to pay more and so on” (*Witte interview*).

But also on less dramatic policy areas, differences between member countries may complicate the work of council and the directors:

“The problem we have is that each country has different policies. Each country is at a different place in the economic cycle. So that for example

France, 27.5%: *The National Center for Scientific Research (Centre National de la Recherche Scientifique, CNRS), The Atomic Energy Commission (Commissariat à l'Energie Atomique, CEA).*

Germany, 25.5%: *DESY (Deutsches Elektronen-Synchrotron).*

Italy, 15%: *The National Research Council (Consiglio Nazionale delle Ricerche), The National Institute for Nuclear Physics (Istituto Nazionale di Fisica Nucleare, INFN), The National Institute for Material Physics (Istituto Nazionale per la Fisica della Materia, INFN).*

Nordsync, 4%: *Danish Natural Science Research Council (Statens Naturvidenskabelige Forskningsråd), Academy of Finland (Suomen Akatemia), The Research Council of Norway (Norges Forskningsråd), The Swedish Research Council (Vetenskapsrådet).*

Benesync, 6%: *The Belgian Federal Science Policy Office (Le Service Public Fédéral de Programmation Politique Scientifique), The Netherlands Organization for Scientific Research (Nederlandse Organisatie voor Wetenschappelijk Onderzoek).*

Spain, 4%: *Interministerial Commission on Science and Technology (Comisión Interministerial de Ciencia y Tecnología).*

Switzerland, 4%: *State Secretariat for Education and Research (Staatssekretariat für Bildung und Wissenschaft).*

United Kingdom, 14%: *Council for the Central Laboratory of the Research Councils.*

Table 15: ESRF Contracting parties and proxy member organizations (*ESRF Statutes*, pp 1-2).

currently the British seem to have quite a bit of money, whereas the French seem to have no money at all. So is this a good time to propose an upgrade program? Or should we wait until the French are rich and the British are poor or whatever?" (*Stirling interview*).

The differences between countries with regard to their domestic synchrotron radiation capacity (see above) is but one of many examples of the different priorities that different countries may have and may want to enforce in the ESRF collaboration. On the other hand, the member countries are said to have a "mutually benevolent attitude" (*Stirling interview*) in scientific affairs that makes most of the issues possible to solve by consensus before going to a vote (*Witte interview*) or a natural tendency to make this consensus arrive through a 'crystallization' with the major contracting parties at the center of action, just as in the 80s when the site selection procedure was put to an end:

"To my surprise, hard disagreeing discussions dissolved frequently into brief and total agreement the next day, i.e. when in the evening the bargains between the main actors had been arranged behind the scene" (Ulrich Bonze, former member of the ESRF council, quoted in Capellas 2004, p 29).

Laboratory organization

Running a laboratory of the size of ESRF is of course not only science and politics, but also administration and finance. Established as a French private enterprise, the ESRF follows French law and regulations and pays French taxes. For the Grenoble area and France in general, the ESRF, employer of 600 people and with a turnover of nearly 80 million Euro, is of course a “money making machine” (*Krech interview*). The annual ESRF procurement expenditure amounts to about 33 million Euros, of which more than half normally is spent in France. The principle of ‘fair return’ (*‘Juste Retour’*) is implemented for procurement and employment:

“‘Juste Retour’ is fully satisfied when the cumulated value of all contracts awarded to firms in Contracting Party countries for the previous 3 years is distributed pro-rata to the share of contribution of each Contracting Party. In these circumstances the ‘Return Coefficient’ equals 1” (*ESRF Financial Rules*, Annex 4, p 7).

A ‘best value for money’ principle is always overriding, but within that, suppliers from all the member countries are supposed to get a chance to make offers. Service contracts are deducted from the fair return as those contracts naturally will be locally awarded, but for the rest, which is about half, the fair return policy is practiced. The supplier informs the ESRF of where the added value is generated, and this guides how the contract is treated in the calculation of fair return. Every such calculation is then based on the balances between the member countries. ‘Well-balanced countries’ have a share of the total value of the procurement contracts that corresponds to their share in the ESRF and thus their budget contribution (*ESRF Financial Rules*, Annex 4, pp 4-6). From the so-called ‘return coefficients’ (share of procurement contracts divided by budget share, shown in table 16), it is possible to deduce that all countries except for France and in the last period listed (2002-2006) also Spain, are poorly balanced.

Spain’s contribution to the ESRF budget is only 4% and therefore the awarding of a single contract to a Spanish firm changed the Spanish return coefficient. After France deregulated the power supply market in 1999, the contract eventually went to a Spanish firm with the result that Spain now enjoys a return coefficient of nearly 2. The corresponding decline for France can be seen in the same period, as power is no longer delivered by a French company (*Krech interview*). When carried out in practice, the fair return policy forces the ESRF administration to always look for an offer from poorly balanced countries. If an offer from a firm in a poorly balanced state is not more

	1996-2000	1999-2003	2002-2006
France	2.26	2.08	1.82
Germany	0.57	0.64	0.65
Italy	0.24	0.36	0.43
UK	0.72	0.88	0.85
Benesync	0.33	0.26	0.55
Nordsync	0.23	0.31	0.46
Spain	0.51	0.65	1.84
Switzerland	0.70	0.84	0.66

Table 16: Average fair return coefficients of the ESRF contracting parties (*Nordsync Annual Report 2006*, p 14).

than 10% above the offer from the well balanced state, the supplier from the poorly balanced state is given the possibility to align, along with the promise to get the contract if they do (*ESRF Financial Rules*, Annex 4, p 10). The fair return policy has some built-in faults that reveal themselves when it is carried out in practice. As seen in the case of Spain and the electricity contract, small countries that have only 4% or 6% participation can have their return coefficient drastically changed by the awarding of one single contract. For countries whose share is significantly larger, a lot of supply contracts are required to change their return coefficient. Furthermore, that the budget percentages by no means correspond to the relative economic powers of the countries, i.e. their ability to offer supplies (*Krech interview*).

Apart from the fair return policy, which reveals itself also in the allocation of beamtime (see below), the ESRF is in most respects managed and organized as most synchrotron radiation laboratories, with director, committees, and organizational divisions between for example accelerator operation and maintenance and the experimental program, only at a larger scale. Six directors manage the five divisions under the director general, which are the Accelerator and source, Technical Services, Computing Services, Administration, and Experiments divisions. The Experiments division is made up of 7 beamline groups plus the optics and theory groups, with group managers, and every beamline has a beamline responsible and a few scientists and technicians. Most of the day-to-day decision-making is naturally done at the directors' level. The de facto influence or power over the experimental program is divided in a manner similar to the general case. The research directors – heads of the experiments division – have a lot of influence on the medium and long term, as they are the ones who in principle are in control of the annual capital investment. The decisions are always done with the Scientific Advisory Committee as an external consulting and approving entity, but the ultimate choice lies largely with the research directors (*Larsen*

interview). It is argued that the scientific continuity at ESRF is to a larger degree represented by the floor-level scientists and technicians than the research directors, mainly because the directors are appointed on terms limited to five years:

"The bureaucracy superstructure, the directors, they are appointed on shorter terms, normally five years, so they come and go. But then you have a staff of permanently employed researchers, like the group leaders, they are the ones who really run the activity" (*Kvick interview*).

"The directors decide. But all the action and intermediate decisions are taken at the midlevel management. And the opinion is formed there. And there is opinion there that is not made of assumptions from the directors but distilled from the positions of different scientists. Socially speaking I think this is a big advantage of a structure like ESRF. Where you have a director that changes every five years, you would risk that every new director as it happens wipes out all that is done and restart everything. And that would be a mess. So there are some anchor points that are given by intermediate management. And that is the way it works" (*Comin interview*).

Several interviewees have highlighted the importance of informal networks to complement or partly replace the formal organization chart. The suggestion is that a very rigid or well-structured organization like the ESRF inevitably will give rise to very well developed informal organizations and modes of communication:

"If things only happen through the formal lines of the organization chart, then for example if a project is to be done by several groups together, in principal all the organizational parts along the formal lines of the organization chart has to be involved, and at the beginning of the ESRF it was just like that. [...] But slowly people started to communicate and slowly they started to build the informal network. And an informal network works. [...] I have of course been here a long time and so I know a lot of people and therefore I know which button I have to trigger in order to make something happen directly instead of asking my supervisor to go through the higher level communication channels" (*Comin interview*).

"Yes, the ESRF has been around now for 20 years, and many people know each other since a long time. [...] And people know each other and there are preferred communication channels. [...] The ESRF relies a lot on those informal communication channels" (*Glatzel interview*).

Beamtime allocation process

Just like most synchrotron radiation facilities, the ESRF has the basic policy that beamtime is free and awarded through organized peer review. Proposals are graded solely according to judged scientific quality, and the only obligation put on the scientist is that results are openly published. At least this is how it is supposed to work, and this is also the image that the ESRF management is keen on presenting, both in printed material and in interviews:

“Beamtime will be allocated by the ESRF Management according to scientific excellence. Priorities will be decided by peer review committees composed of highly qualified scientists, mainly from the community of contracting party countries. Fair beam time allocation must be judged a posteriori by the scientific communities, through the ESRF Science Advisory Committee (SAC)” (*ESRF beamtime allocation policy*, p 1).

“[E]ach proposal should be evaluated for its scientific excellence by a review committee whose members are specialists in the specific areas of science to which the proposal belongs.” (*Ibid.*, p 3).

There are however complicating features. The scientific fair return that is put in place to guard the member countries’ expected return for investment skews the ordinary and transparent procedure of providing beamtime to users, and will be discussed below.

The document regulating beamtime allocation states that 80% of the total amount of available beamtime is supposed to be allocated through the peer review process, 15% shall be earmarked for maintenance, upgrading, development and in-house research, and 5% is for management to distribute to, for example, peer-reviewed excellent projects by groups from non-contracting party countries. The review committees are appointed by the ESRF management with the consent of the Scientific Advisory Committee (SAC), which is also represented by one member in each of the nowadays 11 committees.¹⁶¹ Each committee member is appointed for a period of two years with the possibility of one year renewal, guaranteeing some turnaround (*ESRF beamtime allocation policy*, p 4). The review committees meet twice a year and have

161. In 2008 they were chemistry; hard condensed matter: electronic and magnetic properties; hard condensed matter: crystals and ordered systems; hard condensed matter: disordered systems and liquids; applied materials and engineering; environmental and cultural heritage matters; macromolecular crystallography; medicine; methods and instrumentation; soft condensed matter; and surfaces and interfaces (cf. table 12 on p 228) (*ESRF Highlights 2008*, p 140).

in total between 1,500 and 2,000 proposals to handle. They work without payment, but since their decision is supposed to be taken unanimously they have to convene. The proposals are sorted in advance by the head of the User Office, in categories corresponding to the committees, and sent to the committee members. Contrary to what is the case at many labs, the peer review process is not anonymous. The work of the committees is to grade the proposals, and the granting of beamtime is done by the directors (in practice by the users office) on the basis of the grading, so the review committees do not decide on the cut-off level for granted beamtime. Three grading groups exist:

“Grade A – proposals which must be accepted because of scientific merit or technical (i.e. instrument development) justification. Grade B – proposals which should be accepted if there is sufficient beam time. Grade C – proposals which should, on no account, be accepted since they have not sufficient scientific merit nor technical justification” (*ESRF beamtime allocation policy*, p 6).

‘Technical justification’ of experiments means that the applicants are required to motivate why they need to do their experiment at the particular beamline at ESRF, i.e. show that it cannot be done at another lab. In practice this is a policy that goes back to the original statements in the *Red Book*, that the ESRF shall provide opportunities that does not exist at national laboratories, both from a quantitative and qualitative point of view. If a proposal cannot be shown to specifically require the technical standards of the ESRF, it is not supposed to be awarded beamtime (*Glatzel, Mason 1 interviews*).

Within the grading groups A, B, C, a score between 0 and 5 is awarded, with three significant numbers(!). The general quality of the proposals perceived as high and increasing; out of more than 1,800 proposals in the fall of 2006, only four were graded C, and more than half were graded A (*Mason 2 interview*). In effect this means that – before the implementation of the fair return policy, shall be noted – only rarely do proposals graded B get beamtime, even though their scientific quality may not at all be mediocre:

“There are some very good projects that we are simply not taking onboard because there is simply not enough time available. Time and again I see this coming up in the sheets, it’s one of my standard comments, ‘excellent proposal, very well received, but fell just below.’” (*Mason 1 interview*).

There are indications of a continuous improvement of the already high quality standard stemming from the procedures of the beamtime allocation pro-

cess. Applicants behind the ‘C’-graded proposals have allegedly learned that there is no point submitting again, as have some applicants behind the ‘B’-graded proposals farthest from the cutoff point. The applicants with the best proposals do also learn to write better proposals as they become experienced and learn the details of ESRF instruments (*Mason 2 interview*).

Beamtime on CRG beamlines that is not distributed through the ordinary ESRF procedure takes is allocated approximately one month after the regular beamtime allocation process. In principle the CRGs have full autonomy regarding what process they choose for their own beamtime, but they are informed about the outcome of the ordinary proposal review process so that beamtime is not allocated twice to the same proposal. Though there is no formal rule against that happening, the CRGs are informed so that they can make a fair judgment (*Mason interview*).

The scheduling of the beamtime is done on basis of the general operations calendar (including details on different filling modes) decided by the research directors, in detail showing how many shifts are available at each beamline. This schedule is done on basis of what every beamline responsible has reported in. As a general principle, a small margin is always included so that in the end, more shifts are always delivered than was once scheduled for the period. The margin may be used for projects that fall just below the cutoff line in the allocation process and therefore are put on a reserve list, together with compensation in the event of beam loss and other unforeseen events. The policy of the ESRF is to always facilitate the completion of projects, which can mean a stretching of the otherwise very strict rules of beamtime allocation and usage.

The scientific fair return

The outcome of the process described in the previous section is partly altered every scheduling period by the implementation of the scientific fair return policy. The *Convention* states that the shares of beamtime awarded among the contracting parties should be made to correspond roughly with their shares of the ESRF budget, and that “lasting and significant imbalance” between use and contribution of a particular member state can cause the council to take “measures to limit that use” (*ESRF Convention*, p 6). The document regulating beamtime allocation policy is more explicit:

“The aim of the ESRF is clearly to have those of the proposed experiments

done which, by peer review, are deemed to be of the highest excellence. It is possible that this aim is incompatible with juste retour. However, it should be obviated that, integrated over several 6-month cycles and over all the beamlines, a contracting party receives less than 75% of its juste retour” (*ESRF beamtime allocation policy*, p 7).

The contracting parties are of the opinion that the fair return policy should be implemented as far as possible (*Stirling interview*), and measures have been taken to adjust for the chronic over-use of some countries. If a country receives more than 25% over their contribution, an additional contribution must be paid, and this has been the case for Nordsync for several years. In 2005, groups from the Nordsync countries used 5,85% of the total delivered beam time, causing an additional budget contribution from Nordsync of 398.3 kEuro for that year (*Nordsync Annual Report 2006*, p 3). No rules have been enforced regarding under-use, and so a lasting imbalance prevails for countries like Germany and Italy. The most far-reaching measure taken by the council under the auspice of the paragraph in the convention allowing correction of “lasting and significant” imbalances was when in 2005, after long discussion, the council came to a compromise decision with the Scientific Advisory Committee that a computer program for reallocation of beamtime among the member countries should be applied on the 20% of the granted beamtime that went to the lowest graded proposals on each beamline. In effect this means that 80% of the top projects would go through anyway and for them, scientific excellence would be the sole basis for beamtime granting (*Mason 2, Stirling interviews*). The computer program was taken into operation in 2006, and is reportedly working well:

“The countries with major underusage of beam time, Germany and Italy, have received increased beam time allocations according to these procedures. ESRF considers that this has been implemented without significant loss of scientific quality” (*Nordsync Annual Report 2006*, p 3).

The level of detail on which this system of scientific fair return is implemented is reportedly unique for the ESRF:

“I know of no other scientific institution apart from the ESRF, and to a lesser extent the ILL, that has to return numbers of scientific use at the level of four significant figures. What they look at is not 6% but 6.13% or 5.98%. And we make adjustments and we make very complicated tables which take a lot of time. [...] Science doesn’t work in that way! It’s a classic

bean counting, but I have never seen one that is so precise. Trying to say 'you pay 100 Euros and you get 100 Euros worth of science', that statement doesn't make any sense really" (*Stirling interview*).

As mentioned, the grading of the projects done by the proposal review committees is very detailed, and the normal situation before the adjustments are made is that only very few 'B'-graded proposals are awarded beamtime (*Mason 2 interview*). This means in turn that all 'A'-graded proposals normally are accepted, or in other words, that proposals must in principle be graded 'A' to be accepted. With the fair return policy in operation through the computer program, however, a few 'B'-graded proposals every round will be accepted at the expense of some better ranked, 'A'-graded, project proposals.

As mentioned above, some delegations to the council are calling for renegotiation of the budget shares. A common answer by the other delegations, who are unwilling to even engage in discussion over such readjustments, is that countries with scientific fair return ratio below one simply has to make their scientists use the ESRF more (*Birberg interview*). The approach on part of Nordsync is clear on this point; since an additional payment is made every year to compensate for over-use, the Nordsync countries argue that they already pay what they should pay and no readjustment is needed for their sake (*Nordsync Annual Reports 2005*, p 4, *2006*, p 3-4). Their additional payment is furthermore done on top of the ordinary budget, meaning that the ESRF gets an extra income every year.

Special procedures exist for projects involving proprietary research. If the investigators do not want to disclose the content of their experiment, if they consider it to be of minor scientific importance and thus has no or very little chance of having beam time allocated, or if they want to obtain results more rapidly than is possible through the normal system, beam time can be bought (*ESRF Industry Guidelines*). The first sale of beamtime was done in 1995, with 42 shifts in total, a figure that has since then continuously grown, to 541 shifts in 2004 (*Stirling 2005*, p 3). The general picture is that the ESRF is comparably popular among companies:

"Industry is more interested in things that work, where you can see the results. You know that our beamlines, the macromolecular crystallography beamlines are among the best in the world. They know that. They have access here, so they know how this works, and so they are happy and they continue to come" (*Rodriguez Castellano interview*).

For macromolecular crystallography, the popularity is probably to a great part due to the 'full service' solution called *MXPress* that ESRF has offered

to industrial users for some years, and that allows industrial users to have their samples measured and data taken on mail order. The 'customer' spares the travel and need not adapt to ESRF scheduling, and gets the measurement and analysis done by experienced ESRF staff, and the results are sent back through a secure system. This service differs from the remote access crystallography service provided at, for example, Stanford (see chapter 4) because it is a full service and not merely an interface for remote control. The beamtime and work is billed to the customer, which makes the service entirely intended for commercial use (Rodríguez Castellano 2005, p 5).

On the laboratory floor

Competition has become sharper at the ESRF over the years, not only in the proposal procedure but also between and among groups of users as well as in-house scientists. As noted above, there is a detectable 'Matthew Effect' in the development of ESRF and its relations to the European synchrotron radiation user community: The laboratory's reputation of being technical frontrunner in many areas, providing excellent user support, and also pursuing a comparably thriving inhouse research program seems to be continuously improving by self-reinforcement. Oversubscription rates approaching 5:1 at particular beamlines is an extraordinary feature, however, as table 17 shows, the overall oversubscription rate is between 1 and 2, which would mean that some beamlines in fact are undersubscribed.

Outside of statistics, a general picture is painted by ESRF officials and beamline scientists of a laboratory environment with a very competitive atmosphere. This is allegedly due to the reputation of the facility and the knowledge that competition for beamtime – especially on some beamlines – is very high, and that a less successful experimental run is likely to harm one's chances of getting beamtime granted on the next application (*Larsen, Mason 1 interviews*). But also purely physical features make the general ESRF lab environment 'unopened' and competitive.

All beamlines at ESRF are contained in so-called 'hutches', bearing their names from the early days of synchrotron radiation when experimental set-ups for protective reasons were placed in small cabins, too small for humans and with locking systems that allowed for radiation to arrive at the experimental station only when the hutch door was properly closed. The name has lingered on although the 'hutches' of today are much larger – some experiments still need radiation protection around them but other security systems have been developed that allows for more space around the experimental

Year	Beamtime applications			Experimental shifts		
	Subm.	Granted	Rate 1	Req.	Alloc.	Rate 2
2000	1,470	1,146	1.28	23,216	12,309	1.89
2001	1,582	1,116	1.42	24,824	11,281	2.20
2002	1,489	1,165	1.28	24,585	11,759	2.09
2003	1,622	1,282	1.27	25,740	12,486	2.06
2004	1,675	1,355	1.24	26,251	12,425	2.11
2005	1,881	1,349	1.39	30,311	12,894	2.35
2006	1,892	1,510	1.25	29,923	13,082	2.29
2007	1,907	1,539	1.23	29,959	13,681	2.19
2008	2,013			31,105	13,906	2.23

Table 17: Average oversubscription rates at ESRF, 2000-2008 (*ESRF Highlights 2000-2008*).

equipment (*Winick 1 interview*). At ESRF however, the beamlines with all their instrumentation, lab facilities for sample preparation, and office space are contained in ‘hutches’ that are more of barrack size and type. This has the implication for the general lab environment that the ‘experimental hall’ as it is called is seemingly deserted – only occasionally are people spotted while making their way from their beamline to the coffee room or the offices on the second floor. Hardly any activity is noticeable unless entering a hutch, and even there, the office space with its computers is normally what a visitor will first encounter. The experimental station, where arguably the scientific work is done, is hidden in the inner rooms.

This closed environment is said to reinforce the competitive and impersonal atmosphere in the experimental hall. Since all experimental work takes place behind close doors, the already dominating attitude among users to focus exclusively on the experiment and let nothing else come in sight (*Glatzel, Mason 2, Poloni, Ursby interviews*, see further chapter 7) is reinforced and the larger laboratory context is turned into a ‘black-box’:

“This is the laboratory, light comes in, you don’t care why or how, light comes in and this is the lab. The ESRF is not the lab. And the data is on the screen, and so you’re happy” (*Mason 2 interview*).

This alienation of the user is partly countered by the work of the inhouse staff, the beamline scientists, who are the main link between the users and the laboratory but who also frequently involve themselves in research projects together with users.

The inhouse scientists also work under severe competitiveness but do of course have a more regular work pattern than the visiting user. Inhouse

scientists are beamline responsables, beamline scientists, or beamline group leaders, and for the first two categories, their time is divided on 75% beamline and user support and 25% for their own research (*Eeckhout interview*). Their responsibility for the beamline includes maintenance, upgrading and development of the instrumentation, user administration and support (including detailed beamtime scheduling) and a general responsibility to provide a properly functioning beamline to the overall experimental programme to use (*ESRF beamtime allocation policy*, p 3). The general picture is that inhouse scientists have a very heavy workload. Since there are a few scientists working on every beamline, they are assigned by the beamline responsible to act as local contacts for the user groups, meaning that user support for the particular group is their primary responsibility during the beamtime of that group (*Glatzel, Poloni interviews*). Beamline scientists do have a certain amount of guaranteed beamtime, often one week per scheduling period, but it is only allocated to them should their own proposals not go through the review process (*Eeckhout interview*). Much of the inhouse research program is therefore done on beamtime awarded through the general allocation procedure, as beamline scientists are expected to apply and their guaranteed beamtime normally is insufficient for maintaining a viable research program:

“If you don’t apply for time that is the same thing as saying that you aren’t interested in doing experiments at the beamline. [...] There is not much time for in-house research, so if everybody wants to do a lot of experiments on the in-house time, that is not possible. If you are here it is because you want to do experiments, and in general one experiment per round is not enough so you write proposals” (*Poloni interview*)

Over time the inhouse scientists become good at writing proposals, knowing the technical and scientific capabilities of their beamline in detail and working close to external users for the most of their time (*Mason 2 interview*).

The frequent collaborations between inhouse staff and external user groups also contribute to the blurring of the line between the inhouse research program and the work of external users. In most cases (except for the most automated beamlines, like crystallography) beamline scientists work closely enough with the user groups for them to be regarded collaborators in the project and co-authors of publications (*Glatzel interview*). This collaboration pattern is a concrete example of the much discussed connection between good user support and the maintaining of a vivid inhouse research program (cf. SSRL in chapter 4). The ability of beamline scientists to provide good user support is strengthened by the close collaboration with user groups who often are at the forefront in their respective specialization:

“On average I do one ‘local contact’ per month and I find it very interesting. Users come here from other countries because they have a problem that they can’t solve at home. The experiments are at the forefront of research, so I really learn a lot from our users” (ESRF beamline scientist quoted in Capellas 2005, p 16).

The compartmentalized physical environment at ESRF contributes to the isolation of individual beamlines and the reinforcement of sharp boundaries between different areas, while the frequent and intensive collaborations between inhouse scientists and external users creates a fluidity when it comes to the laboratory’s boundaries to its constituent user community.

The upgrade program

When ESRF opened in 1994, it was the leading synchrotron radiation laboratory in the world according to most contemporary measures: it delivered the x-rays of highest brilliance, it had invested in the most advanced optics and detector technology to make the best use of these x-rays, and its organization was built up to serve a demanding user community and simultaneously maintain a strong inhouse scientific program. Fulfilling the ambition to stay in the forefront was not cheap; each year around 20% (or between 15 and 20 million Euros) of the ESRF expenditures has been invested in new instrumentation and upgrades. The technical and scientific development in synchrotron radiation experienced in the 90s and the 2000s so far has however enabled a whole new global competition in the field. Not only has storage ring technology enabled so-called intermediate sources to produce high-quality hard x-rays, the growth of the user community and the developments of optics and detectors has made several technologies off-the-shelf or at least significantly cheaper or more available than they were by the time of the ESRF opening. Thus several new domestic sources are opening, under construction or in planning phase in the world and especially in Europe, and the previously unchallenged position of ESRF has started to become seriously challenged (Freund 2007, p 22).

Out of the *ESRF Long-Term Strategy* document, ordered from the directorate by the ESRF Council and submitted in 2006, grew the refined ideas of a major upgrade program for the facility. This program was outlined in the *Long-Term Strategy* as giving an enhanced performance of the beamlines “by several orders of magnitude” and simultaneously enabling the move into new scientific areas (*ESRF LTS 2006*, p 2). In the fall of 2007, the finished Upgrade

Programme report, the *Purple Book*, was submitted to council, along with a request of an investment of approximately 287 million Euro added on top of the regular budget (*ESRF Purple Book 2007*, p 5), and describing its major parts. Among these are eighteen new and upgraded beamlines, with focus especially on “nanoscience” applications; a “more than doubling” of the intensity of the radiation from the source; construction of new buildings adjacent to the existing ones to host new instrumentation; and new collaborative efforts with academia, other synchrotron radiation laboratories, and industry (*ESRF Purple Book 2007*, p iii).

In the *Long Term Strategy* document, it is argued that the upgrade – building on already very advanced and well-performing infrastructure – will be a bargain in comparison with what the construction of a similar facility on green field would cost. The approximation in the document holds that an all new equivalent facility and laboratory would cost around 1 billion Euros to construct, compared to the upgrade program’s 287 million (*ESRF LTS 2006*, pp 2-3). The argument is probably emphasized partly to underline that the ESRF upgrade is very competitive and affordable also despite the new challenging European synchrotron radiation landscape with its many new third generation sources directly competing with the ESRF in performance and abilities. The upgrade program was given go-ahead by the ESRF council in November 2008 and construction will start in 2009 (*ESRF Highlights 2008*, p 2). The ESRF has once again set the aims high and gotten its member countries on board.

The original *ESRF Convention* is kept in force also after December 31, 2007, and there are no signs of member countries taking any active measures to adjust the inequalities in the budget shares that cause politics to infiltrate science on a very fundamental level; the peer review beamtime allocation process. But through the upgrade program the flip coin of such extensive political importance has shown itself once again: the strength of the European scientific collaboration in terms of funding. The member countries are not willing to see the facility they have invested so much money and prestige in to fall behind in the international competition.

Conclusions

In its entire history – from the foundation and establishment 20 years ago, through the continuous refinement and development of capabilities, and just recently by the now decided upon upgrade program – the ESRF has been a demonstration of power on behalf of Europe in the arena of science and tech-

nology. The facility stands out because it came into being and has been kept at high standards on basis of high-level political decisions and negotiation, and because of the comparably rigid and bureaucratic organizational structure of the laboratory that also has followed.

As the main conclusive observation of this case study, the identification of politics' influence on the shaping of a powerful but bureaucratized laboratory corresponds to the emphasis on politics and formal organization in the chapter. Especially the intergovernmental relations that ultimately govern the laboratory, and the peculiarities that follow and that show themselves in several instances down to the details of beamtime allocation, is of chief interest. It is well in line with the aim of choosing the case in question, because it provides insight to an important feature of large-scale laboratories in the European context, namely that they necessarily are subject to cooperation between different countries with different policies and agendas. In this particular case, because the ESRF is a synchrotron radiation laboratory and thus the agglomeration of a dynamic and changeable set of scientific activities, it also shows at another level the built-in dilemma of these labs, namely that they are hard or even impossible to fully comprehend and overlook and thus present its governing agencies with a steering problem. This will be returned to shortly.

At a slightly more detailed level, some evident political features of the establishment and running of the ESRF deserve attention. The agreement reached between France and Germany has been identified as the single most important factor for the existence of the ESRF; another outcome of the negotiations between these countries would probably have ended the bid for a collective European effort in synchrotron radiation at least for some time to come. The settlement of the location for the facility and the simultaneous agreement over the main share of the project's funding through this 'crystallization' had several dimensions but was basically enabled by the general collaborative spirit of France and Germany that made up the core of virtually all European collaboration at the time. It is therefore an illustrative example of the force of politics in matters of large-scale scientific infrastructure in general, and of the political ramifications of scientific collaboration in particular: National interests, prestige and sovereignty tend to have just as much – if not more – influence on decisions than purely scientific agendas.

As the title of the chapter suggests, ESRF governance is significantly more bureaucratic than other labs, though this bureaucratization has also fostered reliance on informal networks and communication channels on mid and low levels in the organization. It is a valid question whether the political governance of ESRF and the bureaucracy that seems to have followed from it has

prevented or undone the existence and work of scientific entrepreneurs. The truth is probably that their activities are more hidden than in other laboratories. Scientific entrepreneurs in the shape of resourceful and ambitious beamline scientists certainly craft their alliances and promote their projects. As described in a previous section their collaborations with external user groups may very well blur or even erase the lines between inhouse research beamtime and the beamtime of external users. The 'floor level' initiatives and the work of individual scientific entrepreneurs is sharply separated from the political mechanisms and means of political interest that has gotten a lot of attention in this chapter, and it is to a large degree overshadowed by the bureaucracy described.

There is, however, another point to make out of this. Since the governance of individual beamlines and their experimental programs seems to be largely in the hands of the inhouse staff and the users, who define the activities and pursue them, there is active laboratory governance taking place external to or apart from the political top-down ruling. Thus there seems to be a gap between on one hand a central laboratory structure and bureaucracy, largely defined by the political logic that once put the laboratory in place and that carries traces of European nationalist pettiness, and on the other hand a diversified experimental program, defined partly by the organized peer review process and partly by the choices of individual scientists. This tension or divide is yet another version or incarnation of the governance duality of top-down, institutionalized politics and bottom-up, pragmatic science.

The discrepancy between these political and the scientific governance 'realms' – the two that have been identified as analytically important in the previous two cases – emerges large and clear also in the ESRF case. Though the political procedures that once made possible the creation of the laboratory still penetrate its top-level governance, the 'floor-level' scientific activity at ESRF is very distanced from it. A comprehensive funding profile, allowing for continuous reinvestment and refurbishing, makes possible a sustaining of the high standards. These strong finances are the result of the political procedure and the prestige associated with it, largely because no other entity than an intergovernmental collaboration based on meticulous negotiation would be able to guarantee such a funding profile on such a long term. But the scientific program is nonetheless dissociated from the politics in a very fundamental aspect: The ability of the governments to exert authority and control over a scientific laboratory complex as multifaceted and dynamic as the ESRF is limited. A particularly telling example is the scientific fair return policy. Result- and return-oriented in their investments in science, the investing governments cannot fully comprehend the principle that beamtime

is free and awarded solely on scientific merit, and they therefore try to invent systems according to which it can measure and distribute its expected returns for investment.

Finally, the historical position of the ESRF in the emergence, establishment and institutionalization process from 'esoteric endeavor' to 'mainstream activity' needs to be acknowledged. The ESRF was the first of the large third generation synchrotron radiation sources (the 'big three'), and as such it is both the first large-scale and broad range embodiment of synchrotron radiation 'institutionalized' and the first trial of such an institutionalization. It can therefore be identified as a kind of 'litmus test' for the established and institutionalized synchrotron radiation laboratory concept. On basis of such an observation, it may very well be argued that the establishment of ESRF and its alleged success on a multitude of levels was among the most significant breakthroughs of the establishment of synchrotron radiation in the sciences and in the science policy systems.

Politics and practices

Viewpoints

The descriptions and analysis of the three cases in the three preceding chapters have been made in an inclusive manner, on basis of their specific characteristics rather than in stringent adherence to the thesis' analytical ambitions. The final sections of each chapter have thus been used to draw selective conclusions on basis of the case studies' contributions to the general aim of the thesis, identifying particular points of interest that leads the analysis further.

The selective conclusions may appear asymmetrical or disqualifying of certain aspects or contents, but this apparent imbalance is consistent with the aims of the thesis. The study objects need close and detailed examination to be comprehensible, but for the final analysis choices must be made regarding what findings in the specific cases can and shall be used for the overall analysis and the answering of the research questions. The selective conclusions at the end of each case study chapter guide in the transition from inclusive and specialized to selective and general. The aim of the present chapter is to finalize it.

Informed by the theoretical framework developed in chapter 1, three analytical themes have been developed further throughout the preceding chapters and put to use when drawing conclusions. Between, across and on basis of these themes, another finding has been synthesized, namely the apparent existence of two fundamental 'forces' of governance in synchrotron radiation laboratories: The dynamic and evolutionary efforts of the scientific community to develop and promote their activities bottom-up, and the comparably static and institutionalized political control of laboratories, ultimately in control of its funding and governing science top-down.

Synchrotron radiation laboratories emerged and developed into their present shape in parallel with the general collectivization and sophistication trends in science (Ziman 1994, 2000). Many observations in the preceding chapters point at a correlation – science in the 'dynamic steady state' encompasses collaborative teams and projects, sophisticated instrumentation, and increased competition. Synchrotron radiation laboratories are technologically sophisticated and multidisciplinary, they are built to host sophisticated instrumentation, and as such they are subject to tight competition between

scientists. They are the results of strategic choices done collectively by the scientific community and by science policymakers. Access to the laboratories is provided to groups (collaborations) for time-limited work on projects, and the more scarcely available the specific experimental setup is, the more scientists are forced to collaborate in projects to gain access.

The *institutionalization* of synchrotron radiation laboratories in the sciences and in the science policy systems has not only been parallel to the changing dynamics of science but part in collectivization and sophistication and the rise of *project-oriented team* research. By its own institutionalization and its partaking in broader developments, synchrotron radiation has come to play an increasingly important role in science.

Synchrotron radiation laboratories have a kind of big science 'heritage' because they evolved out of high energy physics and took over high energy physics machines at an early stage of their development. Because the science done in synchrotron radiation laboratories is not 'big science' but in fact very 'traditional' in size and structure and for the most part carried out by scientists from 'ordinary' research institutions, synchrotron radiation laboratories host 'small science' while at the same time being 'big science' with respect to size and cost.

This basic observation is well in line with the aforementioned dual nature of laboratory governance, that synchrotron radiation laboratories are positioned in the nexus of the strong forces of bottom-up science and top-down politics. The ESRF case (chapter 6) shows that science institutionalized by the work of big politics may maintain a dynamic scientific program in the hands of floor-level scientists. The Stanford case (chapter 4) shows that an evolutionary growing scientific enterprise can make use of the structural arrangements of yesterday's single-disciplinary and – according to critics – 'pathological' science. MAX-lab (chapter 5) shows a similar pragmatism on behalf of the scientists and the scientific entrepreneurs, but its merit is also to point at the apparent ability of science collectivized and sophisticated to make its way through a system with less developed mechanisms for large scale strategic commitments.

The logic of political top-down governance of the laboratories and the logic of bottom-up scientific initiative are distinctly different and they are both enacted in a number of ways in the laboratory environment. In the course of the present chapter, a few more topics of interest synthesized from the empirical material will be elevated to further illustrate the dual governing of the laboratories and the unification and disunification dichotomy it seemingly corresponds to. The aim is to make the analysis complete by adding these reflections to the conclusions drawn in the case study chapters, and arrive at

final conclusions about the institutionalization of synchrotron radiation in science and in science policy and the closing of the argument.

The machine and experiments divide

The basic organizational structure of a synchrotron radiation laboratory corresponds to its basic technical structure: the accelerator, the beamlines and experimental stations, different peripheral technologies, and the experimental program (including both inhouse scientists and external users) have corresponding organizational entities.¹⁶² Though parts of the same overarching laboratory mission, these serve very different purposes, and their internal organization and culture is often said to be very different depending on their role in the overall laboratory organization. As an internal laboratory organizational feature, the divide between machine and experiments is a tangible material incarnation of the 'small science' and 'big laboratory' duality. It gives a concrete example of the division of labor and task heterogeneity associated with complex technical systems (Hagstrom 1964; Shrum et al 2007; Duncker 1998; see chapter 1), and invokes the conceptualization of technically sophisticated sciences as driven by the constant trades between (material) subcultures (e.g. Galison 1997; Mulkay and Williams 1971).

The accelerator and experiment divisions broadly and loosely defined are the two branches of the laboratory organization responsible for running and maintaining the machine, and for running the experimental program. Accelerator operations groups usually pursue a scientific program of its own, in accelerator physics, closely tied to the accelerator, which is a complex technical system based on thorough scientific and technical work.¹⁶³ Despite this, the overriding mission of an accelerator group is always to deliver beam to the experimental stations without interruption during the scheduled periods of normal operation. In comparison, the fulfillment of that mission requires

162. There are obviously variations, not least with regard to the labels on different organizational entities. The contents of this and coming sections focuses on the common features of most synchrotron radiation user facilities and is the result mainly of detailed observation complemented with interviews and spontaneous conversations with users and personnel at the laboratories. The findings are generalized and there are exceptions to be found probably in every instance, but a convincing general pattern doubtless exists.

163. All accelerators are uniquely designed and constructed; though most storage rings perform similar tasks they are always optimized with regard to performance of the entire system (*Elleau, Eriksson 1, 2, Lindau 1, Nyholm 1, 2 interviews*). Publications in the field of accelerator physics are usually technical descriptions of design and performance of accelerator components.

an “industrial” mode of work (*Elleaume interview*), and the cultural divide between machine and experiments division may indeed be huge:

“The machine stays a monoblock of people where everybody has to do what the boss is saying. Of course that is not how it is in the experiments division” (*Comin interview*, confirmed by *Elleaume, Larsen interviews*).

The quote concerns the special case of ESRF but the pattern is confirmed by most interviewees as common to most facilities. High figures of uptime is always a priority and a prerequisite for the pursuit of the laboratory’s mission, and the work of the accelerator group is therefore essentially a service function, motivating an ‘industrial’ mode of work. The success or failure of the experimental program and the work of every user is ultimately dependent on the existence of a focused and controlled beam of electrons in the storage ring, and so the machine group tend to be like janitors to the regular user: performing crucial tasks but only asked for when something goes wrong.

The experiments division is basically responsible for running, maintaining, upgrading and put to use everything outside the shielding of the accelerator. This involves on one hand technical support and development of beamlines, experimental stations, data collection and storage, sample-preparation facilities, and every other supporting technology, and on the other hand the organization of the user operation of the facility and the mediation between the requests of the users and the capabilities of the laboratory’s facilities. The specific organizational structure of a lab (and its size) determine the exact division of these tasks among scientists, technicians and administrators, but they are generally kept within the same organizational entity, with the exception of Participant Research Teams (see further below). Similar to the machine group, technical operation and maintenance in the experiments division is both a prerequisite for the lab’s overall functioning and entirely a service mission, but the experimental division is significantly more heterogeneous in its responsibilities and personnel.

A few exceptions from this strict cultural divide are interesting and noteworthy. Smaller laboratories with close university linkages, such as MAX-lab, may have a slightly more academically oriented accelerator group, with both a research program and training. Uptime figures of 98% may be hard to reach for technical (and financial) reasons, but also because the research program in accelerator physics may require some risk-taking with accelerator performance (*Mårtensson 2 interview*). Larger laboratories like the ESRF have a more strict service approach (they are even compared with “an army”, *Comin interview*), as they constantly work to reach an uptime figure of 98 to 99%.

Such a machine division's fulfillment of their duties stands in bright contrast to the dynamic experiments program's small science logic. Embodying the conceptual basics of complex and sophisticated science, it points at the existence of material subcultures on an overarching level of the sociology of synchrotron radiation laboratories. The heterogeneity of the experiments division, and the contrast it provides to the apparent straightforward mission and execution in the machine division, shows in clear terms a duality of synchrotron radiation laboratories also with respect to *practice* – a duality partly corresponding to the governance duality as it emanates out of the disunified scientific utility of the laboratory on one hand and its central infrastructural resource on the other.

The disunified experimental program

The 'scientific program', a common collective name for inhouse activities and the work of external users, is subject to an attempted compartmentalization at most laboratories, mostly for organizational and administrative reasons. At larger labs, the organization of the experiments division may entail a hierarchy with beamline groups, sorted according to technology and basic experimental techniques. But such categorization hardly ever corresponds to taxonomies of scientific activities, such as between different subcommittees for proposal review, or the main headlines in Activity Reports. An example from ESRF¹⁶⁴ is found in table 18. The multiplicity and fragmentation escapes traditional categorization, and this is an inherent feature of the lab and how it is defined internally: Laboratory management and staff are reluctant to adhere to stringent taxonomies, simply claiming there is no use. If the laboratory runs effectively and produces science judged to be of sufficient quality, the problem of defining it according to scientific disciplines or areas lies rather with outsiders attempting to understand it – science studies scholars of course but foremost policymakers and science administrators (cf. Gieryn 1983, 1995; Hughes 1987; Palmer 1996; Thompson Klein 2000; in chapter 1).

164. The discrepancy in these categorizations at ESRF is attributed exactly to the above-stated, that review committees correspond to scientific areas while beamlines correspond to techniques and instrumentation: "The ESRF policy on beamlines is to construct beamlines with specific properties of brilliance, energy resolution, spatial resolution, tunable range etc. Each beamline serves users from a variety of scientific disciplines. In order to evaluate the relative scientific excellence of different proposals, they are grouped according to the scientific area of the investigation, and not to the methods used" (*ESRF beamtime allocation policy*, p 3).

Beamline groups

- Surface and interface science
- Materials science
- X-ray absorption and magnetic scattering
- High resolution and magnetic scattering
- Soft condensed matter
- Macromolecular crystallography
- X-ray imaging

Review committees for beamtime application review

- Chemistry
- Electronic and magnetic properties
- Crystals and ordered structures
- Disordered systems and liquids
- Applied materials and engineering
- Environmental and cultural heritage matters
- Macromolecular crystallography
- Methods and instrumentation
- Soft condensed matter and biological materials
- Surfaces and interfaces
- Medicine

Headlines of the scientific highlights in the activity report

- High resolution and resonance scattering
- Materials science
- Soft condensed matter
- Structural biology
- Surface and interface science
- X-ray absorption and magnetic scattering
- X-ray imaging and optics

Table 18: Examples of categorizations of scientific areas and beamlines. From the ESRF (*ESRF Highlights 2007*).

This points at the prevalence of *subcultures* at another level of the sociology of laboratories: within the experiments division, escaping traditional disciplinary taxonomies and stemming from heterogeneity *within* disciplines, fields, groups and projects.

As they expand their battery of utilities and applications, synchrotron radiation laboratories propel fragmentation of scientific areas and techniques and blurring of traditional disciplinary boundaries (*Mason 2 interview*). This is not least seen in the categorization or organizing problem it causes within the labs. At the free electron laser LCLS (Linac Coherent Light Source, at Stanford, see chapter 4), a laboratory that will be the only of its kind when opening, the planning process of choosing techniques and scientific areas for the experimental stations was complicated, due to the novelty of the instruments and the projected experiments, and the discussed fragmentation

of disciplines and areas common to all synchrotron radiation laboratories. Neither a purely disciplinary approach, designing five experimental stations corresponding to five scientific disciplines, nor a purely technical approach, with basic experimental technique as basis for the categorization, turned out practicable. The coordinators had to “make compromises both ways” (*Arthur 2 interview*).

Regardless of what solution is reached, it will amount to a set of (material) subcultures and ‘trading zones’ between them (Galison 1997, see chapter 1) that are partly new. The laboratories and the instruments they host can thereby also be interpreted as “generic instruments” (e.g. Rosenberg 1992; Joerges and Shinn 2001b, 2001c; Shinn and Joerges 2002), because they avoid most labeling according to established standards and because they are utilized for (partly) new purposes. In this, a unifying force emerges – the generic technology is physically tied to a beamline and serves different experimental activities. In a transferred sense the continuous broadening of scientific utilities of synchrotron radiation that has taken place during the past decades can be interpreted as a ‘generic technology’ moving and developing between and within different new and renewed applications.

One actor group has emerged in the material as especially important as mediators between technological capabilities and scientific ambitions or between previously separated branches of science or technology meet in the ‘trading zone’ environment of synchrotron radiation laboratories. Beamline scientists are employed to adapt the technical possibilities of the instrument they are in charge of to the scientific requests of users, to act as the visiting users’ local contacts in the lab and do the in-house research. Beamline scientists (or inhouse scientists without any specific beamline designated to them) often act as ‘promoters’ of synchrotron radiation techniques, finding or inventing new applications for them, in collaboration with user groups or other scientists, and promoting the utilization with starting point in the specific technique. The most imaginative names for them are ‘synchrotron radiation jocks’ or ‘x-ray jocks’.

The x-ray jocks

The identification of the x-ray jock in the complex and multifaceted environment of synchrotron radiation laboratories can be done on basis of the assertions made in chapter 1 and earlier in this chapter that *experiments* and *instruments* sometimes can merge to form a conceptual unity (e.g. Smith and

Tatarewicz 1994; Van Helden and Hankins 1994; Rheinberger 1997; Galison 1997) because the outcome of the experiment to a large degree depends on the skills of the experimenters in controlling and manipulating the instrument, or in some cases even their skill in designing and constructing instruments:

“Science is always at the forefront of what you can do technically. It always depends on the best technology and the combination of best technologies. [...] And if you are a genius and good at combining these things, then you may come up with a unique instrument and you can do new science. And therefore at some point you start working with new sources or certainly on instrumentation or on detectors or whatever. [...] At some point I think you have to do some things yourself” (*Feldhaus interview*).

“It is not a matter of engineering. Engineering is always a step behind. Scientists make instrumentation as something new. Engineers make sure that something that has been done keeps working and working better. [...] If you want to develop new instrumentation, you need to have a scientific goal. You need to know where to go” (*Comin interview*).

The role spoken about in these quotes is often referred to as ‘instrument builder’ or ‘builder’. Scientists whose fields of study lie close to the instrument development frontier will have to build their own instruments in order to do their projects. In the early days of synchrotron radiation, more or less every experimenter had to be instrument builder because there were no ready-built beamlines anywhere (cf. the discussion on Participating Research Teams, below, and the early PRTs at Stanford, chapter 4). As the community grew and became organized, experimental techniques matured and became standardized, with the entirely automated crystallography beamlines as extreme example. There is, however, also a general ambition among most (if not all) synchrotron radiation laboratories to have some unique instrument components at their beamlines and experimental stations even though their basic function is the same as elsewhere. Thus a particular type of experiment can normally be carried out at several different beamlines around the world, but some experiments that require very specific parameters may only be possible at one particular laboratory (*Nyholm 1 interview*). The forefront developments in experiments are still made by groups who include design and construction of instruments in their research program, and the Participating Research Teams that still exist are generally formed around such groups. But the actor group ‘x-ray jocks’ is even more distinguishable, although in its origin, it seems close to the early Participating Research Teams:

“In the early days, the generation that started with synchrotron radiation

really felt very much associated with x-rays. It was a new generation of x-ray scientists. And early on, many of the meetings, people talked about the development of x-ray techniques, what new experiments they had done, and they thought more in the sense that 'Look, isn't this cool? This can be done with x-rays too. And I have used x-rays in this innovative way.' This was the first generation. [...] And so as the community became more mature, actually we had to change. [...] The ultimate value of x-ray facilities was not x-ray science per se, but what scientific problems in areas such as biology, condensed matter physics, materials science, chemistry, and so on, can x-ray solve, that are of interest to these scientific communities. That means people that do x-ray science should develop into a person with expertise in one of these areas, chemistry, biology and so on. And they had to participate not just in x-ray meetings, but they had to have the goal of making a contribution in these scientific fields, that were valued by the scientific community in those areas. [...] So when I use the term x-ray jock, it's somebody who is really interested in developing x-ray techniques and his or her primary interest is really x-rays" (Stöhr interview).

The actor group 'x-ray jocks' has evolved among at synchrotron radiation laboratory staff scientists or beamline scientists with a special interest and competence in an experimental technique, broadly defined, that utilizes synchrotron radiation.¹⁶⁵ As scientists they have a disciplinary base, but since they are employed at synchrotron radiation laboratories to take part in the experimental program – i.e. conduct inhouse research, do user support, and engage in development and refinement of instrumentation – their primary identity is not as 'physicist' or 'chemist' but as 'synchrotron radiation scientist' or 'x-ray jock'.

This role is taken on by some but not all staff scientists (or beamline scientists). The x-ray jock is a person with the ambition to develop *experiments*, and thus not *instrumentation* per se:

"For me the challenges of working here tend to be more in how do I do the experiment rather than how do I get the knowledge. It really is 'is there a better way of doing this experiment than people have done in the past?' That is really very much the sort of quest if you will for me. [...] But just because I'm a builder of instruments doesn't mean I'm not a scientist. [...] Well, the university will disagree with me. They will say 'he is just a

165. The reason they are called 'x-ray jocks' and not 'synchrotron radiation jocks' is probably the rhetoric attractiveness of 'x-ray jocks'. It should, however, not be interpreted as confining the jocks to a certain wavelength spectrum; these jocks certainly also work with infrared and ultraviolet radiation.

builder of instruments'. But I think of myself as someone who is developing a knowledge of how to do things which didn't exist before" (*Brennan interview*).

But there is also an *outreach* function. The x-ray jocks are said to have played an important role in the past decades accelerated growth of the user communities and the number of disciplines present at the laboratories, a role they continue to play. This mediating role of x-ray jocks is especially important when expertise on instrumentation is necessary in order to do a certain experiment, in which the x-ray jock takes active part by contributing to the collaboration according to the principle of 'task heterogeneity', crucial in collaborations making use of sophisticated instrumentation (*Glatzel interview*). The outreach function of x-ray jocks also show by the role they take in the *recruitment* of users. The 'proselytizing' – inviting and convincing users from new areas that synchrotron radiation would benefit their type of experimental work – is to a large degree done by x-ray jocks, who are specialists of certain techniques and engage in collaboration with users to apply this technique to new problems, often on their own initiative (*Brennan, Glatzel interviews*). X-ray jocks thereby *promote* experimental techniques, and their active recruitment of external users to establish a scientific base around it is part of an *alliance building* effort: users are *enrolled* to become part – a vital part – of the case for a certain type of synchrotron radiation experiment. The alliance is not established primarily around a certain scientific fact or claim, as the rationale for alliance building would be according to ANT proponents (e.g. Latour 1983, 1987; see chapter 1). Rather, the motivation is *political* – the promotion of 'their' experimental technique in the multifaceted and fragmented synchrotron radiation landscape. The enrollment of users in this process is arguably among the most important (see further 'the workshop approach', below).

Several of the scientific and technical developments described in chapters 3-6 have identifiable actors at their center that fits the description of 'x-ray jock'. Though not always staff scientists at synchrotron radiation laboratories, but increasingly so as institutionalization of laboratory organization and practices has been furthered, they have taken the initiative to the development of a certain new utility of synchrotron radiation, advertising techniques at workshops and recruiting user groups (*Bergmann 1, Brennan, Pianetta, Stöhr interviews*). As a kind of personifications of the development of modern synchrotron radiation laboratories into multifaceted and broad user facilities, the x-ray jocks are an actor group that stands in the middle of the dual disunified and unified laboratory environment. They mediate between technological capabilities and scientific ambitions, but they are also promoters of

specific areas and projects, and their enrollment of users (bottom-up science) is a way to strengthen their case in the competition for funding (top-down politics). The strongest alliance will have its beamline built.

Participating Research Teams

The existence of Participating Research Teams (PRTs) has political, financial and organizational reasons, and it alters the role and identity of researchers and instrument builders and complicates the meaning of the term 'user laboratory'. The most urgent reason for a laboratory to engage in PRT arrangements with external user groups is the need to obtain funding from external sources. Another reason is the involvement of external competence – often the expertise in specific areas is found among (potential) users – and in both the Stanford and MAX-lab cases this was clearly an important factor. In the case of MAX-lab however, the lab director states that other, non-PRT arrangements for user involvement in design and construction work would have been a better solution if sufficient resources had been available from the start (*Mårtensson 2 interview*). At Stanford, the PRTs were eventually phased out in favor of a centralized organization of the experimental program. Both examples suggest that PRT arrangements have significant disadvantages.

Involvement of the user community is essential in many instances of the running and development of synchrotron radiation laboratories. PRT arrangements can be seen as an 'extreme' form of user involvement, because the teams are typically given full responsibility for construction, operation and maintenance of beamlines as well as user support. PRT arrangements appear to have been the preferable strategy for user involvement at labs in the 70s and 80s. The 1984 DOE report assessing the future demand for light source facilities in the USA expressed an overall positive attitude towards Participating Research Teams, acknowledging the potential of the specialist user community, and recommended extensive use of PRT arrangements in the buildup of the new facilities recommended in the report (what came to be the Advanced Light Source in Berkeley and the Advanced Photon Source at Argonne) (*Eisenberger-Knotek Report 1984*, p 90). The National Synchrotron Light Source (NSLS) at Brookhaven established Participating Research Teams also in order to meet the needs of parts of the user community, who requested exclusive and long-term access in order to develop their experiments (Robinson 1981b, p 314).

It seems as if the status or reputation of the PRT phenomenon within the synchrotron radiation community has since fallen considerably. The third

generation sources built up in the early 90s, with full-scale user support organizations and a comprehensive 'general user facility' strategy, showed the advantages of having a facility fully financed from the start and centrally organized, including all planned public beamlines (most explicitly exemplified by the ESRF, see chapter 6). Drawbacks of the PRT model also showed when the need for general user facilities increased as a result of the widening of the user community to include also inexperienced users. Furthermore, the example of the Advanced Photon Source (APS) had a generally deterring effect. Its extensive use of Collaborative Access Teams (CAT) agreements was motivated entirely by shortage of funds (*Lubell interview*), and was criticized already during the planning stage, by a co-director of one CAT compared to "buying an automobile off the showroom floor, but without the steering wheel, the instruments, or the seats" (Balter 1991, p 795).

There were advantages; as specialized teams in their respective scientific areas, the CATs were successful in obtaining funding for their beamlines and several state-of-the-art instruments were developed (*Lindau 1 interview*). But problems soon emerged, associated mainly with organization and coordination, since all CATs were basically organized as separate research institutes responsible for whole beamlines, beamtime allocation and user support. The lack of overall coordination of the laboratory activities led to duplication of efforts, as well as lack of coherence in the scientific program (*Galayda interview*). In the 1997 *Birgeneau-Shen Report*, the APS was criticized for lack of flexibility due to the CAT system, the inability to accommodate external, inexperienced users to the degree desired, and especially the difficulty associated with removing unsuccessful CATs (*Birgeneau-Shen Report 1997*, p 88). The overall result – ten years later – has been a diminishing reputation for the Participating Research Team model in the whole synchrotron radiation community:

"I think that there are many people today who look at what happened with the APS and say that was probably not the best way to do it, and we shouldn't have done it that way, we should have gone in there and said this is a machine that absolutely has to be built and we need x amount for the machine, and we need y amount for the beamlines, and here is our long range plan for getting there" (*Lubell interview*).

And so the PRT model for harnessing talent in the user community and guarantee vivid research programs at the beamlines has been replaced, partly by an increase in number of inhouse scientific staff and partly by other types of arrangements that arguably lay more of the responsibility and workload

on the laboratories but in return give them unrestricted influence over their beamlines:

“And so at this point what you do, is that you go to the facility and say ‘I have this great idea, you guys should build a beamline for me to do this, and I’ll help you write the proposal to the DOE, but the funding will be from the DOE to you and I will be advisory and we will build this really cool beamline and I’ll do some really neat science with it, but money doesn’t flow through me.’ So that is the modern way of doing that” (*Brennan interview*).

The phasing out of the PRT concept in favor of other, ‘centralized’ mechanisms for channeling initiatives and engaging users in the continuous development and renewal of the laboratories is a clear example of an institutionalization trend: Though the chief scientific and technological competence in a certain area may remain with external user groups, managerial structures have been built up at the laboratories for harnessing such competence and develop it into concrete projects. This ability on part of the laboratories, and the realization of its advantages, has developed out of a gradual establishment of ‘best practices’, in other words the institutionalization of synchrotron radiation. The case of MAX-lab, however, indicates that financial constraints may force laboratories to continue to rely on PRT-like arrangements.

The user facility principle and its exceptions

In chapter 4, the meaning of ‘user facility’ in the case of the Stanford Synchrotron Radiation Lightsource was problematized on the basis of claims that the laboratory was ‘elitist’ or suffered from ‘clubbiness’ and only attracted scientists from within the same community. An oversubscription rate (calculated on number of proposals granted beamtime and not number of shifts applied for and allocated) approaching one would in the normal case simply mean low general demand and no competition at all, if it wasn’t for the possibility of ‘asymptotic behavior’ among users:

“You can live with [an oversubscription rate of] 2, people expect that sort, and it’s a pretty weak facility if it doesn’t have that kind of oversubscription factor. 3 is alright, but when it gets to 4 and 5, what that means is that you have, even if your proposal was pretty good, carefully thought out, and

you're a good scientist, roughly speaking you're only going to get near the beam once every 3 or 4 years. So you won't do it. And that is one of the reasons why I think we might see a sort of asymptotic behavior as people give up and goes somewhere else. Because it is no doubt that it is easier to get on to some other synchrotrons" (*Stirling interview*).

The quote concerns ESRF, but may very well apply to the laboratory at Stanford, which reportedly has been good at cultivating its user community through the years (*Brennan, Hodgson, Knotts, Pianetta interviews*). It is reasonable that such a community does experience 'clubbiness' over time, which perhaps has a similar effect as Participating Research Teams on the transparency and 'general user focus' that facilities normally are keen to advertise. The mentioned 'asymptotic behavior' is an example where the 'Matthew Effect' (Merton 1973, see chapter 1) seems to be at work; getting a proposal accepted to one of these most oversubscribed beamlines may lead to collaboration with its beamline scientists and an enhancement of the chances to get the next application accepted.

The system with 'Long Term Projects' implemented at ESRF is another arrangement that resembles the Matthew Effect and that is put in place as an arrangement of mutual benefit between the laboratory and certain user groups (see chapter 6). At MAX-lab, only two formalized Participating Research Teams are in place despite the fact that every beamline has its origin in the user community and is designed, built and operated with heavy involvement of users (chapter 5), and so it is reasonable to suggest that tighter relationships between certain users and the laboratory – with mutual benefits – are kept in place even though formal PRT solutions have been abandoned.

Interestingly, while the transparency and peer review process for allocation of beamtime is both a basic premise for the activities at most synchrotron radiation laboratories and often strongly advertised, all three cases have exceptions and deviations from strict adherence to scientific merit and technical feasibility as the sole basis for access to beamtime. Peer review is an inextricable feature of science and a mechanism for governance or policy most closely associated with academic, bottom-up, self-regulating scientific activity – 'small science', in short. The organized peer review process that makes up the procedure for allocation of beamtime is one of the most evident manifestations of small science logic in synchrotron radiation laboratories. The exceptions are – directly or indirectly – induced by another logic, namely that of politics and big laboratory governance. The most apparent example is ESRF (chapter 6), where the peer review process is shortcut by politically motivated demands for return for investment among the member countries. At MAX-lab (chapter 5), the extensive involvement of PRT (and PRT-like)

arrangements were put in place not primarily as an alternative strategy of establishing a 'user facility' (as in the case of the APS) but because of the funding model for the facility, i.e. political necessity. The allegations of 'elitism' at SSRL (chapter 4) are not the result of an active policy but rather the long-term effect of the local political context of the lab. The user community has had the time and opportunity to develop itself in accordance with the particular status of SSRL at SLAC and in the US National Laboratory system, and has thereby possibly become less of a broad user facility.

Though very dissimilar in most sociological respects, and especially the political, all three laboratories have a clearly articulated and advertised 'general user facility' policy, and all three laboratories have more or less institutionalized exceptions from this policy, induced by circumstances and factors external to the small science logic of the user communities and the scientific programs at the labs. The details of the governing of synchrotron radiation laboratories are unavoidably influenced by more factors than the inherent logic of small science, and such factors are arguably attributable to the general governance duality conceptualized above.

The local technological imperative

The general themes of the soon fifty-year history of synchrotron radiation are, as described in chapter 3 and in the case studies, expansion and diversification. Technological and scientific developments have extended the realm of the laboratories' activities and their capabilities to accommodate different types of users with different preferences. These developments have occurred both at an overall global level and within the walls of individual laboratories. Together with the dynamic and substitutional character of the laboratories' assembled infrastructure, the historical trend points at a inherently *generic* capacity of synchrotron radiation laboratories (cf. *generic instruments*, Rosenberg 1992; Joerges and Shinn 2001a, 2001b; see chapter 1), a capacity that makes them unpredictable also years after completion of the initial construction.

Original laboratory designs and scientific case descriptions are always developed and presented to the financier before funding commences and construction starts, but may indeed not cover for all future capabilities and utilizations of the lab; it is, in fact, quite the contrary. Incremental improvements of accelerator performance, implementation of new accelerator components or monochromators and optics, substitution or refurbishment of whole beamlines and upgrades or replacement of instruments at experimen-

tal stations – all are possible alterations of laboratory performance and scientific programs. Examples even exist of labs where the accelerator itself has been entirely replaced with beamlines and experimental stations remaining roughly the same (at Stanford, see chapter 4). Often the laboratories are built during periods of several years, so that the storage ring is perhaps not fully equipped with beamlines until as much as ten years after the first beamlines started user operation. This may have economic or organizational reasons – beamlines are very complex and demanding to design and build¹⁶⁶.

The mounting of the first wigglers and undulators in some of the first generation sources – which were storage rings designed and built entirely without synchrotron radiation in mind – is among the most salient examples of the generic capacity, as is the 90s development of insertion device technology that allowed low energy, third generation rings to produce hard x-rays and by that the broadening of their scientific base to cover also life sciences applications that were previously thought to be confined to higher energy rings (e.g. MAX II, chapter 5). The opposite – i.e. the ‘downwards’ expansion of the activities of hard x-ray labs to VUV and soft x-rays – has also happened, as a result of scientific developments that have changed the overall demands of the user community.

As described in previous chapters, the scientific development has gone hand in hand with such technological advances and opened up completely new areas for synchrotron radiation research. It can probably be safely asserted that not many people in the 70s or 80s anticipated that van Gogh paintings, skulls dug up by archaeologists, or original Archimedes manuscripts would be brought to synchrotron radiation laboratories for examination (to mention the perhaps most spectacular examples).

The generic capacity of the laboratory infrastructure makes it principally possible to construct a whole synchrotron radiation laboratory in a fully compartmentalized, step-by-step procedure and makes the opportunities of incremental improvements and alterations nearly endless. It is shown in chapter 3 that while prospects and possibilities may be known in advance, they are only put into practice when crucial steps in scientific, technological, organizational and social developments coincide. Under optimum circumstances, certain actors (*scientific entrepreneurs*) can push this development and make use of the already multi-faceted environment of synchrotron radiation laboratories to move into a new scientific-technological realm. Other laboratories are seldom slow to copy and carry out their own versions.

Because of this fundamental developmental factor and the generic capac-

166. And expensive, for that matter. It may happen that the cost of a beamline fully equipped with instrumentation amounts to a sum comparable with the cost of an accelerator (Nyholm 1 interview).

ity of the infrastructure, incremental upgrades are continuously done at synchrotron radiation laboratories. Directors, staff and users constantly search out new areas and push technology further, and laboratories are in constant state of change, which suggests the existence of a kind of 'local technological imperative' at synchrotron radiation laboratories. Interestingly, it seems the next major upgrade of the laboratory always turns up as a design and proposal for funding about the time when all beamlines are in place and the lab is in full operation,¹⁶⁷ and this pattern has largely been incorporated and embedded in the culture the laboratories.

It is, however, also coupled with some steering problems. Constant negotiation takes place between different actors inside and outside the laboratory organization to achieve the renewal in all its details: the directors make overarching plans for the medium- and long-term scientific programs and coordinate the work on the laboratory floor, the user communities formulate their requests and plans and draft them in meetings and workshops, funding agencies are involved at different stages to give their consent to embryonic plans and thus giving incentives to carry out more detailed planning, and the advisory committees evaluate and approve of plans as they materialize. The 'information asymmetry' problem, built into the 'social contract for science' and complicating the political governance of science, is deepened by the constant but fragmented development. The challenge for directors, funders and steward agencies is to create organizations with ability to adjust to rapid change. The constant development and expansion of activities also indicate that the 'more is never enough' tendency of science to absorb all funding available (Greenberg 2001, 2007; see chapter 1) perhaps apply especially to multifaceted and generic laboratories. Examples from MAX-lab (chapter 5) supports this indication – despite constant allegations of suboptimal user support and maintenance, the laboratory keeps searching out new scientific and technical opportunities.

Historically, laboratories have responded to these challenges in different ways and with varying degree of success, as examples have shown. The desire to place a large degree of initiative and influence in the hands of users has lead to a weakening of overall laboratory ability to change and renewal, as in the case of the Collaborative Access Teams at APS. It is argued that this has had

167. This is common to all three case studies, albeit with variations. SSRL had barely become fully dedicated in the early 90s before plans for the SPEAR3 upgrade emerged (chapter 4). When MAX II started user operation, the accelerator group started making the first tentative plans for the large, next generation MAX-lab, the MAX IV facility (chapter 5). At ESRF, beamlines were built continuously through the years and when the originally planned thirty were running, refurbishment programs started for the oldest ones. When these were concluded, the plans for the major upgrade program started (chapter 6).

a damaging effect on the facility on long term, especially in comparison with the very similar ESRF in Grenoble where the opposite strategy was pursued and all the beamlines originally planned for were incorporated in the central laboratory organization. The overall maturing of both technologies and organizations in recent years seem though to have led to the emergence of a kind of 'best practice' that is copied or at least imitated at most new laboratories.

The local technological imperative seem though to be less dependent on the organizational specifics of the laboratories, suggesting that it has its roots entirely in the bottom-up side of the governance of synchrotron radiation laboratories. It is therefore also a concrete example of how a feature of small science can be translated into the big laboratory context, and there meet the governing ambitions of politics. The pattern according to which new ideas are developed and nurtured in the laboratory shows a comparably simple but nonetheless interesting model for the relationship between the bottom-up and top-down when it comes to the 'acting out' of the local technological imperative.

The workshop approach

The local technological imperative described in the previous section has its roots in a very fundamental trait of science: that it is ruled by a 'principle of novelty', that it always seeks advancement (e.g. Weingart 2000, p 30; see chapter 1). Nonetheless, negotiation between different interests and between the interests of the users and the capabilities of the laboratory is needed for this progress and the imperative to result in concrete plans that can be presented to funding agencies and eventually materialize. Plans for new instruments, or for upgrades or modifications of instruments, always originate among single scientists or groups of scientists, and small talk among them is a forum in which many such ideas are first drafted: "Building an instrument is something that usually comes from a simple idea, just out of a talk" (*Comin interview*).

The inclination to initiate instrument projects varies among scientists – x-ray jocks are identified above as an actor group specifically engaged in such initiatives. The promotion of initiatives and the selection of ideas that are eventually realized is, however, a *collective* process that involves user communities and various parts of laboratory organizations. For the most part, it is about creating and establishing *credibility* around the proposed instrument or upgrade, amounting to two things: the technical and scientific *feasibility* of

the project and its *usefulness* in terms of support among the prospective user community:

“If you want to be successful in this you need to establish support in strong user groups. So what you generally do if you have an idea of your own or an idea from a user is that you organize workshops and summon people and exchange views and then you arrive at a proposal. And then you generally bring this proposal to SAC who tells you it’s fine. But you need to have external support that you can prove” (*Kvick interview*, similarly described by *Brennan interview*).

Instrumentation-related workshops are ubiquitous in the synchrotron radiation community, dealing with all kinds of scientific and technological features of synchrotron radiation, generally but in many cases also concerning specific ideas or proposals for instruments. These workshops – especially the ones with a general agenda – also tend to have a kind of constituency role for the various scientific communities in synchrotron radiation because they convene representatives of fields and align their collective priorities with regard to instruments and new directions. Most of all though, it is through the workshops that instrument ideas and concepts are promoted and selected, and given the necessary broad credibility.¹⁶⁸

Interestingly, this ‘workshop approach’ seems to be at play regardless of the size of the project being drafted. The free electron laser project LCLS at SLAC was originally a small scale idea, mainly in the interest of some accelerator physicists, but through the years and especially through the workshops, it turned into a \$400 million project and a future major user facility. The sequence of workshops on technical design and scientific utility – with continuously growing groups of participants – is said to have slowly but surely mounted interest, credibility, involvement of needed specialist competence, and on basis of it all the crucial support from the funding agency:

“It developed through these workshops, the 92 workshop, the 94 workshop, the 96 workshop in Grenoble, there was another following workshop. [...] And at that time we here in the US were thinking we needed to gather more support for such a facility if we were going to get money for it, so we started putting together a series of workshops here, and the DOE was encouraging us to do that, they were saying ‘we can maybe give

168. The role of the Scientific Advisory Committee is in most cases to provide ‘expert’ credibility to the proposals for new instrumentation, while the general support from user communities rather signals the existence of a user basis (*Kvick, Mårtensson 1 interviews*).

you money for something but you've got to really prove that you have a strong support for it.' [...] That was a key thing the DOE needed in order for them to say ok, this is worth committing a bunch of money to" (*Arthur 1 interview*).

Seemingly regardless of the size of the project, the 'workshop approach' seems to be the model through which the local technological imperative acts out in the duality of top-down politics and bottom-up science. Projects originate among individuals or groups and are slowly developed through a process dominated by workshops, and eventually a concrete proposal, sufficiently developed scientifically and technically and with a considerable host of support from prospective users, takes shape. Depending on the size of the project, politics is involved at different stages, but the basic relationship between the bottom-up scientific initiative and the top-down politics governance acted out through the workshop approach is proactive on part of the science and reactive on part of politics, with a basic framework set from the start. Science submits its proposals, and politics decide who gets the money.

The user community

The most obvious manifestations of the institutionalization of synchrotron radiation is probably the growth of its user communities in sheer numbers and its growth in disciplinary breadth. Examples from the case studies in chapters 4-6 show that the internal development at laboratories – including purposefully executed organizational changes – has enabled an increased capacity to accommodate users, in sheer numbers and from a greater diversity of areas. The development and growth of the user community has been coupled with a development of the 'user facility' concept, and nowadays there is hardly any synchrotron radiation laboratory that would not call itself primarily a *user facility*, with a mission to serve an external user community. Such is the expectation put on laboratories by funding and steward agencies and the rationale for keeping the labs in operation and continue the investing in them (*Galayda, Glatzel, Hedman, Larsen, Lindau, Mason, Stirling interviews*).

The expanding of the user base has been enabled by the technological and scientific development, but in many cases potential new applications have not been voluntarily explored by representatives of scientific fields but rather brought to the attention of potential users by the *recruiting* work of x-ray jocks. Laboratories may also actively recruit scientists from certain disciplines to build capacity in new areas (*Eeckhout interview*).

The probably most wide-ranging change at the laboratories has been done as a result of the development of crystallography applications of synchrotron radiation. In order to accommodate users whose interest in instrumentation is very limited and who expects turnkey operation, much technical work was needed, but also adaptation of user support routines, safety, scheduling and a lot of other details:

“There has been huge change. Necessary change. Because if you want to bring in research areas to whom the machine and what it produces is a black box, nothing but a black box, they only know the characteristics of what it produces, nothing else. It runs through it all, from safety, you really have to think the system through so nothing can be done to hurt not only people, like with radiation, but also harms instrumentation. The difference when you work with physicists who are in the lab all the time, there are things they simply don’t do, so to speak, they don’t turn the knobs as far as they can without looking at a meter or something. So there is that mindset, plus how you communicate, you’ll have to use the language relevant to the researcher and not the ones running the machine or getting the monochromators in shape” (*Lindau 1 interview*).

“In order to be successful either you’ve got to be able to develop the techniques and the tools and the instrumentation to take care of those users through let’s call it the engineering approach, or you’ve got to have the people who can sit down and handle those users to have them be successful” (*Hodgson interview*).

One salient feature of synchrotron radiation laboratories very much associated with the size, breadth and character of the user community is the competition for beamtime and the oversubscription, which makes beamtime a very valued commodity. This has the immediate consequence for all users that beamtime must be utilized as effectively as possible. Even though there is huge difference in length of beamtime slots – some researchers spend only one or two days in the laboratory, whereas some stay for several weeks – all groups focus very sharply on the experiment, and this is visible in many ways. Scientists hardly ever spend any time outside the laboratory, the guesthouse and the canteen. They typically work in shifts so that they can make use of the beamtime around the clock. The laboratory in itself and the pure occasion of working on a beamline thus transforms a professional researcher, lecturer or administrator into above all an *experimenter*, having in sight only the single goal of finishing the experiment on time and with good results. In most cases this transformation or metamorphosis shows itself most clearly in the ex-

haustion of the experimenter. It is common to work 18 hours a day and ration resting time down to six hours, portioned entirely according to the schedule of the experiment and regardless of the time of the day. In general researchers complain more about the exhaustion and the difficulties of adapting to the different situations at home and at the laboratory, than express concern over the performance of their experiment or the quality of the results.

Though of course situated in science and in a specific discipline and a specific institutional context, the experiment carried out on a beamline at a synchrotron radiation laboratory is most of all defined by the material setting of the particular laboratory and the particular beamline (cf. Smith and Tatarewicz 1994, pp 101, 108; Van Helden and Hankins 1994, p 4).

When studying a scientist 'in action' at a beamline at a synchrotron radiation laboratory, it becomes apparent that the experiment is indeed the center of attention and that its material and technical features have heavy influence in defining (a) science. And it is rather when the experiment is made to *work* that beamtime is perceived as having been utilized efficiently and successfully (cf. Rheinberger 1997, p 27; Knorr Cetina 1981, p 4; Mulkay 1981, p 164).

Experiments and measurements

Different users interact with the laboratory to very different degrees. This is true for their involvement in laboratory affairs and scientific and technological projects at the laboratory, but also their practical work mode – the degree to which they modify and customize equipment and work together with laboratory personnel. Behavioral differences can to some extent be derived from users' disciplinary and institutional belonging, and thereby categorized.

The most general classification of user behavior is between users doing what may be called 'experiments' and 'measurements'. The degree to which samples and equipment need to be modified and adjusted during the beamtime is the basis of this classification, and it also has implications for the length of beamtime slots and also user involvement in laboratory affairs. The distinction is far from sharp but commonly agreed upon as visible and valid, by users themselves as well as by laboratory directors and personnel (Brennan, Cerenius, Eriksson 2, Fahlman, Flodström, Kornberg, Larsen, Liljas, Mason 1, Mårtensson 2, Nyholm 3, Pianetta, Ursby interviews¹⁶⁹). The classification draws on fundamental characteristics of the sciences it represents

169. The following paragraphs are entirely based on these interviews and therefore specific references are given only in connection with direct quotes and the like.

and their relationship to synchrotron radiation, and therefore it is interesting and useful as analytical distinction. However, the argument should be read with some appreciation of the fact that it is based on a generalization and that exceptions always exist.

The fundamental difference between 'experiments' and 'measurements' is that the former generally are done on longer beamtime slots (typically a week, sometimes two weeks and on rare occasions even longer), involving sample preparation and modification in the lab and users' interaction with the instruments in the form of alteration of components to change experiment parameters. 'Measurements' on the other hand typically require only a day or two of beamtime, and are done on fairly standardized equipment. Samples are prepared in the users' home laboratory and brought to the synchrotron radiation laboratory ready to be used, and the practical work at the beamline consists mostly of changing samples and processing collected data. 'Measurement' work of this type is done in crystallography and other similar chemistry or life science applications, for which synchrotron radiation is primarily a method of analysis and the 'real' laboratory work is done elsewhere, mostly in regular university laboratories and the like. The most extreme cases of synchrotron radiation 'experiment' work can be stretched over beamtime periods of several weeks and perhaps not even possible to do unless the group of experimenters can bring some special instruments of their own to mount on the beamline.

For both categories, beamtime is a precious asset, but in different ways. For the scientists doing 'measurements' at synchrotron radiation laboratories, this measurement is often the crucial piece of work that concludes months of laboratory work in the home institution and provides the key information in the understanding of a biological phenomenon or process. The measurement work might be of routine character, but the information it unveils is decisive. For the 'experiment' scientists, the work in the synchrotron radiation laboratory is their prime or even only laboratory work, and they conclude a whole experiment during the beamtime run.

Scientists who spend weeks rather than just a day or two in the laboratory influence the general lab environment to a larger degree. 'Experiments' users work more actively with instruments and lab equipment over a longer period of time, and this leads to a deeper involvement on their part in laboratory affairs, for three partly overlapping reasons. First, their longer presence and deeper collaboration with laboratory staff make them acclimatized and enable the evolution of tighter professional relations with the laboratory and its staff. Second, they are to a larger extent involved in instrument development because their equipment is more flexible and often adjusted by the users themselves to fit their needs. Commitment to instrument development inevi-

tably makes users involved in laboratory strategy and planning, as their initiatives must fit technically and be weighed into the overall development and strategy of the lab. Third, 'experiment' users have a longer history as users of synchrotron radiation, which has made them more 'native' users. In comparison, 'measurement' work is a rather new feature of synchrotron radiation laboratories, and very much of the community in and around the laboratories was established and built up at a time when all synchrotron radiation science was of 'experiment' nature, i.e. non-standardized and requiring technical skills and longer beamtime periods.

One of the most far-reaching developments in synchrotron radiation science, especially during the past two decades, has been the incorporation of 'measurement' users in the scientific programs at the labs and the extensive technological and organizational adjustments to accommodate these users. As noted in chapter 3, some argue that the fields where synchrotron radiation has had its most significant impact is in chemistry and the life sciences through the crystallography applications (Wakatsuki and Earnest 2000, p 13; *Stöhr interview*). The far-reaching developments of instrument automation, and the exceptions and special solutions in beamtime allocation and scheduling processes that have been put in place at most laboratories is part of a comprehensive strategy to answer to and sustain the increasing importance of x-ray crystallography (and other life sciences applications) with synchrotron radiation. The mail order service at ESRF (chapter 6) and the remote control program at SSRL (chapter 4) are mere examples of the efforts at synchrotron radiation laboratories to meet very specific wishes of a scientific community. The 90s and 2000s quantitative growth of life sciences utilization of synchrotron radiation is unmistakable. Between 1995 and 2008 the annual number of protein structures solved by synchrotron radiation and reported to the Protein Data Bank has had a 30-fold increase¹⁷⁰ (*Protein Data Bank Website*). As noted in chapter 6, the 1987 *ESRF Red Book* envisaged use of only half a beamline for macromolecular crystallography, and the facility today operates three beamlines with seven measurement stations for the purpose. These developments together make up an important part of the institutionalization of synchrotron radiation. Streamlined facilities, large turnover of users, and a mode of work resembling a conveyor belt are probably as 'mainstream' as experimental work can get.

170. From 167 to 5,126. The synchrotron radiation share of the total number of protein structures deposited at the Protein Data Bank increased from 17.2% to 80.9% in the same period (*Protein Data Bank Website*).

The governance duality

Throughout the case studies and in this chapter, the institutionalization of synchrotron radiation in science and science policy has been framed in terms of a 'governance duality'. The interpretation holds that the institutionalization has positioned the laboratories in the nexus of two strong governance forces, briefly described as 'bottom-up science' and 'top-down politics'. These two forces are traditional in their institutional foundation, i.e. they operate according to established and known patterns, and the institutional framework of synchrotron radiation laboratories is the result of their combination. The three analytical themes presented in the introduction and utilized throughout the thesis are complementary explanatory patterns for the institutionalization that has created synchrotron radiation laboratories and their governance duality.

While the key to understanding the top-down politics logic arguably is the classic 'follow the money', the bottom-up science is probably best explained by 'follow the scientist'. The political top-down governing of synchrotron radiation laboratories is manifested by the investments done in the lab, its infrastructure and instruments, and it has an ultimate 'say' in enabling the lab's existence and survival. Politics defines the *framework* for the laboratory activities by deciding on the overall budget and exerting the influence that follows with being the investor and funder, and its governance tends to be of *enabling* character. The scientific force is manifested by the multiple small scale and temporary – and for the most part externally motivated – agendas of scientists or groups of scientists who make use of the laboratory resources for their own purposes and whose collected decisions and preferences ultimately decide the *content* of the laboratory activities. For specific projects the political decision process starts its involvement only after the scientific and technological preparatory work has reached a certain level of solidity. Therefore the original initiative lies with the scientific community or parts of it, while the ultimate decision remains in the political sphere.

There are a number of concrete ways in which the scientific communities populating synchrotron radiation laboratories exert their bottom-up governance. They all originate in the agendas of the users – whether manifested by the collected body of beamtime applications, the proposals for new equipment drafted and formulated in workshops and meetings, or the input of users' associations to laboratory management. The present chapter has provided generalized and synthesized observations of these mechanisms of user input that makes up the backbone of the bottom-up scientific governance of synchrotron radiation laboratories. They thereby complement the specific examples of scientific bottom-up initiative and action provided by the case

studies. In all instances, initiative is grounded in the ambition of individual scientists or their groups to do a certain experiment or type of experiments. The role of scientific entrepreneurs shall not be underestimated, and two primary types exist. In the early days, as shown in the case study chapters, one type of scientific entrepreneurs drove the development by promoting entire laboratory projects and areas of utility. With the institutionalization, the entrepreneurial role needed to be taken on by other actors as users became more 'mainstream', and the crystallization of the actor group 'x-ray jocks' is crucial in this. They establish alliances and enroll user groups for the promotion of their projects, typically through the 'workshop approach'. Their professional identity as 'scientists', 'technologists', or 'experimentalists' is blurred and partly replaced by their role as x-ray jocks or synchrotron radiation scientific entrepreneurs.

The top-down political governing is especially visible in a number of examples in the three case studies. The European Synchrotron Radiation Facility (ESRF) (chapter 6) came into being by the agreement between France and Germany – two major European countries who agreed to pay more than half of the costs if the facility be located in France. With the site-selection process settled by a shortcutting of the 'scientific' procedure and thereby the chances for the other prospective member countries to become host for the ESRF completely gone, the negotiations that eventually settled on the shares of the ESRF costs became tinted by politics. It was allegedly the work of high-level politics that made Italy enter at too high a share of the costs and the United Kingdom and the Nordic countries to enter too low. The political strains embodied in these negotiations were apparently both important enough to make the member countries guard their respective shares of the experimental time at the facility heavily (through the scientific fair return system), and trying enough to freeze them at their original level, although it is generally agreed that they are not particularly well-balanced. At the same time, ESRF enjoys certain advantages of being the result of an intergovernmental collaboration with a certain amount of political prestige involved. The overall construction budget was large enough to cover for the complete laboratory including thirty public beamlines, and the operations budget was set at a level allowing for significant annual capital investment within the ordinary budget. These partly politically enabled financial features allowed the ESRF to take and maintain the sort of scientific leadership role included in the laboratory's original ambitions and are concrete examples of the influence top-down politics may have on laboratory development through *framework* and *enabling* governance.

The comparison with MAX-lab's position in the Swedish science policy system could hardly be more striking and illustrative for the concrete outcome of politics influence on large scale scientific infrastructure in general and

synchrotron radiation laboratories in particular. The lack of comprehensive policy and funding strategy for the Swedish national facilities and MAX-lab (chapter 5) has indeed not meant that MAX-lab has not thrived scientifically or not been upgraded and developed technologically through the decades, it is indeed quite the contrary. But MAX-lab has been forced to search out alternative sources of funding, from different sources, and this have reportedly given birth to a 'shadow economy' where the collected Swedish effort in synchrotron radiation is hard to assess and the overall value or importance of MAX-lab for Swedish science and in the Swedish science system is hard to evaluate. As noted in chapter 5, it seems MAX-lab largely has come into being *despite* rather than because of the character of the Swedish science policy system. The interpretation that scientific entrepreneurs (of the first kind, different from the x-ray jocks) in Sweden have been clever enough to 'fool' the system – by getting the research council and the government to invest comparably small amounts of money in a facility and leaving the questions of operational costs and complementary investments to the future – suggests that a strong bottom-up initiative may counter and cover for weak top-down political governance.

Despite the complaints over suboptimal funding and lack of clear political commitments, MAX-lab is in international comparison an extraordinary phenomenon.¹⁷¹ MAX-lab has also been successful in establishing its own local management structure, part of which is the 'shadow economy' but part of which is also the extensive informal networks and the efficiency that follows from it. A speculative conclusion is that lack of comprehensive top-down governing of the lab has been part of creating its alleged ingenuity and scientific excellence, but the important conclusion in this context is that a weak system may provide *opportunities* just as well as hindrances.

Though the reasons for MAX-lab's inability to "compete with the ESRF" (*Cerenius interview*, see chapter 5) is due mostly to the difference in technical optimization of the respective facilities with regard to spectral range, the organizational differences that ultimately are the result of the very different funding models have a certain importance. The interesting point to stress is the mixed messages that are given on exactly this matter: While claims that MAX-lab is underfunded and that user support is below acceptable standards are repeated in interviews, reports and evaluations, the high scien-

171. No country of Sweden's size have had a user facility for synchrotron radiation in operation until the 2000s (CANDLE is under construction in Armenia; Singapore Synchrotron Light Source (SSLS) and the Swiss Light Source (SLS) both opened in 2001) and at the time of MAX-lab's inauguration in 1987, only China, France, Germany, Italy, Japan, the Soviet Union, the United Kingdom and the United States – all scientific 'great powers' in comparison with Sweden – had their own synchrotron radiation laboratories (see appendix).

tific performance of MAX-lab is continuously highlighted by the very same sources. Something similar is the case for the Stanford Synchrotron Radiation Lightsource (SSRL, chapter 4), where laboratory management complains about suboptimal funding but simultaneously claims that their experimental program is and has been successful, also in its details, to the extent that their organizational model now serves as a model for other synchrotron radiation laboratories.

In the United States, an especially interesting case is the review of the Advanced Light Source and the SSRL in the so-called *Birgeneau-Shen Report* from 1997 (see chapter 4). This episode is an interesting example of the top-down politics of research infrastructures of this size because it shows the limits of its influence. The political plan was reportedly to shut down SSRL, but when the scientific evaluation showed high performance on behalf of SSRL, a shutdown was not possible. With ALS, the scientific evaluation showed suboptimal performance and under different circumstances a shutdown might have been the result, but with investments of over \$100 million in the facility in the past decade it would not have been a politically possible course of events. The conclusion to draw from this particular example is perhaps that the overall science policy system in which these laboratories function has certain checks and balances. Such a conclusion is reasonable not to limit to the American case as the basic relationship between politics (the principal) and science (the agent) is similar elsewhere, but the example is on the other hand typical to a case where several domestic laboratories of similar size are operated by the same agency.

The unification and disunification duality

As mentioned in the previous section, the bottom-up scientific governance of synchrotron radiation laboratories involves scientific entrepreneurs but it is also exerted through extensive input from the user communities. The laboratories in the three case studies were all built to serve external user communities and operate as primarily user facilities, as are most synchrotron radiation laboratories around the world. Though ESRF and SSRL have a well-articulated strategy to keep an inhouse research program of as high quality as possible – and some even claim that the inhouse research is of higher quality than that of the users (*Kvick interview*) – these programs are maintained to maximize the preconditions for a vivid external users program rather than to compete with those users. The result is a service institution with clearly defined ‘customers’, whose comfort and vigor is put first. Because these ‘cus-

tomers' are external to the laboratory and because the user community is constantly changing, the scientific bottom-up governance of synchrotron radiation laboratories is – or is supposed to be – more in touch with the needs user community than it would have been if the scientific governance of the laboratory was done exclusively by a group of inhouse scientists. The flow of users through the laboratory, which can make its floor seem deserted and increase isolation between groups (see especially ESRF, chapter 6) is also what guarantees that the mission is fulfilled, because the constant input of the users – for example by the mere deposition of beamtime applications – gives the foundation for the laboratory's strategic choices regarding its scientific program and the associated development of instruments (*Brennan, Krech interviews*). This way of the user communities to exert their bottom-up governance of the scientific activities at the laboratories is common to all the three cases, with minor exceptions.

The fact that this extensive input of the user community is the major part of the bottom-up part of the governance leads to the conclusion that as scientific institutions – built and operated for the scientific communities to make use of to the best of their ability – synchrotron radiation laboratories are self-sustaining. Examples may prove the point. A beamline that was built at the ESRF on basis of other requests than those of the user community, namely from representatives of local semiconductor industry who wanted an experimental station to measure impurities on silicon wafers, eventually failed to attract a sufficient number of users to be 'beneficial'. The interesting part of the story is that the ESRF at first turned the proposal down because there was no clearly-stated support for it in an 'ordinary' user community. The beamline was, however, built with external EU money. As it turned out, there was no demand for beamtime at all – not even from the prospective industrial users¹⁷² – and so the project was terminated (*Rodriguez Castellano interview*). Though the ESRF invested comparably little money in this failed project, it is agreed by most interviewees that a beamline with no users¹⁷³ is something of a disaster, given the global competition for beamtime and the preciousness of high-end experimental facilities.

While the scientific spearhead competence of the user community is the ultimate source of laboratory development, it is often argued that a certain

172. This episode resembles the short experience with lithography that synchrotron radiation had in the 90s (see chapter 3) in that the demands of industry for reliable production line operation of the instrument could naturally not be met by the laboratories, and it was the same in this case (*Rodriguez Castellano interview*).

173. No users ever, that is. There are numerous examples of beamlines where demand goes down significantly with time so that they may be severely undersubscribed (see chapters 5 and 6) but that normally happens only after decade-long operation.

amount of scientific 'leadership' is needed to guarantee it on long-term. Users are said not to always know what they want (*Kvick interview*), and here x-ray jocks fill an important role. Furthermore, when initiatives come from the user community there is always a risk that instruments are designed and techniques developed for existing needs with too little thought of the long-term development in the field and at the laboratory and the future needs of actual and potential users (*Mårtensson 2 interview*). Another duality of synchrotron radiation laboratories – or another dimension of the governance duality – it hereby identified. While science is intrinsically disunified, self-organizing and evolutionary, the laboratories are materially and socially unifying, return-driven, stringently organized, and governed by a hierarchy with politics at its helm. This duality should perhaps be analytically treated as a local or internal version of the governance duality. Both the 'scientific' disunification and the 'laboratory' unification exist and play crucial roles in the shaping of synchrotron radiation laboratories, and while the 'scientific' disunification ultimately draws on science's inherent and ancient characteristics and corresponds to the bottom-up style of governance built into science, the 'laboratory' unification is a result of recent developments in science, partly induced by politics and certainly occurring in reciprocity with politics, and the political tendency to view the laboratory as unitary; a strategic asset or piece of investment. The two coexist in synchrotron radiation laboratories, and it is their combination that *creates* the synchrotron radiation laboratory – the small science and the big machine.

The simultaneous unification and disunification plays out in practice in a number of ways. In the material aspect, synchrotron radiation laboratories are unified because they are spatially demarcated workplaces for experimental science and built around a large accelerator, and disunified because they host complex collections of instrumentation with which the science is done, scattered around the ring. In parts, the infrastructure and instrumentation is rare, unavailable or inaccessible elsewhere, or in some cases even unique. As hosts of wide varieties of experimental work in several branches of science, but also with certain competitive advantages and particularly attractive facilities, synchrotron radiation laboratories could be called *research centers* or even *research hotels* – the presence of active experimentalists is restricted in time and for the main part, the laboratory is a resource to be exploited for these scientists' purposes. In most cases beamtime is awarded to experimental groups without specific demands of institutional, national, or disciplinary belonging. Accordingly, the largest part of the users is miscellaneous with respect to professional and personal traits and is of an institutional belonging external to the laboratory in question. But the user community becomes unified because they all need synchrotron radiation to do their experimental

work. Had an x-ray tube sufficed, they would have stayed in their home laboratories. The unification was achieved through purposeful efforts on part of the laboratories – the recruitment of a user community – and it took time and required simultaneous technological and organizational developments and adjustments.

The actor groups whose roles have been discussed in the previous chapters and preceding sections are rightfully the center of attention for sociological study of synchrotron radiation laboratories. Unified and disunified simultaneously, the laboratories are dynamic and contingent, and their activities are shaped by the long-term and short-term negotiations between the different actor groups. In its fundamentally *enabling* role, politics set the framework for the laboratories' activity and ultimately their mission. Directors, whose role is similar to the management of a CEO, have an overarching operations responsibility. The scientific program is carried out by external users and in-house scientists, and the x-ray jocks take on the role of bridging scientific ambition and technological capability, recruiting users, and enrolling material and social resources in the establishing of credibility around novel and often transdisciplinary ventures. All the actor groups inside the laboratory organization have a largely mediating role because their work is basically to make the collected ambitions of a diversified user community and the agenda of policymakers meet and coproduce a productive laboratory environment. Recurrently, this intermediary, negotiated and *creative* role is coupled with the seizing of initiative and influence in science by the actors in the laboratory and in a transferred sense by the laboratory as a whole.

Institutionalization frameworks

The institutionalization of synchrotron radiation in the sciences and in science policy systems – identified as synchrotron radiation transformed from esoteric endeavor to mainstream activity – has taken place parallel and in correlation with broader and deeper developments in the sciences and their political spheres. The development has been catalyzed by the work of scientific entrepreneurs, by their ability to make use of the system and its dynamic features, also when it's changing.

In chapter 1, establishing the theoretical framework for this study, certain fundamental traits ascribed to laboratories, scientists, instruments and experiments were discussed. These may have seemed only remotely related to the descriptions and analyses in the subsequent chapters, but they have importance for the basic assumptions of this thesis – that laboratories can and

ought to be viewed as institutional embodiments of developments in science and science policy, that laboratories as institutions can partake in the shaping of developments, and that scientific entrepreneurs play significant roles in science also when it is 'industrialized' or collectivized.

Contrary to philosophical epistemology's view of science as unified by method and early sociologists' belief in moral behavior as criteria for scientific purity, the starting point for this study was an understanding of science as disunified and contingent and shaped by social and material circumstances. This is to say that the *contents* and *practices* of science are interdependent, and that both are embedded in politics and society. The social and material *ordering* of science – not *order* as something static, it shall be emphasized – is just as important for the shaping of science as the ambitions and preferences of its individual performers. Experimental science is just as much about manipulating the material and harnessing technological opportunity as it is about describing an independent world 'out there', and so science is heavily dependent on technology and other material settings. Experimental science is in many instances about *making things work* rather than answering a particular question. The material and social complexity of modern science makes the creation of *alliances* necessary. Such alliances may be traditional and long-lasting disciplinary groupings, but part of the theoretical framework for this study is that they are increasingly heterogeneous and transient. In one interpretation, the constant shaping and re-shaping of alliances and the cross-fertilization taking place within and between them is what drives scientific and technological progress.

On basis of this, laboratories – and especially those with material (and social) settings inaccessible elsewhere – emerge as loci of scientific progress and as institutionalizations of science's material and social dynamics. The disunification of science is countered by the unification of multidisciplinary laboratories, like synchrotron radiation laboratories. This is a fairly straightforward observation; scientific work takes place at identifiable geographic locations, for organizational reasons and for effective resource utilization. Technological resources and the social ordering with which they are combined can be generic and put to use to a host of scientific purposes – but as long as they are not very cheap and easily accessible, the sciences intending to utilize them will have to assemble at certain locations and adapt to certain institutional practices. Simultaneously exclusive and multidisciplinary laboratories have a tendency to both manifest and shape particular trends in the dynamics of science and science policy exactly because they are modern-day unifiers of a disunified scientific enterprise and because they partake in forefront scientific activity. It has been a chief intention of this thesis' empirical accounts and the accompanying analysis to show that synchrotron radiation

laboratories are particularly suitable examples of such exclusive, multipurpose, forefront and formative institutional constructs in science.

The institutionalization of synchrotron radiation was most of all an expansion of the *user base* of the laboratories, coupled with changes in user-laboratory relationships. It was gradual and enabled by technological development but catalyzed by institutional and organizational change and in its entirety it was also social and psychological. As much as it constituted a broadening of the function and activity of synchrotron radiation laboratories, it was also transformative for the disciplines and subdisciplines that became incorporated in the laboratory 'constituency'. The process can be interpreted as a continuous *lowering of thresholds*, technologically and socially. Synchrotron radiation's potential benefits to a large number of scientific disciplines were realized theoretically at an early stage, but achieved in practice only after step-by-step implementation of changes in institutional structure and gradual buildup of routines and practices. Parallel with technical and organizational developments and competence building, the *credibility* of synchrotron radiation needed to be established in broader layers of the scientific communities. Convincing evidence had to be brandished that technology and surrounding institutions could not only perform adequately but also deliver in accordance with specific demands of user communities. Among the most vivid examples of this threshold lowering is the emergence and growth of 'measurement' activity beside the 'experiments'. But synchrotron radiation is also a global phenomenon, and institutionalization also entailed global establishments of *best practices*. One example is the almost complete abandonment of the PRT system in favor of a laboratory-controlled, centralized system of harnessing talent and competence and putting it to use. Local infringements of the general user facility policy that still exist at laboratories and that have been discussed in the case studies may provide an 'exception that proves the rule' in this global context but should perhaps rather be seen as a remnant of the 'un-institutionalized' synchrotron radiation laboratory of past times. One interpretation of the lowering of thresholds could be that synchrotron radiation is a *generic technology* and that its institutionalization is the process of putting it to use for various potential purposes, a matter that will be returned to, but it should be borne in mind that the process was not only technical but also sociological.

Institutionalization can of course be also viewed through the institutions it creates, i.e. the laboratories. Such an interpretation would yield the conclusion that synchrotron radiation laboratories have grown into strong *actors* or *assets* in science and in science policy. This strengthens the case that synchrotron radiation laboratories not only manifest trends and developments in science and science policy but also partakes in shaping them. Synchrotron

radiation laboratories are policy-shaping institutions both in an *actor* and *asset* interpretation and thus deeply embedded in the overall changing role of science and technology in politics and society.

Synchrotron radiation and the changing dynamics of science

Several trends are identifiable as part of the changing dynamics of science. The slowing down of science's growth, partially novel demands for productivity and accountability and an altogether *instrumental* view on science's knowledge production has led to increased competition for resources and recognition, and increased demands from funders and surrounding society. This has in turn provoked reshaping and restructuring of disciplinary, professional and institutional categories and boundaries in science at a new intensity and pace. It has entailed broader sociological changes, conceptualized as *collectivization* and *sophistication*. In this context, synchrotron radiation laboratories can be identified as new not only with respect to their scientific and technological content, and not only in their practices and in its politics, but in the way the politics and practices of synchrotron radiation laboratories mirror or embody overall trends.

Synchrotron radiation laboratories are highly sophisticated and collectivized workplaces of science. The technical developments described in chapter 3 and in the case study chapters and summarized above as the technological pieces in the institutionalization and 'threshold lowering' are clearly *sophistication* developments, creating 'hi-tech' scientific instrumentation and providing it to growing scientific communities in a growing number of scientific areas. The changes in organization and social structure of the laboratories have been described as similarly important as the technological development, and these sociological changes carry *sophistication* to the sciences concerned. As exclusive pieces of scientific instrumentation or infrastructure, synchrotron radiation laboratories heighten competition, and in the specialized sub-fields for which synchrotron radiation becomes indispensable, they become a focus of competition and driver of competition.

Sophistication drives collectivization on two fronts. First by its exclusive and competitive force; sophisticated instrumentation and infrastructure imposes collaboration upon scientific undertakings to increase efficiency in resource utilization. This does not necessarily mean that the research becomes collectivized in terms of how a certain project is carried out, only that scientists and scientific work agglomerates at certain geographical locations.

Second by enforcing changes in the sociology of scientific undertakings and expanding science's organizational units, as a result of complexity and diversity of technological (and scientific) tasks. The heterogeneity in competence and skills required to operate a sophisticated instrument necessitates a collectivized organization, in the most apparent examples very large teams with broad sets of complementary knowledge and skills. The empirical material and the analysis in the previous chapters show clearly that synchrotron radiation laboratories are examples of collectivization of both sorts.

Changing dynamics of science is in a wider context seen in the advent and establishment of *projects* and accompanying *teams* as the prime organizational entities for scientific undertakings. This development of a new organizational pattern for science is ascribed to the changes in science's position in politics and society. Increased competition, new demands for accountability and returns, and scarcer supply of resources are said to provoke more clearly distinguishable undertakings (projects) and identifiable and more competitive organizational entities (teams), as well as faster turnover of them both. Synchrotron radiation laboratories are not as easily identified as instigators or drivers of such a trend, but perhaps sustainers and doubtless manifestations of it. The valuable commodity *beamtime* is almost exclusively awarded according to the *team* and *project* pattern.

Synchrotron radiation and the changes of science policy

The trends described in the previous section have a political context and a political dimension, and the transition they amount to correlates with broader changes in the politics of science. The exposé over the rise and fall of big physics in chapter 2 sought to explain the political and scientific realities in which synchrotron radiation laboratories first emerged, but also to hint at a shift in science's positioning in politics and society and the evolution of the big science/little science dichotomy and its political dimensions. Developments in the late 20th century show that the old logic and rationale for the spending by superpowers (and some other countries, such as Germany and Japan) of enormous sums of money on scientific ventures with little or no economic or societal benefit but a remote (or in later days imagined) connection with military/strategic ambitions, has been almost completely wiped away. The fall of big physics – in retrospect possible to ascribe ultimately to the end of the Cold War and symbolized by the cancelling of the Superconducting Super Collider – meant more than the discontinuation of a seemingly ever-

increasing governmental commitment to fundamental physics research on three continents. It was a kind of final confirmation that governmental support for science needed other rationales than prestige. Even though money is still spent on basic science with little or no prospects of economic or societal benefit, such commitments are weighed against others in a competition more balanced than the one out of which sites for new accelerator complexes were chosen in the 50s and 60s.

Though seemingly apt for the dynamics of modern science and apparently created on basis of the new political logic, synchrotron radiation laboratories did first emerge while big physics still had its hegemony and progress in terms of increases of accelerator energy sufficed for several hundred million dollar commitments. As noted, synchrotron radiation is tied to high energy physics by the simple fact that they originally utilized the same machines – and still do, in the intuitive view of most people, including some policymakers. However, one interesting conclusion to draw from the fact that synchrotron radiation during its first decades of existence (with few exceptions) was a parasite on high energy physics machines is that there simply was no other option. It is reasonable to suggest that synchrotron radiation would not have been given opportunity for necessary exploratory work had high energy physics accelerators not been around already, in great numbers. History shows – and it has been carefully pointed out in the previous chapters – that the developments in high energy physics during the 60s and 70s was crucial for the emergence and evolution of synchrotron radiation. On a purely technical level, functioning storage rings – which were taken into operation for high energy physics in the late 60s and early 70s – was a prerequisite for the possibility to obtain a beam of radiation stable enough to do meaningful experimental work. On a political and sociological level, the enormous increase in size of the next generation high energy physics machines in the 70s – coined the transition from big science to ‘megascience’ – had the results that resources were drained from several smaller and fully functional accelerators that were subsequently taken over by some of the first synchrotron radiation programs. And where deserted accelerators did not suffice for use as radiation sources, accelerator physicists left behind could be employed to design and build new ones, for the new purpose.

Big physics entailed a comparably simple political logic – for every next step in the development of high energy physics, a new machine had to be built (or in some cases an existing machine significantly upgraded). The community formulated a goal, and politics responded. Included as matter of fact in the deliberations was that a significant advance in the field of high energy physics required an investment of several hundreds of million dollars.

The issues surrounding the politics of synchrotron radiation laboratories bear traces of these past times. Though multidisciplinary and substitutable and thereby very well suited for the dynamics of modern science and new political and societal conditions – as described and analyzed above – synchrotron radiation laboratories still operates particle accelerators, and particle accelerators are still pieces of large-scale scientific infrastructure. Interestingly, this is part of the contextual explanation for the basic status and handling of synchrotron radiation laboratories in political realms, in other words also the instances where ultimate decisions are made to commit to the construction and operation of synchrotron radiation laboratories. As emphasized in previous chapters, such decisions are always coupled with priorities, also if the basis for making priorities is not entirely solid.

Politics' dilemma

A fact as simple as it is crucially important constitutes the fundament for the science-politics relationship: control over funding is in the hands of politicians and the political spheres. The statement should not be mistaken for a claim that the momentary and short-term negotiations and agreements that signify politics is what ultimately rules and governs science. It is quite the contrary. As shown in the case study chapters, it is the broad and inert institutional frameworks established by politics on basis of long-term agendas and deliberations that set the ground rules. Within these ground rules, however, there is room for a great deal of maneuvering.

The synchrotron radiation program at SLAC exemplifies this. A well-established and strong institutional framework *and* the ability to partly alter the institutions, proved essential for the emergence and establishment of synchrotron radiation in the United States. The National Laboratory system had built-in mechanisms both on laboratory management and political level to absorb and cultivate (or in some cases just *allow for*) new initiatives. Such an initiative, when equipped with a scientific 'case' strong enough, could eventually alter laboratory structures and deploy them for its own purposes, thereby exploiting the distinctive combination of institutional autonomy and powerful funding and mission from the government that signifies the National Laboratory system.

The ESRF came into being by a process with an important political dimension. First, the complicated but comparably successful CERN venture had showed that Europe was capable of collaboration but that the intergov-

environmental agreements required was an intricate issue. Second, the revived European collaborative spirit around the Franco-German entente and the efforts to reestablish Europe scientifically and technologically by collaboration during the 'Davignon period' provided the crucial political breeding ground. In the 'crystallization' process leading to the final site selection and resolving of the funding question, as well as in the codified framework for operating the facility, it was shown that politics is an overriding force in matters of large scale – national interests, prestige and sovereignty trumps purely scientific motives.

With respect to politics and the significance of politically induced systems and institutional frameworks for science to maneuver in, the Swedish case, MAX-lab, most of all shows that the system need not at all be optimized through deliberate measures on behalf of politics. MAX-lab came into being and developed itself into present day size and status through the piecemeal adding of small financial contributions, with no overarching decision taken at any point in time for a comprehensive commitment to synchrotron radiation on behalf of the Swedish government. This means that the existence of MAX-lab rather can be attributed to the abilities of actors involved (scientific entrepreneurs) to pragmatically make their way through a system with clear shortcomings. But it also shows that a seemingly suboptimal political system need not preclude other conditions and mechanisms for the establishment of a high-performing laboratory with a vivid experimental program, only that it will perhaps require more inventiveness and pragmatism. It also shows that a system with certain characteristics – such as a traditional lack of mechanisms for strategic decisions on large scale – may not be easily changed even if an initiative proves to be strong and successful enough to perhaps 'deserve' or 'motivate' extraordinary action on part of politics.

What these collected conclusions from the case studies amounts to is a confirmation that the political forces at play in the governance of synchrotron radiation laboratories is exactly as top-down, long-term, inert and foremost *enabling* as described in previous sections. But they also open up for an interpretation of this top-down political governance of synchrotron radiation laboratories as a telling example of a more general feature of science policy. The 'information asymmetry' problem, discussed in chapter 1, is a seemingly unavoidable feature of the political governance of science. When large governmental 'lump sum' investments in science are coupled with increased demands for accountability and tangible results, the information asymmetry seems to become particularly pressing. The 'more is never enough' tendency – that science will always expand to absorb as much money as made available for it – complicates matters further. There is no price tag to be put on a

country's activities in synchrotron radiation against which performance can be weighed and a commitment can be evaluated, not even when there is a four-wall laboratory in place to perform it and symbolize it. The laboratory is not only multipurpose and contingent and its activities multidisciplinary and for the most part transitory – it is also almost impossible to comprehend in terms of return for investment. The question how much money a given country invests in synchrotron radiation related research in a given year is just as impossible to answer in concrete terms as the question what exactly the benefit of synchrotron radiation related research is for this given country. Matters are complicated further by the risk that these laboratories are viewed as old time 'big science' facilities, i.e. unitary and single-purpose and with performance possible to measure – in *giga electron volts* (GeV) – not least against the GeV achieved by a rival superpower.

The ESRF is perhaps the most vivid example, since its political genesis was so delicate, and it is shown by the distance or gap between politics and science at the ESRF, noted in chapter 6. It does not imply that productivity is not high or that success is not achieved, only that productivity and success is achieved through means not in politics' control. The only control politics can exert is the funding stream, and this stream is kept flowing on basis of (claimed) macro-level achievements. The comprehensive funding profile has had important part in *enabling* the relative success of the ESRF – and this is coherent with the argument here that politics *enables* but cannot *manage* and cannot *appraise*.

Thus the priorities determining governmental funding of science are made on basis of something else than considered opinions of the usefulness or return for investment science provides. Synchrotron radiation laboratories show this in a tangible way because they are (with few exceptions) the results of large governmental 'lump sum' investments and simultaneously in their essence incomprehensible. Traces of linear model and Cold War 'energy race' thinking are detectable in the politics of synchrotron radiation laboratories.

But the above discussion about synchrotron radiation laboratories' correspondence with trends of the changing dynamics of science – collectivization and sophistication – obviously has connections to science policy, and in another perspective, synchrotron radiation laboratories seem comparably well suited for a science and science policy climate of accountability and demands for tangible returns. As 'research hotels' they are multidisciplinary and transient, and oriented towards several of the scientific areas commonly identified as 'strategically important' to society. Although the full range of activities at a synchrotron radiation laboratory can perhaps not be comprehended, the multipurpose and contingency character of the laboratories makes them re-

sponsive to societal and political trends and demands, which increases transparency on some level. The potential of synchrotron radiation laboratories to answer to developments in science, science policy or society at large is shown by the growth of life sciences applications of synchrotron radiation and the unmistakable trend of the laboratories to orient their activities towards such applications, clearly corresponding to the generally elevated status of life sciences of the past few decades. Materials science, nanotechnology, and similar 'strategic' areas of science have also entered synchrotron radiation laboratories as they have emerged, or developed in great part at synchrotron radiation laboratories, out of disciplines like solid state physics. The institutionalization of synchrotron radiation in science appears to have been joined by continuously improvements of its political attractiveness, almost inseparably.

Generic instruments institutionalized

The evolutionary and dynamic character of synchrotron radiation laboratories have been extensively described and analyzed throughout this thesis, in case-specific examples and in more general observations. It has been noted that synchrotron radiation laboratories do not constitute an anomalous or completely novel entity in science but rather is a kind of agglomeration of a broad range of 'ordinary' science in a new institutional construct, adapted and adjusted to the utilization of large scale infrastructure. The bottom-up, evolutionary, and transient governance force that has been presented as a counterweight to top-down politics as described above is science itself and the influence of ordinary science on the laboratories. The 'local technological imperative' has purely scientific (or in some cases perhaps purely technological) origins. It has been analyzed along with institutional mechanisms that facilitate it – the workshop approach and the organizational structures at laboratories that are put in place to channel and make use of talent and competence within the user community. These mechanisms can be conceptually identified as the means by which the encounter of bottom-up science and top-down politics *creates* synchrotron radiation laboratories. Dualities within the lab, corresponding to this fundamental duality of its governance, have also been identified and discussed: for example the machine and experiments divide in an earlier section.

Synchrotron radiation laboratories are situated in the nexus of these two strong forces. Multifaceted bottom-up science is capable of adaptation and adjustment of priorities according to the needs of science, science policy, and

the surrounding society. The large scale scientific infrastructure provided by the investment and commitment by the government is what *enables* both the activities and their ability to change.

After the identification of these two *forces*, two interpretations of the *laboratory* are available. One is that it is *created* by the constraints and opportunities given by the influence of the scientific community (bottom-up and momentarily), the science policy system (top-down and long-term), and the overall demands and opportunities presented to science and politics by the surrounding society. The other one is that the laboratory – or rather the *actors* of importance for creating, maintaining and ‘operating’ it – instead *enrolls* parts of the scientific community, the science policy system, and the surrounding society in their *alliances* to promote a scientific project, field, instrument, career, or laboratory. They are *scientific entrepreneurs*.

A central feature of synchrotron radiation laboratories is their manifestation of the *experiment* as the chief sociological (and epistemic) entity in science. As established in the introductory theory chapter, *science* and *technology* are inseparable and apparently increasingly so with the sophistication trend. The suggestion is then that the traditional separation between the professional identities associated with science and technology – scientists and engineers – is becoming partially obsolete when the *experiment*, tightly linked with the *instrument*, becomes the prime focus of scientific development. Less surprising is of course the indication that this is explicit in ‘hi-tech’ laboratory environments like synchrotron radiation laboratories.

The theoretical framing in chapter 1 of the *experiment* and associated *instrument* as the prime sociological (and epistemic) entity in science entails a description of *experimental systems* – an attempt to describe the experimental setup and its ordered social context, such as a specific instrument in a specific laboratory environment, as a whole. This whole system is chosen, designed, put to use, optimized and refined by an experimenter, a scientist – and the overriding purpose is to *make an experiment work*. As such, the system becomes a resource for the scientist to utilize. Exclusive and expensive instruments and infrastructure, coupled with the collected additional resources of the laboratories including for example technical support, can be viewed as experimental systems that are developed and optimized for certain scientific purposes. And as shown, the system can be optimized for several purposes simultaneously.

Experimental systems have a *unifying* force: In a science disunified with respect to theory, method, organization and institutions, experimental systems are unifying – they assemble technological and social resources and make them work collectively at physical locations. In addition, experimental

systems are developed to transcend the boundaries of the disunified sciences and act as temporary unifiers. Such temporary unification is suitable for a science in a fast turnover and return-driven mode of organization.

While generic and applicable to a host of scientific work perhaps not even included in the original intentions of its 'inventor', the systems are at the same time static in that they are located at certain places, operated by certain people, and provided only in competition. Thereby a connection is established between the unifying ability of the systems and the political force that put them in place. The concept of experimental systems and its contribution to the understanding of instruments, experimental science, and laboratory activity thus becomes applicable to synchrotron radiation laboratories and their activities on several levels. The whole laboratories, the beamlines and experimental stations, and projects with momentary experimental setups are all experimental systems. Similarly, actors – scientific entrepreneurs – can be identified in each instance.

These actors, associated with the experimental systems, can bridge islands of disunified science by facilitating the use of the systems on new areas. They can thereby unify disciplinary, institutional, and sociologically disunified sciences in demarcated projects or collaborations, in which pieces of technology are at the center and the social ordering around them create favorable conditions to make an experiment *work*. As noted, the expansion of synchrotron radiation to cover new areas of utility – an important part of the institutionalization of synchrotron radiation in science – can be interpreted as synchrotron radiation being a *generic technology* moving and developing between and within different new and renewed areas of application. Individual techniques and areas of application are possible to interpret similarly. In both cases, there are clearly distinguishable actors. The early pioneers convincing high energy and accelerator physicists to give them access to the radiation, the first proponents of areas of utility that formed the early PRTs, the enthusiasts constructing storage rings at universities with piecemeal funding and little overall strategy, and later the synchrotron radiation proselytizers who took part in the targeted efforts to convince biologists and chemists and other reluctant representatives of the sciences to give the accelerator-produced x-rays a try – all of them are scientific entrepreneurs. Their role in the institutionalization of synchrotron radiation should be analyzed with care, so as not to surrender to mystified tales of scientific heroism, but it should nonetheless be acknowledged as important. The 'x-ray jocks', to which significant attention was given in a previous section, are the perhaps most clearly visible scientific entrepreneurs of synchrotron radiation (but not the only ones). In the modern and institutionalized laboratories, x-ray jocks have a clear mediating role between the technological capabilities of laboratories and specific instru-

ments and the scientific ambitions of users. They promote specific areas and projects by establishing alliances around them, enrolling users and laboratory resources in the strengthening of their case in a competitive and constantly changing science enterprise.

Small science on big machines

The new societal and political context for science, with its focus on returns, productivity, strategic choice and accountability, is distinctly different from the era of big physics as described in chapter 2. When it changed, the 'carte blanche' from the superpower governments was withdrawn and big physics could not retain its hegemony. This historic development is important both for the understanding of the emergence of synchrotron radiation and the analysis of it in broader scientific and science policy terms.

Big science did not – as dystopian commentators in the 60s feared – swallow or outmaneuver small science completely. In rich varieties, small science prevailed and evolved, largely in the shadow of big physics, and developed its contents and practices. The political shifts and the changes in science's societal context eventually gave small science a kind of reawakening. The changing dynamics of science, toward shorter term collaborative project work in teams, the general shift towards results orientation and productivity, and the pressure on science to interface with societal needs on swifter terms, seems now to have reinstated small science as a model sociological structure of science. However, as authors have argued and as discussed above, sophistication, collectivization and increased competition has also made more and more branches of science dependent on centralized organizational constructs and technically advanced infrastructure.

From this overall perspective, the establishment and institutionalization of synchrotron radiation in science is a sign and a part of a general trend of advanced, large and exclusive scientific infrastructure and the laboratory organizations hosting it transforming from single-mission to multi-purpose. High energy physics laboratories and synchrotron radiation laboratories are telling examples since they utilize the same basic pieces of infrastructure. Such a change is thus both a result of general changes in the dynamics of science and a contributing factor to these changes. A broadening of the 'constituency' of synchrotron radiation laboratories, i.e. the range of concerned disciplines, brings on far-reaching changes to these disciplines. Within the laboratory institutions experiencing these changes, it is obvious that the transition is not entirely smooth and unproblematic, which is particularly shown

at SLAC (chapter 4). For a laboratory like SLAC, it is about changing *identities* and giving up a previously majestic position as the spearhead of science's penetration into the unknown. A reasonable suggestion is that this development – and the symbolic fact that synchrotron radiation as large scale scientific infrastructure arguably well suited for the modern scientific realities took over accelerator complexes – points at the peculiarity of high energy physics and its extraordinary position in science and society during the second half of the twentieth century. The extreme collectivization represented by these 'megascience' high energy physics laboratories was in such an interpretation not the ultimate version of a general collectivization pattern but rather a premature collectivization anomaly:

"Big Science interpreted thus becomes an uncomfortably brief interlude between the traditional centuries of Little Science and the impending period following transition. If we expect to discourse in scientific style about science, and to plan accordingly, we shall have to call this approaching period New Science, or Stable Saturation; if we have no such hopes, we must call it senility" (Price 1986/1963, p 29).

If post-academic science or science in the dynamic steady state is this 'New Science' or 'Stable Saturation', then the analysis of science's changing dynamics along the lines of collectivization and sophistication should be inserted into a framework of big science and little science and the institutional varieties of the two. Synchrotron radiation laboratories then appear as particularly interesting *and important* entities in science – acting out collectivization and sophistication on 'ordinary' small science, enabling scientific disciplines to find new ways of utilizing synchrotron radiation in their own areas, and adjusting sciences to the generic instruments and the generic instruments to the sciences. Synchrotron radiation laboratories are simultaneously manifestations of and contributing factors for sophistication and collectivization. What could reasonably be more attractive – or *necessitated* – in the dynamic steady state than a multipurpose laboratory, where exhaustive attempts are made to satisfy broad range of users?

There are of course political catches, attributable not least to the information asymmetry and the 'more is never enough' tendency as they appear in the material. They have contributed to the positioning of synchrotron radiation laboratories in the nexus of the two strong governance forces of bottom-up science and top-down politics. Distance between these forces and limitations of their understanding of each other's motives and mechanisms present laboratories with challenges and obstacles, sometimes resolved with

inventiveness, sometimes with pragmatism, and sometimes with expanded budgets. However, it is clear that synchrotron radiation laboratories, by these and other characteristics, represent and manifest features and trends in the broader scientific landscape and the broader science policy context. Regardless of whether ‘big science (on big machines)’ was an anomaly, “an uncomfortably brief interlude” as Price (op. cit.) noted, it belongs to the past. Both in politics and in practices, the institutionalization of synchrotron radiation laboratories meant the emergence and establishment of a novel way of combining demands and resources that is unmistakably small science on big machines.

Appendix

List of synchrotron radiation user facilities around the world

- Advanced Light Source (ALS), Lawrence Berkeley National Laboratory, Berkeley, California, USA. In operation since 1993. Approximately 35 beamlines. (www.als.lbl.gov)
- Advanced Photon Source, Argonne National Laboratory, Argonne, Illinois, USA. In operation since 1996. 50 beamlines. (www.aps.anl.gov)
- ALBA Synchrotron Light Facility, Barcelona, Spain. Under construction, planned start of operation 2010. Seven planned beamlines. (www.cells.es)
- Angstromquelle Karlsruhe (ANKA), Forschungszentrum Karlsruhe, Germany. In operation since 2003. 13 beamlines, four under construction. (ankaweb.fzk.de)
- Australian Synchrotron (AS), Melbourne, Australia. In operation since 2006. Five operating beamlines, an additional five under construction. (www.synchrotron.org.au)
- Beijing Synchrotron Radiation Facility (BSRF), Institute of High Energy Physics, Beijing, People's Republic of China. In operation since 1991. Nine beamlines. (www.ihep.ac.cn/bsrf)
- Berliner Elektronenspeicherring-Gesellschaft für Synchrotronstrahlung (BESSY), Berlin, Germany. In operation since 1982. Replacement of the accelerator in 1998. 50 beamlines. (www.bessy.de)
- Canadian Light Source (CLS), Saskatoon, Saskatchewan, Canada. In operation since 2004. Five operational beamlines, another six under construction. (www.lightsource.ca)
- Center for the Advancement of Natural Discoveries using Light Emission (CANDLE), Yerevan, Armenia. Under construction. (www.candle.am)
- Cornell High Energy Synchrotron Source (CHESS), Cornell University, Ithaca, New York, USA. In operation since 1980. Six beamlines. (www.chess.cornell.edu)
- DAFNE Light, Laboratori Nazionali di Frascati (LNF), Frascati, Italy. In operation since 2001. Three beamlines. Parasitic activity at another ring started in the 70s. (www.lnf.infn.it)
- Diamond Light Source, Oxfordshire, United Kingdom. In operation since 2007. Replaced the Synchrotron Radiation Source (SRS) in Daresbury. 13

- beamlines, an additional 13 planned. (www.diamond.ac.uk)
- Dubna ELectron SYnchrotron (DELSY), Joint Institute for Nuclear Research, Dubna, Russian Federation. Under construction. (wwwinfo.jinr.ru/delsy)
- ELETTRA Synchrotron Light Laboratory, Trieste, Italy. In operation since 1993. 25 beamlines. (www.elettra.trieste.it)
- European Synchrotron Radiation Facility (ESRF), Grenoble, France. In operation since 1994. 40 beamlines. (www.esrf.fr)
- Hamburger Synchrotronstrahlungslabor (HASYLAB), Deutsches Elektronen-Synkrotron (DESY), Hamburg, Germany. In operation since 1963. 20 beamlines. New laboratory (PETRAIII) under construction. (hasylab.desy.de)
- Hiroshima Synchrotron Radiation Center (HSRC), Hiroshima, Japan. In operation since 2002. 13 beamlines. (www.hsrf.hiroshima-u.ac.jp)
- INDUS, Raja Ramanna Centre for Advanced Technology, Indore, India. In operation since 1999. Six beamlines. Second ring under construction. (www.cat.ernet.in/technology/accel/indus)
- Institute for Storage Ring Facilities (ISA), Aarhus, Denmark. Five beamlines. (www.isa.au.dk)
- Laboratorio Nacional de Luz Sincrotron, (LNLS), Campinas, São Paulo, Brazil. In operation since 1997. Ten beamlines. (www.lnls.br)
- MAX-lab, Lund, Sweden. In operation since 1987. Three rings with in total 20 beamlines. (www.maxlab.lu.se)
- National Synchrotron Light Source (NSLS), Brookhaven National Laboratory, Upton, New York. In operation since 1982. Two rings with 12 and 29 beamlines respectively. New ring planned. (www.nsls.bnl.gov)
- National Synchrotron Radiation Laboratory (NSRL), Hefei, People's Republic of China. In operation since 1991. 13 beamlines. (www.nsrl.ustc.edu.cn)
- National Synchrotron Radiation Research Center (NSRRC), Taiwan. In operation since 1994. 20 beamlines. (www.nsrrc.org.tw)
- Photon Factory (PF), High Energy Accelerator Research Organization (KEK), Tsukuba, Japan. In operation since 1982. 20 beamlines. (pfwww.kek.jp)
- Pohang Accelerator Laboratory (PAL), Pohang, Korea. In operation since 1995. 20 beamlines. (paleng.postech.ac.kr)
- Saga Light Source (SAGA-LS), Kyushu Synchrotron Light Research Center, Tosu, Japan. (www.saga-ls.jp)
- Shanghai Synchrotron Radiation Facility (SSRF), People's Republic of China. Under construction, planned start of operation 2009. 20 planned beamlines. (ssrf.sinap.ac.cn)

- Siam Photon Laboratory (SPL), National Synchrotron Research Center (NSRC), Suranaree, Thailand. In operation since 2007. Three operational beamlines and three under construction. (www.slri.or.th)
- Siberian Synchrotron Research Center (SSRC), Budker Institute of Nuclear Physics, Novosibirsk, Russian Federation. In operation since 1972. Ten beamlines. (ssrc.inp.nsk.su)
- Singapore Synchrotron Light Source (SSLS) In operation since 2001. Four beamlines. (ssls.nus.edu.sg)
- Stanford Synchrotron Radiation Lightsource (SSRL), SLAC National Accelerator Laboratory, Menlo Park, California. In operation since 1974. Recent major upgrade. (www-ssrl.slac.stanford.edu)
- Super Photon Ring 8 GeV (SPRING-8), Hyogo, Japan. In operation since 1997. 40 beamlines. (www.spring8.or.jp)
- SuperSOR Synchrotron Radiation Facility, University of Tokyo, Japan. (www.issp.u-tokyo.ac.jp/labs/sor)
- Swiss Light Source (SLS), Paul Scherrer Institut (PSI), Villigen, Switzerland. In operation since 2001. 15 beamlines. (sls.web.psi.ch)
- Synchrotron Radiation Center (SRC), Madison, Wisconsin. In operation since 1968. New accelerator replaced the original one in 1982. 15 beamlines. (www.src.wisc.edu)
- Synchrotron SOLEIL, Paris, France. In operation since 2006. Replacing the synchrotron radiation laboratory in Orsay. Approximately 20 beamlines in operation or under construction. (www.synchrotron-soleil.fr)
- Synchrotron Ultraviolet Radiation Facility (SURF), National Institute of Standards and Technology (NIST). Gaithersburg, Maryland. In operation since 1962. (physics.nist.gov/MajResFac/surf/surf)
- Synchrotron-light for Experimental Science and Applications in the Middle East (SESAME), Allaan, Jordan. Under construction. (www.sesame.org.jo)

(Information from www.lightsources.org and the listed websites, viewed January 12, 2009. Note that laboratory websites may contain outdated or incomplete information and that the intention of listing the laboratories is only to give a rough picture, not exact and/or comparable data.)

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