Big Science Transformed

Science, Politics and Organization in Europe and the United States

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Contents

List of Figures

List of Tables

1

Introduction and Framework

Starting Points

This book uses a variety of perspectives, conceptual tools and empirical cases to argue that Big Science in North America and (Western) Europe has transformed dramatically and, by most accounts, beyond recognition.[1] Promoting a new understanding and a partly new use of the slightly worn-out and arguably very vague and analytically unworkable term "Big Science," the book argues that the basic structures of Big Science (big machines, big organizations, and big politics) have remained in place but that the content of the research activities that nowadays constitute Big Science are radically different from some decades ago. Likewise—and importantly—the political and organizational forms for Big Science have changed profoundly. There is thus both continuity and change in

[1] This argument is found in the works of many scholars, although it is not always as articulated, and not as comprehensively laid out and structured as in this book. It was the theme of a workshop in Lund, Sweden, on January 16–17, 2014, under the headline "The New Big Science," organized by Thomas Kaiserfeld and the author. Since then, the term and concept "New Big Science" has taken on a life of its own, as a label for all kinds of seminar and workshop activities more or less connected to the original issue. Thus, while the particular term "New Big Science" is rather catchy, and the undersigned is one of its original authors, for this book and its title, it has been deliberately deselected in favor of "transformed Big Science."

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Big Science, and while the central term is ambiguous, it can be relied upon as an independent variable in the conceptualization of the topic and the building of a framework for the analysis: although many of the preconditions for its original existence are long gone, Big Science has not vanished, but has transformed.

Thomas Kuhn (1959) identified an "essential tension" between innovation and conventionality in science, which is a workable starting point for much conceptualization of publicly funded and organized science as an institution, social system, profession, or organized social activity, as shown by Whitley (2000/1984) and Ziman (1987), among others. A scientific knowledge claim must be new in order to be meaningful, but conventional in order to make sense. If it does not in any part describe something previously unknown, it does not advance knowledge, and if it does not in crucial ways connect to existing knowledge and adhere to certain institutionalized procedures and norms with respect to form and presentation, it cannot be comprehensible, relevant, taken seriously, and integrated into the scientific commons. This fundamental guiding principle for science as a human activity also extends to its organization (in a broad sense, and understood as a verb): The vocational activities of individual scientists and the aggregation of these activities to organizations, institutions, and systems, as well as to assemblages of facts and claims, and to physical infrastructures, are crucially devoted to change and fundamentally anchored in continuity. This includes Big Science, and it is the basic theoretical realization that enables the conceptualization of a transformed Big Science, as well as the documentation and analysis of its transformation processes.

But continuity and change are also topical themes in the broader empirical and theoretical study of science in society, science policy, science governance, and science organization. A dominating discursive theme in such study, currently and at least two decades back, is an alleged change of science and its interface with society, and the flood of conceptualizations and empirical observations concerning this change is overwhelming. Whether it regards a changing Social Contract for Science (e.g. Elzinga 1997; Vavakova 1998; Hessels et al. 2009), the influence of corporate managerial practices on the governance of universities (e.g. Berman 2014; Deem et al. 2007; Ginsberg 2011), the changing nature of the valuation of scientific knowledge in society (e.g. Radder 2010; Carrier and Nordmann 2011; Mirowski and Sent 2002), or the alleged poststructuralist emancipation from delusional modernist beliefs in scientific truths by radical change in cultural discourse (e.g. Latour 1993; Bloor 1976; Collins 1981), there seems to be consensus that science has changed or is changing beyond recognition—but only partially beyond recognition. Some essential features remain and guarantee continuity, which also shows by the ubiquitous use of prefixes in the flood of conceptualizations of current science: postacademic science (Ziman 1994), post-normal science (Funtowicz and Ravetz 1993), strategic science (Irvine and Martin 1984), finalized science (Böhme et al. 1973), mode 2 science (Gibbons et al. 1994) (for an overview, see Hessels and van Lente 2008)—all of them signal the change of some (key) features while a core of some kind remains intact.

A very similar message is at the core of this book and its description of a profound transformation of a well-known (yet perhaps conceptually elusive) phenomenon of science and technology of the second half of the twentieth century. Thereby, it is acknowledged that there has been change to what Big Science is, compared to when it emerged and first grew to prominence, and this book makes a case for such an interpretation of a slice of recent history of science, with a crucial conceptual awareness that draws from an eclectic and pragmatic definition of the work as sociological study of science policy and organization. But, importantly, there is also continuity, and an ultimate aim of the book is to contrast continuity and change so as to conceptualize a transformed Big Science not as something entirely new and discontinuous but as something partly new and partly built out of existing elements and within existing institutional frameworks. The basic relevance is secured by showing that there is Big Science with some important new features (see next section), and a ubiquity of studies of (old) Big Science or studies where the concept Big Science is used with a far too broad or careless definition (see the section after that), from which it follows that more work is needed to conceptualize and empirically investigate what has and has not changed in Big Science, and how Big Science can be characterized and defined in various contexts, for various purposes. The book describes various aspects of how Big Science has transformed, into what, and why. It goes deeper in understanding this, empirically and theoretically, than previous work on the same topic has done. By combining the compilation of previously published articles and an extended review of secondary sources with some additional original empirical work and not least a new synthesis, the book aims to cover as many aspects of the phenomenon as possible, and present the reader with a coherent set of empirical observations, theoretical insights, and arguments that advance the sociological and historical study of Big Science. The definition of Big Science used in the book is tailored to its purposes, and the details are found later in this introductory chapter, which also draws up a framework for the whole book and provides the basic tools for the analysis.

The Kuhnian "essential tension" and its organizational incarnation in the dichotomy of continuity and change provides a useful starting point also for identifying and discussing the origins of Big Science as we know it. Regardless of how exactly it is defined, Big Science is part of a broader science system or part of the "institution of science," as Merton (1938, 1942, 1957) quite persuasively called it. The use of very big instrumentation or the organization of science projects in large teams is, from one viewpoint, a new technical and/or organizational approach to scientific work that itself has a very long tradition. The evolution of scientific disciplines over the centuries is not within the aims of this introduction or this book to lay out, explain or analyze, but Big Science places itself in this evolution as a recent variety of scientific method or organization to advance human knowledge by the use of systematic and socially structured inquiry, with a long institutional tradition. In other words, Big Science is a recent branch of activities in the natural sciences that happen to demand the use a specific type of very large and complex instrumentation and/or very large and complex organizational arrangements to maintain its progress in the accumulation of knowledge. In short—and this conceptualization will be returned to in greater detail below—Big Science is understood as science made big in three dimensions: big machines, big organizations, and big politics. But the transformed Big Science also constitutes a change in the way (some) scientists use instrumentation, with a division of labor between operation and use of instrumentation on a new scale and with new organizational (and political) features. As such, it has spread through the disciplines of the natural sciences at a pace that suggests it is not a marginal phenomenon, but central in the current international science system, and in congruence with several other major shifts (see below).

What Has Changed?

Big Science in the original version was a Cold War phenomenon. It was born out of the highly specific (geo)political and scientific-technological conditions of the post-World War II era, when the superpower competition on global scale and the associated race to technological superiority (also outside the realm of nuclear weaponry) clearly dominated most policy areas and political life, including publicly/governmentally funded science of which Big Science is a part. Hence, the old Big Science had a clear military connection, as well as a politics that was oriented to the bipolar geopolitical world order. This military connection is discussed at some length in Chap. 2, where also the Cold War era and the post-Cold War era are differentiated, and the old and transformed versions of Big Science are connected to these historical periods. It deserves, though, to be mentioned already at this point that this post-Cold War era of science and science policy did not, in a strict sense, start in the years 1989–1991 when the Cold War officially ended (in political terms), although the radical tilt of the global geopolitical balance in those years certainly affected the form and function of Big Science. Rather, science and science policy transformed gradually during the whole post-World War II period, and the transformed version of Big Science emerged long before the end of the Cold War, as did the new politics that sustained it, and thus the post-Cold War era is a flexible concept that refers to a time period with a fuzzy starting point and no end point (yet). Nonetheless, it has conceptual relevance because it can be contrasted against the very specific political, scientific/technological, and societal order of the Cold War, and in this contrast, several important insights are revealed.

The old Big Science, as a Cold War phenomenon, was most of all the use of large machines—reactors and accelerators—for subatomic physics research with some connection to nuclear energy and weaponry (e.g. Hiltzik 2015; Stevens 2003). In addition, huge telescopes for ground-based astronomy (e.g. McCray 2006) and various space programs (e.g. Smith 1989) were launched under the same auspices (the military connection) and contributed to a similar perpetuation of images of scientific and technological superiority vis-à-vis the other superpower as well as the capabilities of smaller yet important countries. Very soon, however, the military connection was in practice lost, not least because the military (classified) and civilian (open) research and development (R&D) activities on nuclear energy and related technologies became institutionally separated (Hewlett and Holl 1989). Civilian Big Science became all about seeking answers to the most fundamental questions regarding the structure of matter on subatomic level and the origins of the universe, which was done by the help of increasingly larger accelerator complexes for particle physics, where elementary particles were smashed together, and the result of the smash, the particles' smaller constituents (e.g. quarks), observed and documented[2] (Hoddeson et al. 1997). The transformed Big Science is likewise about the use of large machines—predominantly accelerators but to some extent also reactors—but for other purposes and in a whole other setting, including serving a far broader spectrum of scientific disciplines and with a broader and more intense interface with society including innovation for economic growth and the work to meet society's grand challenges. Two techniques dominate: the use of neutrons and x-rays, both produced by particle accelerators (and in the case of neutrons, also reactors) for the study of materials (including biological materials) on atomic, molecular, and nanometer levels. The facilities are called neutron sources or neutron scattering labs, synchrotron radiation labs (or simply synchrotrons), and free electron laser labs or free electron lasers. Neutrons have been used as a probe to study materials since World War II, and their usefulness and feasibility for these purposes have gradually increased since then. In the 1960s, the first purpose-built reactors for neutron scattering[3] were constructed, and in the 1980s, an accelerator-based technique for producing intense beams of neutrons (spallation sources) emerged. Neutrons complement x-rays, which have been used for over a century in various studies of matter, produced by tabletop sources such as those used at hospitals and airports. When in the 1960s it was realized that the x-rays (and ultraviolet, visible,

[2] In Appendix 1, detailed descriptions of the technical and scientific basics of the utilization of reactors and accelerators in the old and transformed Big Science are found.

[3] Neutrons are fired at a sample, scattered off of it, and the patterns of this scattering are measured and documented to gain knowledge about the material—hence the name "neutron scattering" for the technique. Some synchrotron radiation and free electron laser experimentation works in the same way. See Appendix 1.

and infrared light) accidentally produced by accelerators built for particle physics research could be extracted and put to practical use—and that their intensity, and thus usefulness for all kinds of experimental applications, was several orders of magnitude better than what was otherwise available—the organized exploitation of synchrotron radiation began. It has since grown tremendously and spread across the disciplinary spectrum of the natural and technical sciences. Free electron laser is a rather recent refinement of synchrotron radiation that has brought extreme improvements on some parameters, and free electron laser labs exhibit some specific organizational features to distinguish them from neutron scattering and synchrotron radiation facilities.[4] Especially the growth of life sciences applications of neutron scattering, synchrotron radiation and free electron laser has been remarkable in the past two to three decades, but several other fields of science have also benefited enormously from the technical, scientific, and organizational development of these labs (see Chap. 2 and Appendix 1). The users of neutron scattering, synchrotron radiation, and free electron lasers in Europe and the United States are today counted in tens of 1000s.[5]

In addition to the Cold War superpower competition logic and the arms race, the old Big Science relied heavily on the post-World War II science policy regime of elite governance and the promotion of science for its own sake, as a general source of good, combined with the Linear Model of Technological Innovation as the framework for motivating public expenses in broader perspective (Elzinga 2012; Greenberg 1999/1967; Guston 2000). In this respect, the transformed Big Science differs radically from the old: as especially elaborated in Chap. 6, it is not viewed as science in its own right and it is not justified by a Linear Model of Technological Innovation. Quite the contrary, it is science motivated

[4] For a more detailed and comprehensive description of the technologies and science of these labs and their use, see Appendix 1.

[5] Exact counts are difficult to obtain since labs record statistics very differently: some count individual users, others count user visits, yet others count only experiment proposals and scheduled slots of experimental time. For the United States, comprehensive user statistics exist for the six major neutron scattering, synchrotron radiation, and free electron laser facilities at the National Laboratories (see Appendix 2 for a list), and in fiscal year 2014 (October 2013–September 2014), these together served 12,895 users (individual scientists) (DOE User Statistics 2014). There is no reason to assume that the collective European user communities of neutron scattering, synchrotron radiation, and free electron laser are any smaller.

directly by its (assumed) application and embedded in the contemporary science (or innovation) policy regime of (regional) innovation systems and similar doctrines with similar catchphrases.

Publicly funded science must have a strategic dimension in terms of commercial implications (or expectations) in order to mount necessary political support, and Big Science has a symbolism and is physically imposing, which attracts attention and makes it susceptible to expectations and demands of utility. In addition, the transformed Big Science has this strategic connection by default, due to its disciplinary breadth and foci: neutron scattering, synchrotron radiation, and free electron laser have clear connections to areas like drug development, semiconductors, and climate-neutral transportation. While in essence it is fundamental science, publicly funded and with its results predominantly disseminated in scientific journals and incorporated into the scientific commons, there is an evident and tight connection to applications that are considered strategically important. The transformed Big Science is very much science in "Pasteur's Quadrant" (Stokes 1997): it is part of a quest for fundamental understanding, but there are also considerations of usefulness of the results.

Neutron scattering, synchrotron radiation, and free electron laser facilities are used by 1000s of scientists from universities, research institutes and corporations around the world every year as tools for their work in a wide variety of sciences (see Chaps. 3 and 5), with very naturally varying degrees of applicability. Thus, the road is still quite long before these transformed Big Science facilities become the hubs or motors of regional knowledge economies that policymakers wish them to be (see Chap. 6), but in comparison with the old Big Science and its occasional delivery of another quark or two to fill a gap in the Standard Model of Particle Physics (Hoddeson et al. 1997), the transformed Big Science is clearly attuned to strategic, application-oriented science in a wide range of scientific disciplines. The multi- and interdisciplinary character is itself an important feature: whereas the old Big Science was epitomized by single-disciplinary monolithic labs turning mostly inward, to its own disciplinary community, the transformed Big Science labs are essentially contingent and flexible service facilities that provide a set of experimental tools for users that in the normal case only visit the labs temporarily to gather data (Chap. 3), and whose work often has an inter- or transdisciplinary potential that can be built on to achieve new inter- or transdisciplinary combinations (Chap. 5). Not least, the breakthrough of Big Science in areas of the life sciences (Chap. 2) was important in this part of the transformation of Big Science because it tore down an important barrier between physics and the rest of science, typical for the Cold War regime, and invited the perhaps most prestigious and cherished sciences in contemporary society, namely biology and the medical sciences, into the realm of Big Science.

In a 1967 article in *Physics Today*, nuclear physicist and former director general of the European Organization for Nuclear Research (CERN) (1961–66) Victor Weisskopf discussed some epistemological implications of the development of science and Big Science that he had witnessed in his work, launching a rather ingenious distinction between “intensive” and “extensive” sciences. The former aims at the most fundamental questions of nature, “the first principles, the fundamental questions, the basic laws, and this is the only interesting thing; the rest is just application and reapplication of known principles and not of great interest” (Weisskopf 1967: 24). The latter—“extensive” sciences—aim at making “useful” contributions to human knowledge. Usefulness does not necessarily mean practical or technological usefulness, writes Weisskopf, but should rather be taken to mean assisting other sciences in advancing their cause and answering their most pressing questions. Dividing scientific knowledge production in these two categories is rather extreme, admits Weisskopf, but it should be seen in light of the rather extreme growth of Big Science at the time: it was in the 1960s that the old Big Science took its first steps towards “megascience” (see Chap. 2). The distinction between “intensive” and “extensive” sciences is based on the realization that particle physics was one of the fields of science that enjoyed the most generous financial support and cultural prominence in the time when the article was written, and it simultaneously was (and is) the most “intensive” of all, always asking “the last question,” namely: “What are the fundamental particles of matter?” (Weisskopf 1967: 24). To this, the advocates of the “extensive” sciences, among whom Weisskopf cites Alvin Weinberg’s classic (1963) “Criteria for Scientific Choice,” would complain that asking this question and pursuing an answer will lead to an increased decoupling from other branches of science, because they will have little or no use for the discoveries coming out of this research endeavor. There is much to suggest that particle physics underwent such a development in the later third of the twentieth century, and that this also contributed to its eventual (relative) decline in importance and status as well as the redirection of policy and funding priorities to other fields and experimental resources, most of all the materials sciences, life sciences and the accelerator-based labs of the transformed Big Science, but also biomedical research centers and large-scale efforts in the biosciences such as the Human Genome Project, and projects to meet grand challenges, such as climate modeling. Therefore, Weisskopf's conceptualization of "intensive" and "extensive" sciences has great relevance as a basic framework for contrasting old and transformed Big Science and establishing the fundamental principles for how they differ.

Weisskopf (1967: 25) identified the "extensive" natural sciences as sprung out of early twentieth century atomic physics, whose mapping of the electronic structure of elements and similar atomic structures paved the way for chemistry as we know it, as well as both modern or present-day materials science and many of the fields straddling the division between biology and medicine that we today call life sciences. As a fundamental model, Weisskopf's distinction between "intensive" and "extensive" sciences therefore works to separate old and transformed Big Science with reference to their essential purposes: Particle physics is still the crown of "intensive" science, pursuing the inner secrets of matter, now at a somewhat reinvigorated pace after the 2012 discovery of the Higgs Boson at CERN, and it is the epitome of old Big Science (see below). The sciences that utilize neutron scattering, synchrotron radiation, and free electron laser are "extensive" to their cores, not least perhaps taking into consideration the pressure they are constantly put under, to prove their usefulness, productivity, and relevance (see especially Chaps. 2 and 6). They use a highly treasured laboratory resource—neutrons or radiation produced by reactors or accelerators—to study the properties of materials on molecular and atomic levels, with techniques that are specifically adapted to the high quality of the neutron or radiation beams (in terms of intensity and focus, among other things) but that build on long experimental traditions and instrument-development paths like photoelectron spectroscopy, x-ray imaging (similar to hospital x-rays), crystallographic structural determination of macromolecules (e.g. proteins), and several other prominent experimental techniques of the nineteenth and twentieth centuries (Margaritondo 2002; Berggren and Matic 2012; see Chap. 2 and Appendix 1).

In this respect and many others, the differences between old and transformed Big Science are significant, to the extent that it would be an uphill battle to use a square comparison between them as a framework for the analysis in this book. But interestingly, as has been claimed above and which will be returned to (especially in Chaps. 2 and 4), there is also significant continuity in Big Science: The old and transformed Big Science are products of essentially the same institutional and organizational structures of national and international science and science policy systems. Furthermore, the transformed Big Science has succeeded the old Big Science within organizations, under relatively stable scientific leadership, and the same basic physical infrastructure has been used for both. Continuity is partly ensured by the bigness of Big Science, in its organizations—chiefly national laboratories and the like—that have a built-in resilience typical for large organizations (see below), and by the big politics of Big Science which ensure that labs are kept alive and working as long as they manage to renew their missions in accordance with changed demands and expectations from the surrounding society, as expressed in, for example, a changed science policy regime or other strategic priorities of society's R&D efforts. Quite naturally, there is also resilience inherent in big machines, although these succeed each other on another level than do, typically, big organizations.

One way of framing this is of course to say that while problems change, the solutions remain. Much theorizing in the (neo)institutional realm would support this (see below). But there are wider implications; not least, it is partly a mistake to say that one science policy regime has replaced another, because the new science policy regime is also significantly more complex, heterogeneous, and fickle than the old. The bipolar geopolitical world order of the Cold War was surely complicated in diplomatic terms and horrifying in its extension to Mutually Assured Destruction (MAD), but it was comparably simple and predictable, and set the framework and basic terms of reference for all political activity, including that of funding and prioritizing in the area of science policy. Today's postacademic, postnormal, or strategic science connects to a science policy regime that draws on a set of political framework conditions that possibly are not all known in detail, and that have a heterogeneous set of impetuses or grounds: innovation-based economic growth, governance modes inspired by new public management, globalization (including new competition from East Asian countries), sustainability challenges (including not least the threat of anthropogenic climate change), fights against pandemic disease, and the consequences of aging populations for health services, to name a few.

In this reflexive, late modern or postmodern society, a key issue is how to make sure that large science labs contribute to mitigating or solving these and other problems. The Audit Society (Power 1997) is all about making sure that there are indicators and appraisals for everything, so that nothing is left without proper monitoring. Science policy and the governance of (transformed) Big Science are under strong influence of this and similar doctrines (e.g. managerialism, economization, see below) for the conduct of policy and organization of public administration. On the next level, the question is how to accurately ensure that transformed Big Science has managed to contribute. In other words: what indicators can be used to implement the key governance tools of the Audit Society, namely (quantitatively oriented) performance assessment against which costs can be weighed? The bibliometric performance measures that current science policy and funding actors are so obsessed with (Whitley and Gläser 2007; Weingart 2005) are highly inadequate for evaluating the performance or impact of state-of-the-art Big Science labs in relation to the costs they incur, and even if these measures are qualified and nuanced to include indicators for boundary-breaking and urgent research, they are still not good because they continue to produce a highly skewed image: Big Science is insanely expensive in comparison with ordinary (small) science (see Chap. 5). The old Big Science labs delivered a quark or two (possibly also earning directors or leading scientists the Nobel Prize in physics), and perhaps most of all, their performance could easily (and not entirely inadequately) be measured by their size and power, and squarely compared to competitors nearby or on other continents, especially at the other side of the Iron Curtain.

The transformed Big Science labs of today are apparently more attuned to the expectations and needs of current society, but their contribution to fulfilling these expectations and needs is simultaneously almost impossible to measure. Similarly new in the transformed Big Science is the (organizational) relationship between the labs and their scientific use: While big machines, big organizations and big politics are key characteristics also of the transformed Big Science, the actual scientific work conducted at these labs does not differ much from the ordinary (small) science that takes place in universities, institutes, and the like, which means that the teams are generally not bigger, the affiliations of the collaborating scientists are not different, and the fact that they used this piece of very large instrumentation is not always so easy to note in their journal publications (see Chaps. 3 and 5). The transformed Big Science, which is the topic of this book, is essentially small science that uses big machines operated by big organizations, with the help of big politics.

Wide and Narrow Interpretations of "Big Science"

The concept of Big Science, however, has far wider connotations that stem from its historical origins and later use, and that need to be discussed in order for the definitions in this introductory chapter and the rest of the book to make sense. The coining of the term Big Science is generally attributed to Alvin Weinberg (1961, 1967) and/or Derek J. De Solla Price (1986/1963), who had slightly different intentions. Weinberg used the term Big Science to describe what he saw as a worrying development of governmentally sponsored research (primarily in physics) in the United States, namely a growth of the sizes of equipment and teams necessary to make scientific progress, and a growth of the complexity of scientific undertakings, with respect to theory, method, and organization. Weinberg was clearly worried about what happened to science when it grew (too) big in one way or the other, including all science organized in big teams and units and requiring organization charts, administrators and cadres of technicians. He warned that the inevitable bureaucratization of science would overtake classic academic science and eventually extinguish its natural (and crucial) creativity and serendipity (Weinberg 1961: 162).

Price, for his part, used the term Big Science to describe a general growth of science in nearly all aspects. Although he made references to the growth of instrumentation as well as research teams and organizations to extreme sizes, Price did not see these as definitional to Big Science but rather as byproducts of a science otherwise growing in most respects. Unlike Weinberg, therefore, Price neither saw Big Science as a pathological condition (cf. Capshew and Rader 1992: 5), nor as an inevitable consequence of history, but rather as a current "interlude": the exponential growth of science in terms of money, manpower, and publications that is provable (and proven by Price) for the past three centuries will very soon level off, and Big Science is merely the extreme condition that science finds itself in right now, before this saturation (Price 1986/1963: 28–29).

Since this launch of the term in the 1960s, the concept of Big Science has been incorporated into common language and popular culture (Capshew and Rader 1992: 4), and joins a flood of similar popularized uses of the prefix "Big" in front of well-known societal phenomena: Big Business (Fay 1912; Drucker 1947), Big Government (Pusey 1945), Big Democracy (Appleby 1945), Big Cities (Rogers 1971), Big Foundations (Nielsen 1972), and most recently, Big Pharma (Law 2006; Ansell 2013) and Big Data (Cukier and Mayer-Schonberger 2013). These authors share with Weinberg and Price an ambivalence towards the claimed bigness of the societal phenomena they describe and analyze: while essentially manifestations of progress in modern society, these new big things also bring bureaucracy and institutional inertia that might counter or suppress human creativity and liberty, which would be a generally troubling development, but perhaps especially worrisome in science (Weinberg 1961: 162). But the incorporation of the term Big Science among other big things in popularized views of the world has of course significantly diluted its definition as a concept, and thereby also invited a wide community of scholars to squeeze in all kinds of scientific and technological undertakings into the category, including the space programs of the 1980s (Smith 1989; Kay 1994), the large corporate R&D divisions of the 1970s (Hounshell 1992), early twentieth-century mission-oriented and state-controlled research in the Soviet Union (Graham 1992; Kojevnikov 2002), large projects in biology, ecology, and geosciences (such as the International Geophysical Year in 1957–1958 and the International Biological Program in 1964–1974) (Aronova et al. 2010), and also nineteenth-century naturalist explorer missions to Latin America (Knight 1977) and sixteenth-century astronomy (Christianson 2000). Consequently, historian of science Catherine Westfall (2003: 32) has sagely pointed out that some of the elusiveness of the term Big Science stems from the fact that ever since its coining in the 1960s, "scientists have tended to shape the discussion of Big Science along the lines of professional self-interest." The use of the term for one's own purposes is, apparently, up for grabs.

Galison and Hevly (1992) gathered a group of authors with claims to studying Big Science in a book with this name. While the book and its collection of empirically very disparate chapters reveal many important and highly useful empirical insights, it makes little or no progress on conceptually defining Big Science in a way that would allow further development at the intersection of theoretical and empirical work on Big Science as a phenomenon of recent and contemporary science and society. Instead, in conceptual or definitional terms, the book mostly adds to the confusion, which the authors also admit in their afterword: "even after 100s of pages of text, 'big science' itself remains an elusive term" (Hevly 1992: 355). Contemporaneously with Galison and Hevly (1992), Capshew and Rader (1992) published an exhaustive inventory of the uses of Big Science in scholarly work: Big Science is a pathological or natural state of science, produced by historical inevitability (cf. Weinberg, Price, op. cit.); Big Science is the conduct of science with the use of especially big instruments, in especially big ("industrial") organizational arrangements, or in an institutionalized mode tied to contemporary society's need for symbols of progress; or Big Science is especially politically entangled science. Capshew and Rader (1992) provide a highly useful historical contextualization of all these dimensions of the concept of Big Science, but also fall short of contributing to the development of a theoretical model or definition that can guide the study of contemporary Big Science outside the field of history of science and technology, where they are active themselves and where the interest in theory, either as tools for analysis or in its own right, is traditionally limited. Historian of science Peter Galison, who probably has dug deeper than anyone in his quest to understand late twentieth century physics, complains that "as an analytic term, 'big physics' is about as helpful to the historian of science as 'big building' would be to a historian of architecture" (Galison 1997: 553). But he still uses the term, as do many others with him.

There is, in other words, a necessity to achieve functional differentiation in the use of the term and distinguish between, on one hand, the wide and colloquial (but essentially diluted) use of the concept Big Science, and a significantly more narrow use that is practical for analytical purposes and that, importantly, helps in advancing knowledge about contemporary and topical scientific phenomena. This is especially needed in a quest to deepen the understanding about a Big Science that has changed in some key features while remaining intact in others. Such a quest requires clarity and contrast to make sense and to have explanatory value. In the following, therefore, a fundamental generous attitude is retained towards all possible uses of the concept of Big Science, but the use of the concept in this book is simultaneously narrowed down to achieve a reductionist conceptual definition that is instrumental and pragmatic—or, in other words, useful.

Big Organizations, Big Machines, Big Politics

As a first step, in order to achieve stringency and clarity with respect to empirical focus, "science" is here identified as R&D work organized as part of the public sector, in what Whitley (2003: 1016) defined as public science systems (PSSs). This means science predominantly funded from the public purse, done by professionals primarily for scholarly publication, in organizations and embedded by institutional arrangements that set a basic framework that includes funding, priorities, performance evaluation, and reward allocation. In the particular context of Big Science, this demarcation excludes from the analysis those large-scale undertakings that have a scientific component but are motivated by practical application, like infrastructures for research on nuclear energy (including recent fusion energy R&D plants) (Kohlrausch and Trischler 2014: 208–241;

McCray 2010). Further, Big Science is hereby defined as science made big in three dimensions: big organizations, big machines, and big politics. In all three can be deployed the continuity/change dichotomy as an analytical tool for understanding change and homing in on what transformed Big Science is. Importantly, the definition builds on the criterion that in order for something to qualify as (transformed) Big Science and be discussed in this book, it needs to fulfill all three at the same time, while also organized to serve public science, as noted above.

Consider first the notion of big organizations. This can refer to experimental science organized in very large group sizes and long experiment runs, which would be the preferable frame of interpretation regarding Big Science as emerging and developing in the first two to three decades of the Cold War, on both sides of the Iron Curtain as well as in Japan. The key example is of course particle physics but ground-based astronomy and nuclear physics also generally fit the description. The thresholds for what counts as "large" group sizes and "long" experiment runs change with time, since these are essentially relative concepts: groups are "large" and experiment runs are "long" mostly in comparison with contemporary mainstream teams and experiments. Particle physics appears to have fulfilled these criteria for most of its existence: the teams doing particle physics research in the late 1950s were comparably very large then (Heilbron et al. 1981; Kaiser 2004), as were the teams of "megascience" particle physics in the late 1970s and on (Hoddeson et al. 2008) that still are the models in that discipline. But big organizations can, importantly, also refer to the support organizations necessary for conducting research with the help of neutron beams or synchrotron radiation. Although this is essentially "small science on big machines" (Hallonsten 2009), the organizations put in place to supply small science experiments with the treasured experimental resources they need are necessarily big. This is also the major organizational difference between old and transformed Big Science, as conceptualized here (see below): In the old Big Science, the organization of the experiment was big. In transformed Big Science, the organization required for the essentially small-scale experiment to be carried out is what must be big; it is small science done with the aid of big support organizations. It deserves of course to be noted that in a general sense, neither the organizations of old or transformed Big Science are particularly big; on the contrary, they are quite small compared to both major industrial corporations and, in fact, also compared to most universities and other public research organizations.[6] But since big organizations is here not taken to mean only big formal organizations, like universities or corporations, but organizations that are put in place to enable scientific work—and for this purpose simply must be bigger than most similar organizations—the term is still relevant: the operation of a piece of instrumentation for experimental work in science is in the absolute majority of all cases, historically and contemporarily, the work of one or a few people. Evidently, this is not the case neither for old or transformed Big Science, and therefore big organizations account for a crucial one-third of the conceptual definition for the purposes of this book.

Second are big machines. In the understanding of Big Science brought forward here, the physical bigness is key because this is what necessitates big organizations with their parts or divisions physically collocated. As it happens, the chief example will inevitably be particle accelerators, complemented by a minor category of reactors, and while this is seemingly coincidental it is also technologically determined: The renewed use of particle accelerators is key to the transformation of Big Science, and it means that Big Science has a physical continuity although it has changed radically in some most vital aspects. What counts as "big" machines is of course also relative, and it is clear that only squarely comparing accelerator sizes makes "big" almost irrelevant as an indicator: One of the "biggest" accelerators of the first postwar decade, the "Cosmotron" at the Brookhaven National Laboratory (BNL) on Long Island that started operation in 1952, had a circumference of 72 m. 20 Years later, the "Main Ring" at the Fermi National Accelerator Laboratory (Fermilab) in Illinois opened for experimental use with a circumference of 6.3 km, and the Large Hadron Collider (LHC) at CERN where the Higgs boson was discovered in 2012, is no less than 27 km in circumference. All three were purpose-built for particle physics. But the state-of-the art

[6] One of the best equipped Big Science facilities of today in terms of manpower, the European Synchrotron Radiation Facility (ESRF) in Grenoble, France, has a total staff of 567.5 full-time equivalents (ESRF Highlights 2013), which is less than most European universities, and of course only a tiny fraction of the size of the work forces of the world's largest corporations.

synchrotron radiation facilities built today are only a few 100 meters in circumference, and their sequel refinements free electron lasers, as well as the most recent spallation neutron sources, make use of linear accelerators of maximum a few kilometers length. What really counts as "big," as measured in meters, has hence varied widely during the decades and must, just like "big" organizations, be judged in relation to the times. Within the broad category of instruments used in the natural sciences, it is fair to say that both the 1952 Cosmotron and the 2010s synchrotron radiation facilities (similar in size) are significantly bigger than most contemporaries. But the identification of big machines as another crucial one-third of the definition of Big Science in the context of this book also restricts the empirical scope in a very useful way, namely to those Big Science endeavors that are physically bound to single infrastructural sites, thus excluding for example grid computing, genome sequencing or climate modeling. While the collected amount of instrumentation used in such endeavors is probably quite big, it is distributed instead of tied to one physical location, which in particular means that there are important organizational differences between these and the Big Science facilities under study here.

Third, big politics. This is taken to mean that Big Science has a high visibility in politics and attracts some attention, through news stories as well as among policymakers and in governmental documentation. Although the share of funding in national R&D budgets that goes to Big Science is minuscule compared to small science, that is, universities and institutes with smaller-scale science programs, Big Science is science with high visibility and thus with naturally elevated political stakes. Westfall's (2003: 32–33) claim that the term and concept Big Science has been used strategically by scientists and lobbyists in trying to win political support for their projects was mentioned earlier, and is important here as well, because it suggests that there is in fact political weight associated with both the concept Big Science and its physical manifestations. Big Science attracts political interest and seems to carry a symbolism that can give it special treatment in science policy. This was most evident after World War II, when the beliefs and fears in the powers of nuclear physics was enough to get Big Science going on its own arena of superpower competition, and especially so after the 1957 Sputnik Crisis which extended the expense account of the federal US government significantly, also taking Europe with it in a vast expansion of particle physics and other sciences (see Chap. 2). Big Science was also very political in the 1960s and 1970s when social movements directed fierce criticism towards the political and military establishment, of which governmental R&D and Big Science were evidently a part. Towards the end of the Cold War, when the Reagan administration made heavy investments in the Strategic Defense Initiative, the Superconducting Super Collider, and several other projects, Big Science took a place at the center of politics again (Smith 1990; Gaddis 2005/1982). In the post-Cold War era, when investments in Big Science seemingly are made on the basis of an expectation that they will contribute to gained or increased scientific/technological competitiveness (see Chap. 6), the political stakes are likewise high. Mirowski and Sent (2008) have argued that the current state of science policy, in contrast to the Cold War era, is a "globalized privatization regime," where a market logic permeates most if not all levels of science policy (cf. Radder 2010; Berman 2012, 2014; see also below and Chaps. 2, 5 and 6), and this seems true also in Big Science, although some doubt should be voiced over the "privatization": Big Science of the type analyzed in this book is almost exclusively funded from the public purse. It is therefore still fundamentally political, but the politics have changed.

Table 1.1 The threefold conceptualization of Big Science, cross-tabulated with old and transformed

	Big organizations	Big machines	Big politics
Old Big Science	Large teams, long-term experiments	Accelerators for particle collisions, reactors for nuclear research	Military/security applications (or remote connections)
Transformed Big Science	Large support organizations	Accelerators and reactors for neutron scattering, synchrotron radiation, free electron laser	Innovation-based (regional) economic growth, sustainability, grand challenges

Hence, transformed Big Science is conceptualized here as transformed in all three of these dimensions: big organizations, big machines, and big politics. Table 1.1 shows the tentative result of the conceptualization by cross-tabulating these three dimensions of Big Science with old and transformed. Note, particularly, that the table shows only some dominant features of old and transformed Big Science in the three dimensions, and that there are exceptions.

Theory

In order to analyze various features of transformed Big Science, on the basis of Big Science defined as science made big in the above three dimensions (Table 1.1), some analytical tools from social theory are necessary. This book analyzes different features of the topic and uses several different angles, and the analytical tools therefore vary somewhat between the chapters. In some chapters where specific theory is crucially needed, it will be launched and put to use to make detailed analyses. In others, no theory is in this sense needed but the analysis will instead rest upon the general outline provided in this chapter.

The sociological study of science policy and organization is not a homogeneous field, and therefore it has no unquestioned theory tradition. A wide variety of science studies within sociology, economics, management, anthropology/ethnology, political science, and information science have all laid some claim to the label "sociology of science," and all keep their own theory canons, but none of them provides a toolbox useful enough for the analytical ambitions of this book. Instead, the basic conceptual framework is compiled primarily from organization theory, but also social theory more generally as well as political science; useful tools have been sought broadly among those traditions that offer help for the study of organization, both understood as a noun and as a verb. The usefulness of these strands of theory lies mainly in their richness and variety (for overviews, see March 2008: 2–22; Scott 2004) and in their constructive bridging of the eternal (and oftentimes very damaging) conceptual macro–micro divide in the social sciences. In organization studies, it is questionable whether this division was ever a real issue;

the very influential early works of Simon (1957) and March and Simon (1958) preemptively bridged the divide by accounting both for purpose/intentionality/rationality and social/institutional/structural constraints, and crucially, acknowledging the symbiosis between formal and informal structures in any organization. Although most of the schools of organization studies in the second half of the twentieth century seem to have been preoccupied with organizations and their environment, and thus the institutional level (for an overview, see Scott 2004: 4–9), there are many contributions (among the classics as well as more recently) that not only permit a complementary focus on individuals but also make use of it as a contrast to the view of the organization as an actor, and to differentiate organizational behavior and individual behavior (e.g. Barnard 1938; Hirschman 1970; Pfeffer and Salancik 2003/1978; Coleman 1982; March 1988; Weick 1995). The conceptual link between individual agency and collective interests/action is provided by social theorists in the school of sociological rational choice (Coleman 1990; Hechter 1987; Opp 1999), but while these rest ultimately on methodological individualism, their use in this book does not preclude parallel utilization of institutional theory and other tools that are chosen on the basis of their usefulness for the analytical task at hand.

Some may misread this variety and claim that the perspectives are mutually disqualifying because of the slightly unorthodox combination of theoretical macro- and micro-perspectives that many scholars shy away from, because of traditions in social theorizing. This view is not only counterproductive, but also built on an anachronistic (Popperian) understanding of theory as something that should make a set of predictions to be empirically tested and verified/disproved. Such an approach typically leads theorizers to try to build Grand Unified Theories intended to explain everything (or everything judged interesting; residuals can perhaps instead be ignored), and it risks leading their followers to strongly biased views of the topics they study. While devoted adherents to such schools or paradigms have made commendable contributions by focused and undistorted advancement of their perspectives, they have also perpetuated the unfortunate view that loyalty to a theory school or paradigm is more important than constructive and pragmatic use of theory as a tool to explain (social) phenomena and processes, in other words a bias that may even lead to a failure to notice some aspects or elements of the phenomena and processes under study. The alternative course, as Münch (2002: 9–11) has devised, is to make the best possible judgment of the suitability of a particular theoretical (and methodological) approach to a particular purpose and topic of study, and use a variety of different theoretical tools in an attempt to account for as many variations of social reality as possible.

In this book, therefore, theory is most of all used as a toolbox. The choice of tools from this box has been made in the spirit of open-mindedness and pragmatism, in order to find theories and conceptual angles that can help make sense of various aspects of the transformation of Big Science. Importantly, this also means that there are parts of the analysis that make no use of theory, because there are occasions when it seems the terrain is sufficiently colorful and varied in its own right, or simple and straightforward in its revelations, to be explored also without the help of a map.

This view of theory can be translated to method, and the resulting eclecticism in both realms is the logical consequence of the desire not to simplify (unless necessary) but to make complexity and variety into virtues. Because, as anyone who pays close enough attention to historical events and processes would find, there are few or no "one size fits all" conceptualizations or explanations for history. In spite of the attempts to describe the current science policy regime as simply "postacademic," "finalized," "postmodern," or "postnormal," there is no one organization pattern of science, no one institution of science, and no one way in which science interfaces with society. And further: there is no one science policy system, no one type of laboratory organization, no one process by which large science lab organizations change, or for that matter, no one particle accelerator design. The following sections therefore review pieces of social theory that are judged to have relevance for the analyses of the book, sometimes directly as tools that will be used in the analyses, and in other cases as general enlightening perspectives. This variety in the outline of the theoretical framework reflects the variety of the empirical analyses (and theoretical discussions) in the book, and is ultimately based on the threefold conceptualization and definition of Big Science launched above, and the overall continuity and change theme that is used to distinguish transformed Big Science from old Big Science.

Instruments and Actors

What fundamentally defines the scientific use of very large instrumentation is that it is localized to specific places and offers opportunities for experimental work that are otherwise not available: should it be possible to smash elementary particles together, or obtain high brightness x-rays or intense bursts of neutrons without large machines, then large machines would not be used. This is not unique to Big Science; most work in the natural sciences requires specific material arrangements and places of its own, for organizational reasons and for effective resource utilization (Henke and Gieryn 2007: 355; Livingstone 2003). Throughout history, experimental science seems to have been driven more and more towards concentration to specific locations and use of increasingly sophisticated technological setups.

John Ziman (1994) has conceptualized this trend, identifying a broad and long-term sociological change of science that takes the shape of specialization, disciplinary restructuration, and the emergence of new (cross-disciplinary) fields closer to commercial interests or fulfillment of immediate societal ends ("extensive" science, in Weisskopf's terminology). An important part of this is the "sophistication" of technologies, that is, an increase in the complexity of instruments, which also drives "collectivization" and the rise of team- and project-oriented work (Ziman 1994: 122–123). Scientific collaboration has, authors argue, increased gradually during at least the past century (Adams et al. 2005; Melin and Persson 1996; Vincent Lancrin 2006). Most of the evidence for such a development is quantitative, identifying a broad and general increase in co-authorship of scientific publications. Katz and Martin (1997) take a different perspective than Ziman's (1994) structuralist view of collectivization as a default development of science, arguing on a rational choice theory basis that collaboration is the result of deliberate choices on behalf of individuals, to get together and produce a "joint good" (cf. Hechter 1987). Both have their merits and explanatory value, but scientific collaboration, what it is and what it means, remains a complex topic to study.

One tradition in the social studies of science, that of laboratory studies, deals with this topic but with a radically different point of departure, namely with epistemological rather than sociological ambitions.

The ambitions of its proponents to study "science in the making" in order to unveil its inner functions and "deconstruct" its fact claims have produced some important contributions to the understanding of contemporary science: There is little or no methodological unity in science (Knorr Cetina 1981: 47); laboratories deal with purified and idealized versions of phenomena rather than "nature" (Knorr Cetina 1999: 26–27; Hacking 1983: 226); and science is just as much—if not more—about making things "work" than about finding "truth" (Knorr Cetina 1981: 4; Mulkay 1981: 164).

The last point is important because it emphasizes the material aspect of scientific work, which is of course especially evident or tangible in the case of Big Science. The argument forwarded above, that scientists would not use very large machines if these were not necessary for their scientific pursuits, can be reversed and taken further: much scientific work would not be possible without very large machines, and therefore, very large machines enable and entice scientific work just as ideas and people do (Van Helden and Hankins 1994: 4). Especially in old Big Science, but also in many little science or transformed Big Science settings, experiments are planned and executed in parallel with instrument development, to the degree that it can be difficult to separate instrument from experiment, and indeed the design and construction of technological gadgets from the formulation of research questions (Smith and Tatarewicz 1994: 101). Tied to the material reality of scientific investigation and the sophisticated technologies of instrumentation is the advanced skill of both instrument designers/builders and those that utilize instruments or facilitate its use (Livingstone 2003: 142), and so there is both a partial dissolving of the boundaries between the professional roles of scientists and instrument designers/builders, and the emergence of new roles.

Shinn and Joerges (2002; Joerges and Shinn 2001) launched the concept of "research technologists," a professional identity in the border area between scientific investigation and instrument development, whose practitioners are the promoters of certain technologies and who move rather freely in an "interstitial arena" between organizations and interest spheres such as universities, firms, labs, institutes, government, and the military (Shinn and Joerges 2002: 207). These actors are typically the inventors and promoters of instruments with the potential of serving many different areas of use, so-called “generic instruments” or “generic technologies.” These may be designed and developed for one purpose but subsequently used for other purposes, or deliberately constructed to fill several, not predefined needs. The concept is not restricted to science, but applies to technology in a larger context, not least as Rosenberg (1992: 382) has shown by pointing to the microchip as perhaps the most spectacular example, a technology whose wide use today was certainly not anticipated at the time of its invention. Other examples mentioned by Rosenberg (1992: 384) are the electron microscope, which has been adopted for a wide range of purposes in physics, chemistry, and biology; the technology of nuclear magnetic resonance (NMR), which was invented to measure the magnetic moments of atomic nuclei, and which rapidly became an indispensable tool in analytical chemistry and was later adopted by biologists as well as in medicine, for imaging as part of medical diagnosis; and of course, particle accelerators. Originally invented to examine the structure of the atom, particle accelerators were used to produce nuclear material as part of the Manhattan Project (Heilbron et al. 1981; Hiltzik 2015) and for production of radioisotopes for medical research and treatment. Later, as will be described in greater detail in Chap. 2, particle accelerators became “generic instruments” and used to support a whole range of experimentation in the natural sciences. The transformed Big Science is arguably one of the most prominent—and physically imposing—examples of a technology that becomes generic and starts to move across disciplinary and institutional boundaries.

Conceptually, this has two important implications. First, it has been argued that the disunity of science with respect to method, organizational patterns, and all other traditions and cultural traits is countered by the unity of instrumentation. This unity is not to be understood as a lack of variety in technological setups—quite the reverse, as technology varies as much as the social world—but by the role of instruments in science: they are physically tangible and clearly delimited, and they have a generic ability to serve many purposes (Hacking 1996: 69; Van Helden and Hankins 1994: 6). This means, importantly, that the big machines of Big Science, old as well as transformed, make up durable entities that provide continuity in a changing world (Shrum et al. 2007: 2–3). Second, the actor group conceptualized by Shinn and Joerges (2002) as “research technologists" should be expanded somewhat: while it would seem like technology itself has a capability of shaping laboratory settings and scientific and organizational structures, there is always human agency behind or involved. Literature on innovation and institutional change identifies the role of "institutional entrepreneurs" as significant: this refers to actors who initiate or drive changes that make significant contributions to the transformation of existing institutions (DiMaggio 1988; Battilana et al. 2009) by investing time and resources in technological and social innovations, be they (generic) instruments or initiatives within or across organizations, and that also enjoy a social status that gives them legitimacy that can spill over to these causes that they promote.

Such actors play important roles in the history of science and Big Science: scientific progress and scientific work always originates with individuals. As a cognitive activity, surely embedded and entrenched in all kinds of (social) institutions, cultural habits, and discourse, but originating in thought and creativity, science is a fundamentally human endeavor and can originate only in individual minds. There is no such thing as a thinking organization, institution or system. This postulation, provocative for some, is generally confirmed by all those closer looks at the micro level of Big Science and, more generally, of science, science policy, and science organization that have been provided by historians (e.g. Crease 1999; Greenberg 1999/1967; Heilbron et al. 1981; Hermann et al. 1987, 1990; Hewlett and Holl 1989; Hoddeson et al. 2008; Holl 1997; Kaiserfeld 2013; Krige 1996; Lindqvist 1993; Lowen 1997; Mody 2011; Riordan et al. 2015; Westfall 2008b, 2010, 2012). In all cases where the detailed historical narrative of the development of a Big Science lab has been laid out and analyzed, it has become clear that where change has occurred, it has been either driven by myriad micro-level deliberations, decisions, and actions that in combination have caused transformation from below—surely resonating with the shift of aims and objectives on policy level and with institutionalized expectations and routines, but impossible to explain without accounting for actors—or by especially powerful actors who certainly have a power base in institutional/organizational settings and politics but who are framed as independent and strong individuals. While occasionally balancing on the verge of hagiography, these historical accounts show that a theoretical model that captures the role of the individual, capable of making rational decisions and acting determinedly, is instrumental to complementing theories with a focus on politics and institutions. In other words, they (unknowingly) advocate methodological individualism, the axiomatic basis of rational choice theory (Coleman 1986; Lindenberg 1990).

Rationality is a contested concept in the social sciences, mainly because of its association with neoclassical economics, which is unfortunate but not impossible to shrug off. The nonsensical claim that a rational choice perspective on action would presuppose a dehumanized and perfectly rational "homo economicus" has been refuted directly (Opp 1999, 2013) and was also preemptively discarded by the ingenious development of the concepts of "bounded" and "limited" rationality within organization studies (Simon 1957; March and Simon 1958; Pfeffer 1982), as well as by the promotion of "procedural" or "sociological" rational choice theory (Esser 1993; Goldthorpe 1998) and the use of rational choice theory in political science and decision-making theory that is not directly tied to, but inspired by, game theory and idealized analyses of negotiation and political action (Shepsle and Bonchek 1997; Coleman 2009). All these show that while human actions are obviously restricted by conventions, habits, and lack of information, and certainly also by a myriad of things that can be summarized as "institutions" (see below), this does not preclude humans from acting in accordance with what they believe to be the best course of action to promote and preserve their interests—in other words, acting rationally. And most of all, it would be difficult to account for social change processes and analyze them responsibly without allowing for a view on individual action as rational or at least purposeful.

The rational actor framework is instrumental for a view on individuals and groups of individuals as agents of change, because it enables free and creative hypothesizing and deduction on the basis of assumed, stated or proven interests and ambitions of the scientists, lab directors, government bureaucrats and politicians that are central to the story. The deeper an analyst goes into a case study (or a set of case studies), armed with the rational actor model as an analytical tool, the more detail can be revealed, and thus the more knowledge can be established, about the case(s). But since the virtue then is the detail, and since the demarcation is the case study, this will also lead to an inevitable loss of context and generalized understanding. Which means, by extension, that the rational actor model must be contrasted against, and preferably used in combination with, excerpts of theory that account for macro- and meso-levels, and that enable acknowledgement of longer time frames and processes on other levels and scales.

Institutions and Politics

A key component of the conceptualization of a transformed Big Science (above) was the acknowledgement that there is continuity in its institutional embeddedness. This is most evidently shown by the remarkable resilience of the organizations of Big Science, which led to the comment in an earlier section that it seems as if problems change while solutions remain, but also by the rudimentary realization that anything situated in the historical period of (late) modernity by necessity is embedded in a vast conglomerate of loosely coupled institutions and organizations, each with a life of its own and in relation to the others (e.g. Giddens 1991; Münch 1988), and each with an influence of some kind on our focal category of analysis: Big Science. A fundamental insight from organizational sociology conveys that all organizations were once created to attend to a specific set of problems and perpetuate a specific set of interests and act in partial independence to do so, and that organizational behavior in a particular instance is determined primarily by routines established prior to that instance. Organizations have purposes and practices that are other than those of any individual, and also capabilities that go beyond the sums of the partaking individuals (e.g. Sims et al. 1993; March 2008). Organizations also, to some degree, follow routines that may seem to run counter to their purpose(s) and that only partially cohere with formal or declared practices, but that are explicitly or implicitly believed to render them social legitimacy and prestige (Meyer and Rowan 1977; DiMaggio and Powell 1983). This goes for the organizations of Big Science as well as for the organizations that embed and enable Big Science: governmental agencies, universities, research institutes, scientific societies, and trade unions, for example. As formal and informal structures, organizations are resilient by their inertia (Zucker 1977; Meyer and Rowan 1977), and this is conceptually important because it relates directly to how change is accomplished in spite of inert structures, or within them and with the aid of them. Organizations are formally structured but also rely heavily on informal relationships, hierarchies, and networks that promote and hinder initiative and action (March and Simon 1958). This furthermore connects to an important conceptualization by Luhmann (1995, 2000) with direct bearing on the study of organizational inertia, change, and action: seen as social systems with the ability to renew on basis of their own elements (autopoiesis), organizations are necessary structures for facilitating the accomplishment of long-term and profound change in any given field. Put differently, it is difficult to imagine change without continuity.

Hence, institutions are persistent but institutions also change. Merton's aforementioned conceptualization of science as an institution is a telling example because behind it lies the identification of the persistence and resilience of the institution of science as instrumental to the progress of scientific knowledge production, which is intrinsically about change (cf. the Kuhnian "essential tension" discussed in the first paragraphs of this chapter). It can well be argued that the established routines of organizations and institutions with generic purposes of, for example, governing a country, or distributing money from the public purse to priority areas, or keeping a balance of different regional interests in political decision-making, becomes all the more important vis-à-vis the power of the logic of efficiency or purposefulness when science is at the heart of the process. Science is inherently unpredictable and therefore the organizations put in place to sustain scientific activities are always inert and partly out of touch with the content of these activities. In other words, it is necessary to adopt as part of the basic framework an institutional perspective on organizations that account for those regulative, normative, cultural, and cognitive features of organizations that provide stability and are crucial for organizational survival and legitimacy (e.g. Meyer and Scott 1983). These "institutional logics" that consist of norms and value systems that govern individual and organizational behavior differ, importantly, between institutions and can be distinguished as part of an analysis of the interplay of various interests and actions in negotiations over priority-setting (Thornton and Ocasio 1999; Thornton et al. 2012).

Institutions matter, and therefore the institutional perspective on organizations and governance is necessary. But both classic institutionalism and neoinstitutional theory (which is nowadays not so new anymore but also quite classic) have one major drawback, namely that they view institutions as so stable that they cannot convincingly explain how they emerge and change (which they doubtlessly do) without invoking the concept of exogenous shocks that occur at "critical junctures" and that "punctuate equilibriums" (Capoccia and Kelemen 2007; Pierson 2004). Empirical observations, perhaps not least in the history of science, strongly refute this.

Recent advances in historical institutionalism have enabled a view on change in highly institutionalized settings as not necessarily caused by radical, discontinuous events (the punctuation of equilibriums) but by gradual, incremental, and cumulative renewal whereby elements of organizations and institutionalized systems and fields are added, substituted and forsaken on multiple levels and in multiple dimensions, so that the overall long-term result is radical and discontinuous (Mahoney and Thelen 2010; Mahoney 2000; Streeck and Thelen 2005). Acknowledging the institutionalized nature of organizations and actors in society, going back at least to the Enlightenment and the first industrial revolution, is necessary to make sense of some patterns of decision-making and behavior that are not easily captured by other means. In this book, the institutional perspective guides in both of the two central quests: explaining continuity and explaining change. The combination of institutional theory and theory of (rational) action has a remarkable strength in this pursuit (cf. Hall 2010). But one piece in the puzzle remains.

Key processes in the development of Big Science from old to transformed occurred in the realm of politics. In brief, after the end of World War II in 1945, the Soviet nuclear test explosion and the Korean War in 1949–50, and the Sputnik Crisis of 1957, the old Big Science stood at its height. The political conditions then changed, by the Superpower détente in the wake of the Cuban Missile Crisis, the social upheaval of the late 1960s, and the collapse of the Bretton Woods system and the Oil Crisis in the early 1970s. These events provoked a renegotiation of the Social Contract for Science that brought a "squeeze" on Big Science in the late 1970s onward (Westfall 2008a), and the grip was not really loosened until the mid-1980s, when the "Second Cold War" policies of the Reagan administration (Judt 2005: 592), in combination with new or complemented expectations and demands of (provable) competitiveness and accountability on science, gave renewed life to Big Science. The end of the Cold War seems to have brought the final blow to old Big Science, most extremely manifested of course by the demise of the Superconducting Super Collider (SSC). The rise of other pressing issues (health and grand challenges) as well as new solutions (materials and life sciences) with Big Science applications of their own also made the emergence and growth to prominence of a transformed Big Science seem like a historical inevitability (see Chap. 2). Riordan et al. (2015: 254) summarize with the rhetoric eloquence of euphemism the science policy doctrine of the Clinton administration, taking office in 1993: "Being the best in the world in an expensive, esoteric discipline with little direct impact on health, jobs, or industrial competitiveness was not high on their priority list" (the "discipline" referred to was, of course, particle physics). Behind this doctrinal shift, which did not occur overnight in 1993 but had been underway for a long time, lied politics with its own institutional logics and its own strong individual actors, as well as procedures that can be rewardingly analyzed with the help of some conceptual tools from political science.

Politics is a game, especially when theorized, and an important part of understanding a phenomenon that is both scientific and political—like Big Science—is to distinguish between the (institutional) logics of science and the (institutional) logics of politics, to see how they act out to coproduce a historical development. The game as such has a number of framework parameters and input values that stem from the essentially collective nature of politics as opposed to individual action (Coleman 2009) and the constant state of compromise that follows from it; its logic of persuasion and choice/priorities; its requirement to follow certain procedures (e.g. constitutional law); its rather straightforward identification of power as equaling influence over outcomes; and the duality of most political action of simultaneously operating on national and international arenas which have radically different logics (Shepsle and Bonchek 1997; Neustadt 1991). All these conditions constrain and facilitate political procedures, and while outcomes of politics may seem simple in the public view, the procedures as such are typically complicated enough to "defy simple summary and easy generalization" (Allison and Zelikow 1999/1971: 263). The combination of collective bargaining, the logic of persuasion, and power equaling influence over outcomes means that the average outcome, not the exact outcome, of politics has primacy for evaluating success: if a certain piece of policymaking is successful in, say, seven out of ten cases, it is likely to get the support of the electorate and/or get passed by whatever legislative chamber it needs to pass, even though it fails in no less than three out of ten cases. This means that one single failed policy outcome does not logically equal bad policymaking or bad decision-making, because there is always a wider picture where known and unknown (i.e. classified or otherwise hidden) factors mix to create a complexity that makes politics differ greatly from, say, the corporate world or public science systems. The outcome of a big historical event with broad ramifications in political terms, such as the Sputnik Crisis or the Oil Crisis, might therefore be something other than what the logics of the specific situation perhaps tell us, because there are deliberations and actions in politics that simply have reasons and meanings that both go beyond the definition of rational and that, because of their complexity, evade most attempts of sense-making. The same goes for the process by which policy is made, especially in immensely complex situations, where interestingly, political action can seem to be defying much of the logic by which the structures for policymaking were set up, and thus create quite unpredictable outcomes (Allison and Zelikow 1999/1971). It goes for Big Science, just as for any other notable area of public policy and investment, that the winners that get to write history have won a game that is just as much political as anything else; in this case, just as political as scientific or rational.

In the case of Big Science and other major public investments, the concept of "pork barrel politics" is especially important. The location of a Big Science lab (or any other big installation such as a military base or a transport infrastructure project) in a certain region customarily makes the elected politicians from this region into supporters of the project in question (Shepsle and Bonchek 1997: 202–206), because the investment constitutes political "pork." The Superconducting Super Collider project, cancelled by the US Congress in 1993 (see Chap. 2), is a prime example of when and how pork barrel politics go wrong (Riordan et al. 2015: 168), but there are several other similar instances where it has played a significant role in both the launch of Big Science projects and the preservation of existing ones with new missions, with less damaging results. One particular example is the context of European collaborative Big Science, where the local/regional benefits of hosting a major project often has gotten precedence in negotiations and decisions (Hallonsten 2014a). Importantly, pork may very well come in the shape of continuation of existing programs or simply decisions not to close a lab or a site that has an outdated mission, but keep it alive by redirection of tasks and purposes (cf. "problems change, solutions remain" above). This is an instance where the respective institutional logics of politics and science seem to meet to create an outcome that is favorable for both.

Among the more popular theoretical models describing and conceptualizing the relation between political decision-making and outcomes is the "principal-agent model" (Pratt and Zeckhauser 1985; Miller 1993), which has fruitfully been applied to science policy and the study of the roles of governmental agencies as intermediaries in making sure that governmental ambitions regarding the public R&D system are fulfilled (Braun 1993; Guston 1996; van der Meulen 1998; Braun and Guston 2003). The principal-agent problem, identified as central to policy delegation, is chiefly one of "information asymmetry": The agent who is supposed to fulfill a task given to her by the principal has, by definition, greater knowledge and expertise about how this task should preferably be carried out. This creates a risk for "adverse selection," which means the selection of inappropriate agents for the task because of a lack of ability of the principal to fully appreciate the suitability of different possible agents; and for "moral hazard," which means that agents cheat or shirk to gain the advantages of the arrangement while not properly attending to the delegated tasks.

Information asymmetry is a key ingredient in understanding science policy and also the big politics of Big Science. In the most basic sense, the quite natural lack of expertise in nuclear physics among politicians (and military officers) was what gave rise to the World War II weapons programs in the first place, and also what provoked the formulation of a science policy doctrine after the war that relied heavily on the Linear

Model of Technological Innovation and that enabled the growth of (old) Big Science on the governmental/public expense account. On another level of detail, the escalation of funding for particle physics in spite of the fact that its connections to warfare and national security was remote, at best, can also be rewardingly viewed in light of the concept of information asymmetry. The Sputnik Crisis quite simply created a need to show political determination in the area of science policy and facilitated vast increases of R&D budgets, and scientists knew how to make use of the opportunities that were thereby opened. Later, the gradual but profound doctrinal policy shift that was briefly mentioned above, and will be returned to in greater detail below and in Chaps. 2, 5 and 6, can also be analyzed with the help of the principal-agent model and the concept of information asymmetry, perhaps as a reaction to (real or just alleged) adverse selection and/or moral hazard in a previous funding regime.

Interestingly, while this shift of science policy doctrines has brought about an increased demand for measurable productivity that coheres with the proliferation of "audit cultures" in public administration (Power 1997), the expectations of what science (and Big Science) will contribute to society have also grown more complex and, in a sense, intangible. Nuclear energy and weaponry, and technological superiority over another superpower, was a comparably simple objective for publicly funded R&D. Today's grand challenges are more fuzzy and unpredictable, and their solutions less tangible and clear. Nonetheless, trust in science's capacity to solve them seems to be greater than ever, which shows in the mobilization of promises and expectations around Big Science: these become tremendous political assets because they provide forceful motivations for new investments. Any political course of action apart from supporting the investment in a major new R&D tool that is pictured as able to contribute to solving society's greatest challenges is seemingly easily discarded as irresponsible (van Lente 2000; Brown and Michael 2003; Borup et al. 2006). Politics is therefore not less important in the current era of transformed Big Science compared to the old; rather, the big politics of Big Science have changed. Put differently, the transformed Big Science is adapted to contemporary political realities. It is still, speaking conceptually, a result of the science policy delegation problem as expressed by information asymmetry: Big Science exists because there is a political motivation for it.

The Knowledge Society

The final paragraph of the last section leads to yet another conceptual discussion about the current science policy regime, its origins in a wide and deep shift, and its extension in the more general relation between science and society. A popular concept to describe current times is the "knowledge society." Its origins may be the concept of "the knowledgeable society" as launched by Lane (1966) to represent a proliferating (over)optimist view on scientific knowledge in the 1960s, which he interprets as potentially revolutionary for current societies in need of reevaluation of authorities and models for decision-making. Though not as analytical as Drucker (1969), who uses "the knowledge society" as a headline for the last section in his renowned book the *Age of Discontinuity* and inserts "knowledge" at a central position of the economies of Western societies, Lane (1966) foretold some of the rhetoric of the "knowledge society" present today by presenting "knowledge" and "knowledgeable" as synonyms of "enlightenment" or "illuminated" and thus the key ingredient in a redemption of an essentially corrupt society heading in the wrong direction.

The use of terms like the Knowledge Society in political rhetoric has attracted some interest in sociology; van Lente (2000: 44) has revived the concept of "ideographs" from studies in rhetoric, originally coined by McGee (1980), to explain the phenomenon. An ideograph is a social asset, "a link between rhetoric and ideology"; colloquial words imported from ordinary language into political discourse where they are used to represent a "collective commitment to a particular but equivocal and ill-defined normative goal" that "warrants the use of power" and "guides behavior and belief into channels easily recognized by a community as acceptable and laudable" (McGee 1980: 15). Society is united by ideographs and divided by their meaning; ideographs simultaneously carry a strong imperative and a strong interpretive flexibility. Typical examples are "liberty" and "equality," which were used for forceful motivation of political (and military) action at both sides of the previous Iron Curtain but meant radically different things for those who used them (McGee 1980: 6). If "knowledge" is an ideograph, or a word that can hardly (if at all) be taken to represent something negative but instead is inseparably tied to concepts like "enlightenment," then the emergence of the term and concept Knowledge Society in political rhetoric is highly expectable. Välimaa and Hoffman (2008: 266) consequently note that the Knowledge Society is a concept that "has created its own images, expectations and narratives" and that can entail a variety of connotations exactly because it is "an imaginary space" which can include "everything related to knowledge and knowledge production (…), regardless of whether it concerns individuals, organisations or entire societies." The Knowledge Economy, on the other hand, is a concept that by most available evidence describes a reality that has developed by the parallel and partly intertwined processes of "commodification" and "economization" of (academic) science (see Chap. 2) and the growth of a postindustrial economy where prosperity and development in at least some countries and regions in the world rely less on production in the classical sense and more on innovation and knowledge-based services.

Yet, the tremendous rhetorical power of the Knowledge Society concept also has a matching reality behind it. The growth of a service economy, the generally increased levels of education across populations, and the proliferation of information technology have, undoubtedly, increased the importance of knowledge as a personal asset both professionally and privately (Stehr 1994). As a result, whole societies and economies are dependent on knowledge in the shape of intelligence and skilled labor just as much as raw materials or physical infrastructure, and there is a clear development path in this as well, as UNESCO (2005) notes in its first World Report on the theme, where it also defines the Knowledge Society as a political objective to which nation states, regions, and the global community should purposefully orient their development work.

They are not alone. When "knowledge" is made a "key defining aspect of contemporary and future societies," it is also expectable that "knowledge" becomes "an increasingly politically laden concept and one on which a range of social interests try and make a claim" (Sörlin and Vessuri 2007: 1–2). "Knowledge" is then not only a very prominent feature of the contemporary organization of society. The institutions engaged in its production and dissemination are also increasingly penetrated by a large number of actors and actor groups with different interests of their own that they aim to project upon the institutionalized processes of knowledge production, dissemination, validation and use, and with their own agendas regarding what knowledge is good for and how it is supposed to be used (Chap. 6). As part of the background for the analysis in this book, hence, the Knowledge Society is to be understood not so much (or at least, not only) as a society permeated in all its parts by knowledge, but as a society where the institutions of knowledge production are penetrated by all of society's other interests and institutions, including but not limited to politics. The emergence and growth of a globalized knowledge economy, and the intensified competition for funding and recognition in science, has produced a flood of performance evaluation exercises that all measure productivity, quality, and "excellence"[7] on the level of individuals, groups, departments, institutes, and even national research systems (Chap. 5). This is a key postulation for the analysis in this book: Transformed Big Science laboratories are characterized by an increased fickleness that has to do with a broader and more complex interface with the institutions of science, politics, society, and the economy (Chap. 6). The fickleness, in turn, has evolved both because of an endogenous capacity or potential to satisfy an increasing set of demands from an increasing number of actors (Chaps. 2 and 3), and the increased pressure of these demands as such, which have all become deeply intertwined with policy, the priority-setting, and the creation and implementation of standards of quality, relevance, and accountability.

Another way of phrasing this is to say that Big Science facilities have become further integrated, together with other organizations and institutions, in an "innovation system." This would mean that both the currently tighter links and greater permeability between the R&D-performing organizations and entities in the public and private sectors also extends to (transformed) Big Science, and that the facilities and the research activities that they sustain are joined with other organizations and institutions in a system whose overarching goal is "innovation." Although partly ideology, and hence wishful thinking (see e.g. Godin 2009), such an "innovation system" framework is useful for conceptualizing results of structural change in those industries and sectors of society that perform R&D tasks and that are engaged in the production and dissemination of knowledge. The "innovation system" was originally conceptualized as comprising the

[7] "Excellence" is a debated and contested concept (see e.g. Readings 1996; Hellström 2011; Münch 2007), hence the use of quotation marks.

"network of institutions in the public and private sectors whose activities and interactions initiate, import, and diffuse new technologies" (Freeman 1987: 1). Today, after several decades of theorizing, it stands for a rather useful inclusive view on the organizations, institutions, and processes in society that are engaged in knowledge production and dissemination. It accounts for previously less recognized factors for knowledge-based economic and social development, such as the role of organizational environments or the importance of interactions within heterogeneous sets of actors in the system (e.g. Lundvall 1992). The transformed Big Science is more fickle and more exposed to all kinds of interests and institutional arrangements in society. This can be usefully viewed by referral to the concept of innovation systems, both descriptively and with regard to rhetoric and policy goals. Neutron scattering, synchrotron radiation, and free electron laser labs are important resources for knowledge production, doubtlessly engaged in a broad set of interactions with various organizations and actors in its environment (Chaps. 2 and 3). They are also increasingly exposed to policymaking based on an ideological view of society, or a sizable share of society, as a system with innovation as the ultimate goal (Chap. 6).

Method and the Structure of the Book

Large parts of this book consist of either syntheses of previously published work, adapted and supplemented by analyses from partly new angles (mostly Chaps. 3, 4 and 5), or syntheses and analyses of work by other authors, that taken together, shed new light on some known phenomena (mostly Chaps. 2, 4 and 6). Methodologically, therefore, the book uses a combination of primary and secondary material, and should be read as combining the elements of three types of academic publications: First, a compilation of previously published articles; second, an extended review and synthesis of secondary sources; and third, a standalone monograph with a thesis of its own. By combining these, the book develops a collective message that in some of its parts may lack some originality but relies on established facts and arguments that have already been laid out in detail in other work. Most importantly, though, the coherent whole forwards an argument and publishes insights that are new and innovative.

In line with what was established above about the necessity of carefully choosing analytical tools from a rich theoretical toolbox, it is important to underscore here that the seeming asymmetry in the treatment of cases and material throughout the book (most obvious in this regard is perhaps the focus on the United States in Chap. 4) is not the result of carelessness or negligence of methodological considerations but a deliberate choice. Cases must be analyzed for their specificity, and in order to achieve qualitative and, not least, interesting results, cases must also be chosen so that their richness can be given justice. There are no two cases that are the same, and no two national science policy systems that are the same, and certainly no two scientific disciplines that are the same. This is a corollary to the approach to theory outlined above, and it not only means that cases and the use of case studies may be highly asymmetrical but also that methods can change between cases, and here, between and within chapters.

Chapter 2, *History and politics*, begins with a historical overview of the postwar development of science policy in (Western) Europe and the United States, with emphasis on those features of the development with direct relevance for the topic. Thereafter, the chapter outlines the respective histories of the use of neutrons for studies of materials and the use of synchrotron radiation and later free electron laser for similar purposes, and reviews the respective political contexts of Big Science and its transformation in Europe and the United States. The chapter is entirely based on secondary sources. In Chap. 3, *Organization*, focus lies on the labs and their social organization. Neutron scattering, synchrotron radiation, and free electron laser laboratories are similar enough in their organizational structures to be described in general terms in this chapter, with the help of some conceptual tools from social theory and the sociology of science. Actors and actor groups are identified and their respective roles in the complex systems of these labs and their external political and scientific environment are laid out.

Chapter 4, *Resilience and renewal*, contains a detailed inquiry and analysis of how, through reorientation of activities from old to transformed Big Science, major laboratory organizations have been able to survive in spite of dramatically changed environmental conditions, by adaptation and redirection of efforts. A focus on the United States is both inevitable and deliberately chosen for the sake of richness and relevance. The first part of the chapter reviews a series of historical articles about facility projects in the US system of National Laboratories, where the resilience of these labs is proven and understood through their ability of renewal. From there, the analysis zooms in on a detailed case, namely the detailed story of how one specific Big Science organization, the SLAC National Accelerator Laboratory in Menlo Park, California, shifted focus completely from particle physics to synchrotron radiation and free electron laser in a transformation that took a few decades but is now close to completion. This case study builds extensively on previous publications but also adds new perspectives and conclusions.

Chapter 5, *Users and productivity*, similarly builds on previously published research to discuss what it means that the transformed Big Science is essentially user-oriented and that the facilities operate in a globalized and increasingly competitive science system. The chapter follows up this discussion with an analysis of the consequences of imposing those contemporary (quantitative) performance assessment schemes that are seemingly inalienable parts of the globalized competitive science policy system on state-of-the art Big Science facilities, thus discussing the inevitability of such an exercise as well as pointing out its weaknesses and risks. The second part of the chapter contains a detailed discussion on the relationship between transformed Big Science labs and their users, as expressed by the body of scientific publications that build on work done at the facilities and that are advertised on facility websites and in annual reports, but that are difficult to trace back to the facilities in a stringent way. This analysis and discussion shows a key feature of transformed Big Science that is crucially important for the role of these facilities in the science system, and by extension, in society: They are essentially service facilities for external users.

Chapter 6, *Socio-economic expectations and impacts*, departs from the realization that there is a gap between the actual epistemic or technological content of the research activities at contemporary Big Science labs, and the selling points for the labs in public rhetoric and advertisement material. This gap is growing, which sometimes can leave the observer with the impression that these facilities are the solutions to all of humankind's problems, both in a scientific sense, by being the loci of the breakthrough research activities that will resolve the issue of global warming or a similar grand challenge, and in a wider socio-economic sense, by becoming hubs or centers of gravity in thriving local and regional knowledge-based economies. The chapter begins by reviewing advertisement material and analyzing its content, placing it in proper context of society's views on modernity and progress. From there, the chapter moves on to a literature review in the area of regional economies and regional innovation systems, against which the established knowledge of transformed Big Science is tried and discussed. The last part of the chapter goes back to the original question of how and to what extent (transformed) Big Science labs can be expected to impact local, regional, national and global economies and society at large, and whether and to what extent this can be measured. The chapter builds on previously unpublished research and a thorough literature review.

The final chapter, *The implications of transformation*, contains a concluding discussion, where threads are woven together and some general conclusions are drawn, also returning to the topics and perspectives reviewed and discussed in this introductory chapter. Conclusions and important results are communicated not only in this chapter but also throughout the others, and Chap. 7 therefore functions more as a summarizing discussion on some especially important themes in the overall analysis, rather than as a classic concluding chapter.

Appendix 1 contains some comprehensive descriptions of the science and technology of particle physics, neutron scattering, synchrotron radiation and free electron laser facilities, their use, and their basic technological setups. Appendix 2 contains a list of neutron scattering, synchrotron radiation and free electron laser facilities currently in operation in Europe and the United States, and some basic information on these.

2

History and Politics

The Military-Industrial-Scientific Complex

Big Science, defined as in the introduction to this book as the combination of big organizations, big machines, and big politics, has its origin in the unprecedented mobilization of science and technology in the service of the state during World War II. These efforts produced the first nuclear weapons, among other important inventions, and from the perspective of politics they most of all showed the world that basic research could have the power to alter the course of history through politics and translated into military might. But scientifically, the wartime R&D efforts were also a continuing pursuit of knowledge, a recent step in a natural continuation of scientific developments, though under new political and organizational conditions. The wartime experience set the framework for postwar science policy in Europe and the United States, and produced Big Science.

Most of the changes to science and science policy (and funding) systems usually attributed to World War II had their origins before the war. A new kind of intimate relationship between politics, the economy, the military, and the institutions of science and technological development

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had been growing in the first decades of the twentieth century. In the late 1930s, several elements of the modern institution of science were in place, including performing organizations (e.g. research universities and institutes), disciplinary categories and their boundaries, institutionalized professional identities, career tracks and peer review systems, and global proliferation of publications in journals and printed books. Public funding for science had started to grow in Europe and North America despite some skepticism and fear of corruption (Greenberg 2007: 5–6; Smith 1990: 28–29). Also, prewar embryonic Big Science had emerged in the form of governmentally sponsored particle accelerator programs, most famously in Berkeley, California, under the leadership of physicist and 1939 Nobel Laureate Ernest Lawrence (Seidel 1992: 21, 28–29), whose lab and resources were harnessed by the US atomic bomb project, codenamed the Manhattan Engineer District and popularized as the Manhattan Project, launched in 1941 (Hiltzik 2015: 213ff).

On August 6, 1945, all doubts as to whether science and technology could have a dramatic impact on geopolitics effectively ended. In the aftermath of this first use of atomic weapons, it also became clear that science endowed with large governmental funding and organized in huge complex endeavors with partly hierarchical command structures could lead to spectacular results. Not only did the atomic bomb emerge out of the wartime efforts within the Manhattan Project and its British counterpart the "Tube Alloys" project, but so did other inventions and innovations with great impact such as synthetic materials, penicillin, cryptology, and radar (Pestre 2003: 70). That results would come out of the large-scale efforts and generous governmental funding was realized on the political level long before the end of the war, which led to the purposeful work towards a formalization of what science historian Daniel S. Greenberg (1999/1967: 51–52) has called the "marriage between science and the state": broad governmental patronage and priority-setting, demands and expectations of utility and (measurable) benefit, and institutionalization or systematization of the connection between science, technology, and all kinds of social and economic development.

The Manhattan Project and its sibling efforts during the war showed that science could be made big or that big organizations, big machines, and big politics could be combined to produce results at an unprecedented level. The alliance between science, the military, and governmental funding only

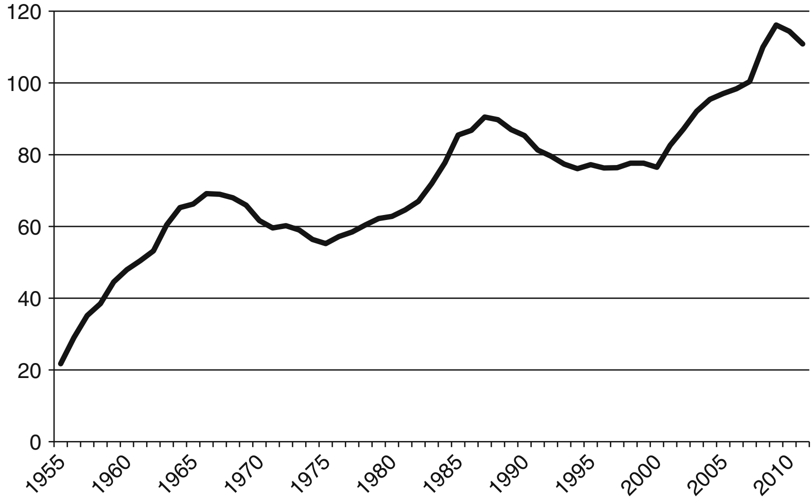

Fig. 2.1 Federal US governmental expenditure on R&D, 1955–2011, in constant (2005) $ billion
(*Source:* National Science Board 2014)

intensified in the years after the war, drawing heavily on the logics of the emerging bipolar geopolitical world order and the superpower competition, but also on postwar economic growth that enabled improvement in standards of living, and political ambition for social change. The United States federal government in particular spent enormous amounts of money on science during the first postwar decades (see Fig. 2.1), but most industrialized countries did as well, and most also launched atomic energy and/or weaponry and nuclear/particle physics programs, with reactors and accelerators as the big machines at the center, and in other areas less technology-intensive but likewise gaining from large organizations and political interest at the highest levels.

Naturally, nuclear physics was the star of the show. Initially, weapons R&D and peaceful exploitation of nuclear energy were not organizationally separated but remained integrated, at least until the 1953 launch of the Atoms for Peace policies by the Eisenhower administration (Hewlett and Duncan 1969: 222ff; Hewlett and Holl 1989: 209ff). Physicists, and not only those working with the subatomic level of matter, soon became used to the privileged situation of getting practically whatever they wanted from their governments. As expressed by physicist and Nobel Laureate Luis Alvarez, quoted in Pais (1986: 19), the physicists "had a blank check from the military" and "never had to worry about money." Particle physics was soon able to break off from nuclear physics largely because of an increasing focus on the use of accelerators to study the subatomic world, and formed its own distinct subdiscipline (besides nuclear and solid-state physics) (Martin 2015: 711). The inherently peaceful purposes of particle physics to expand human knowledge about the inner structure of nature's smallest building blocks made it a popular complement to nuclear physics, which retained its clear military link. Particle physics thus became the prime peaceful research branch in the Atoms for Peace era, but it also developed into an area of its own for superpower competition, partly with another logic where also Japan and (Western) Europe, besides the USA and the Soviet Union, competed directly (especially in the 1960s and onward). This was perhaps most of all because of its grand scales and easily graspable logic: The larger and costlier the accelerator complex, the better, and the higher the accelerator energy, the better (Greenberg 1999/1967: 218–219). Thus, the "blank check from the military" was extended to cover the particle physicists' need for ever larger machines, and these costs were allowed to grow from hundreds of thousands of dollars in the 1940s, to millions in the 1950s, hundreds of millions in the 1960s and 1970s, and eventually billions in the 1980s (Hoddeson and Kolb 2000: 308) (although when the billion-dollar mark was reached, progress was halted, as will be discussed later). Nuclear physics and nuclear energy/weaponry remained an essential feature of US federal R&D for several decades after the war, and to some extent it still is. Although it perhaps did not experience the same stunning resource growth as particle physics in the 1960s and 1970s, it had a prominent role within the United States National Laboratories system and its counterparts in most Western European countries, also including West Germany after 1955, when the allied ban on nuclear research was lifted. The close connection to nuclear weaponry in the United States, United Kingdom and France as well as initially in smaller European countries, and the consequential partial secrecy around R&D efforts in nuclear physics, makes it hard to assess their volume, but they were likely significant in most industrialized countries.

Vannevar Bush, engineer, inventor, and director of the United States Office of Scientific Research and Development (OSRD) during World War II, has been canonized as the architect of the postwar US science system (Zachary 1997). Although his July 1945 report *Science, the Endless Frontier* was only one among several similar white papers on the postwar alliance between science and the state (Greenberg 2001: 47–49), it was incorporated in "the mythos of American science policy" as the policy document that defined the US development into a scientific superpower (Guston 2000: 52–59; Stokes 1997: 2). It established the "Social Contract" between science and society—in short, the arrangement that government shall pay the bill but leave science to be governed by scientists so as to achieve the greatest outcome—and the Linear Model of Technological Innovation, which laid down as a basic principle that if only enough money was invested in fundamental scientific research, it would eventually produce practical applications, innovation, social development, and economic growth (Guston 2000: 37–45; Greenberg 1999/1967: 107, 112–114).

Bruce Smith (1990) has identified the initial 20 year period of postwar US science as characterized largely by a "postwar consensus" between politicians and various professional groups that science policy should be based on the principles of the Social Contract and the Linear Model. If only generously funded and left alone, basic science would flourish and produce results that would turn into commercialized innovations and social, material, military, and economic development "almost automatically" (Smith 1990: 36–37). Neither President Truman (in office 1945–1953) nor President Eisenhower (in office 1953–1961) were particularly interested in science policy, but allowed science to be governed internally and saw to that it was generously funded, until October 1957, when the Soviet launch of the Sputnik satellite provoked a reevaluation of policies. A "Sputnik Crisis" arose in many Western countries as it dawned upon their governments that the US and Western technological dominance over the Soviet Union perhaps was beginning to crumble (Hewlett and Holl 1989: 515). In its wake, R&D investments grew rapidly and the first small-scale attempts at a "strategic turn" in science policy (see below) emerged in the USA in the shape of more direct steering and strategic decision-making on behalf of science policy officials and agencies (Smith 1990: 113).

The most visible macrosociological developments in science in the aftermath of World War II were dramatic growths of investments, of numbers of professional scientists and engineers, and of scientific publications (Nye 1996: 226; Price 1986/1963: 1–13). This growth brought an ample professionalization and routinization of tasks, especially on the side of the physical sciences (Kaiser 2004; cf. Weinberg 1961, op. cit. in Chap. 1). On the governance side, an important development to highlight is the increased involvement of professional scientists in military and government affairs (Needell 1992: 290–291), especially physicists whose positions were particularly privileged. Physics was the most prestigious branch of science, which gave prominent physicists direct access to high political levels and put them in charge of important authorities such as the federal Atomic Energy Commission (AEC), which governed all US atomic energy R&D, military and civilian alike, and oversaw the system of National Laboratories[1] that was created in 1946/1947 out of the remaining physical assets and human resources of the Manhattan Project (Hewlett and Anderson 1962: 714–722).

In Europe, the situation was initially different but developed in some convergence with the United States. In the USA, federal and state governments as well as private enterprise and universities could build on their various prewar and wartime efforts to establish a strong national research system. But Europe was largely in ruins and had little to build on: the exodus of scientists right before and after the war and the Nazi occupation's and warfare's destruction and/or corruption of existing scientific institutions left few assets for postwar rebuilders to use and transform into viable peacetime capacities, with the exception of those countries not invaded by Nazi Germany, in other words Sweden, Switzerland, and the United Kingdom (Herman 1986: 11–13). Nonetheless, in Europe's rebuilding after the war, the notion of science's essential role in economic development had also taken root, and as the economy took off beginning in the late 1940s, national efforts to build up capacity in R&D ensued. The "love affair" between science and the state in Europe after the

[1] Capitalization is used here and throughout the book to distinguish between the National Laboratories of the United States, which is a distinct category of organizations with particular importance in the history of Big Science (see below and Chap. 4), and similar research organizations of national importance and national/federal supervision that exist in other countries, usually under other names, but whose functions are similar and thus are apt to be named "national laboratories" or "national labs," though as a generic category and hence without capitalization.

war (Herman 1986: 15) is just as evident as the "marriage" between the same two parties in the USA (Greenberg 1999/1967: 51–52, op. cit.). In addition, US experiences and policies were exported to Europe, both intentionally and centrally directed, as part of the Marshall Plan and the securing of US influence in Western Europe (Krige 2006), but also by the spontaneous exchange and remigration of exiled scientists to their home countries, who contributed to exporting US science policy and governance to Western Europe.

In general terms, US science policy was emulated in several Western European countries during the Cold War, and the cycles of growth and decline in overall public research funding and the status of science in national policies and public consciousness were also similar between the two continents during this period. European belief in the power of science to drive economic and societal development accelerated in the first decade after the war (Tindemans 2009: 4–7), and the Sputnik Crisis of 1957 led to significantly increased R&D expenditures as well as increased political meddling in the governance and organization of science also in Western Europe. In the 1960s, science came under attack on two fronts. One of them echoed the United States in pointing out negative effects of science on the environment as well as its elitism and connection to society's old and conservative structures. The other was the first emergence of the claim that European countries performed suboptimally in turning achievements in basic science into technological innovations and economic and social development, later named the "European Paradox" (Andreasen 1995: 10) and discussed in science policy as well as several studies (e.g. Maasen and Olsen 2007; Dosi et al. 2006). The economic downturn of the 1970s leveled off the R&D spending growth curves that had been enabled by the tremendous economic growth of the first two to three decades after World War II (Herman 1986: 18–19; Middlemas 1995: 75). A figure for Europe with the same or similar data as that going into Fig. 2.1 (above)—would such a figure be possible to construct—would likely exhibit a similar pattern for the years 1955–1980. After the Oil Crisis of 1973, austerity followed in several areas, not least in R&D, and as neoliberal ideas of increased accountability and demands of relevance spread in many Western European countries in the 1970s and 1980s, the national science systems of these countries underwent similar developments as the US federal system, as described in another section below (Herman 1986: 23–75).

In organizational and institutional terms, what crystallized in the first very few years after the end of World War II was something that historian of science Stuart Leslie (1993) has termed the "Military-Industrial-Scientific Complex," with reference to the popularized term "Military-Industrial Complex" used by President Eisenhower in a January 1961 speech. It was most evident in the United States, but certainly also seen in Western European countries, especially the larger ones. In the first decades of the Cold War, the three sectors of government/military, industry, and (academic) science were deeply involved in a mutual dependence and developmental alliance very much reminiscent of what has been conceptualized, several decades later, as the "triple helix of university-industry-government relations" (Etzkowitz and Leydesdorff 2000). The military connection to basic research had been proven by the end of World War II and became institutionalized in the mix of postwar efforts that all seemed to draw strength from the extraordinary standing of atomic energy. The R&D spending by the US Department of Defense (DOD) reached its peak in 1960, several times higher than the wartime high point of military-related R&D. In the 1960s, the DOD share of the total federal R&D budget oscillated around 80%, and as it decreased rapidly in the 1970s, reaching a level of 20% at the end of the decade, other agencies such as the Department of Energy (DOE), the National Science Foundation (NSF), the National Aeronautics and Space Administration (NASA), and the National Institutes of Health (NIH) took over most of its share. Although in the 1980s, adjusting for inflation, the military share of the federal R&D budget was back up to the record levels of the 1960s, the long-term development was one of gradual detachment of (basic) science from military interest and military funding (Leslie 1993: 1–2).

The Institutionalization of Big Science in the United States and Europe

In the United States, after World War II, the several sites and material installations used in the Manhattan Project and other wartime R&D efforts were converted into a system of national laboratories and several smaller R&D centers with peacetime (albeit not always peaceful) purposes and missions (Hewlett and Anderson 1962: 714–722). Though similar publicly funded organizations conducting mission-oriented large-scale R&D had emerged elsewhere already in the 1920s, for example in the Soviet Union (Graham 1992: 53), the US system of National Laboratories is often regarded as a blueprint for other similar installations that emerged in several industrialized countries in the 1940s and 1950s (Hallonsten 2012b: 89). In 1946, the AEC was created and put in charge of five National Laboratories that would conduct large-scale programs in atomic energy and other areas, including nuclear weapons: the Argonne National Laboratory in Illinois, the Brookhaven National Laboratory in New York, the Lawrence Berkeley National Laboratory in California, the Los Alamos National Laboratory in New Mexico, and the Oak Ridge National Laboratory in Tennessee (Westwick 2003: 31–34). Another seven National Labs were added to the system in its first decade of existence, followed by three in the 1960s, one in the 1980s, and one in the 1990s (Hallonsten and Heinze 2012: 462).

The US National Labs were not only of unprecedented size for publicly funded R&D organizations, but also set a new standard of delicately balanced autonomy, summarized in the "GOCO principle" (Governmentally Owned, Corporately Operated). With their chief mission(s) defined by the government but with far-reaching freedom to pursue their own programs, the labs became tools for the federal government's science and technology policies as well as relatively independent R&D organizations (Westwick 2003: 3, 49, 55). The freedom and the breadth of activities were considered necessities for the labs to be attractive workplaces for scientists so that they could maintain a desirable talent pool, an educated and skilled workforce ready to respond to whatever national security threats and challenges might arise, and to pursue a government-controlled atomic energy program (Westwick 2003: 154). The labs certainly made use of the freedom: as expressed by Norris Bradbury, the first director of Los Alamos National Laboratory, quoted in Westwick (2003: 227), "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."

As shown by Krige (2006), US federal science policy and organizational models had profound influence over the postwar reconstruction of science in Europe. Several countries, most notably the Federal Republic of Germany, directly copied the model of the US National Labs when setting up their own similar institutes in the 1950s (Hallonsten and Heinze 2012: 455ff). With this export of the US model for an institutionalized Big Science, the breeding ground for the vast expansion of particle physics was also put in place in (Western) Europe. For while the activities of the National Labs were Big Science in several possible meanings of the term, also including large mission-oriented programs that did not (and still do not) use big machines, the institutional framework of national labs seems to have been especially conducive to the expansion of particle physics, both in the United States and elsewhere.

As noted, it was not until the 1950s and the launch of the Atoms for Peace program that weapons R&D and peaceful uses of nuclear energy were singled out as distinct entities in the pursuit of the atomic age and became subject to differentiated policies. The international Geneva Conferences on peaceful utilization of nuclear energy and their accompanied policies of sharing and exchanging information on the results of such utilization stand in sharp contrast to the restrictions and secrecy surrounding all the nuclear programs for military purposes in various countries (Hewlett and Holl 1989: 209–270). From this time on, classified R&D on weapons technologies and declassified R&D on civilian nuclear energy and its spinoff technologies (such as neutron scattering, see below) were separated in the federal US governmental R&D budget as well as in those European countries with weapons programs of their own (France and the UK). In addition, the previously launched peaceful nuclear energy research programs of several smaller countries in Europe (and elsewhere) got a boost from Atoms for Peace and the direct help offered by the newly created International Atomic Energy Agency (IAEA) and European Atomic Energy Community (Euratom). The offshoot of particle physics from nuclear physics occurred gradually throughout the 1950s through reciprocal enhancements in accelerator technology and particle discovery, and the culture and community of the discipline was, early on, very much tied to the use of larger and larger accelerator complexes (Mersits 1987: 24ff).

Synonymous with high energy physics (HEP),[2] the growing field of particle physics soon became a field of superpower competition of its own through the "energy race" between the Soviet Union and the

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[2] Particle physics refers to the topic of investigation, subatomic particles, and high energy physics refers to the fact that elementary particles (most commonly electrons and protons) need to be accelerated to high energies and then smashed together in order to disintegrate and allow for their constituents to be analyzed. For a comprehensive technical and scientific description, see Appendix 1.

United States that started in the 1950s as accelerator size grew. At first, the competition was on size and energy level of accelerators, and later it was the discovery of new particles and Nobel Prizes in Physics. In the USA, the AEC had initially hesitated towards funding the construction of accelerators intended only for particle physics research, but the commission also needed a peaceful advertisement science that could show physical realness and produce tangible results, and fit with the Atoms for Peace program (Hewlett and Holl 1989: 257; Holl 1997: 155). In 1957, the US Congress decided to give the AEC a monopoly over funding particle physics machines, and before the end of the decade, its budget had become dominated by accelerator construction and operations costs (Westwick 2003: 151). In the Soviet Union a particle physics accelerator complex opened in the town of Dubna in 1956 (Jungk 1968: 159). Europe soon joined the "energy race" as a third contender. In West Germany, the *Deutsches Elektronen-Synchrotron* (DESY, German Electron Synchrotron) was founded in 1956 and started operation in 1959 (Heinze et al. 2015: 459ff). The Western European CERN collaboration (*Conseil Européen pour la Recherche Nucléaire*, nowadays named the European Organization for Nuclear Research but still abbreviated CERN) was established in Geneva in 1954, largely on the initiative of European physicists returning from their wartime refuges in the United States, and with significant involvement of US political and scientific leadership (Krige and Pestre 1987).

The accelerators for particle physics built in the 1940s and 1950s were still of relatively modest sizes: the Bevatron at the Lawrence Berkeley National Lab which opened in 1954 cost $10 million to construct, and the Alternating Gradient Synchrotron (AGS) at Brookhaven National Lab, opened in 1961, had a price tag of $26 million (Crease 1999: 222). But the 1957 Sputnik Crisis set off a "25-year building spree" in the federal R&D system including the National Labs (Crow and Bozeman 1998: 114), and not least the construction of several more big machines. US federal operating costs for particle physics grew from $7.3 million in 1954 to $33.2 million in 1960 (Greenberg 1999/1967: 216–218). The Zero Gradient Synchrotron (ZGS) at Argonne was approved in 1957 at a cost of $27 million (Westfall 2010: 357), and in 1962, ground was broken for the first accelerator project to break the $100 million ceiling, the Stanford Linear Accelerator (see also Chap. 4). Only five years later, the $250-million-dollar National Accelerator Laboratory (later renamed the Fermi National Accelerator Laboratory, abbreviated Fermilab) was approved by the US Congress and built in Weston, Illinois (Hoddeson et al. 2008: 90–91). At the start of operation of the 6.3 km-circumference Fermilab Main Ring in 1974, the situation for federally sponsored research in the USA had changed, and similar alterations of the Social Contract for Science had been provoked also in other countries. But the systems of national laboratories in the USA and elsewhere remained largely intact: despite temporary budgetary declines in the 1970s and 1990s, no US National Lab and no German Big Science lab has ever been closed—on the contrary, several new ones have been launched within these systems over the years (Hallonsten and Heinze 2012).

The US National Labs institutionalized the postwar Social Contract for Science and managed to renew themselves beyond their initial missions, providing breeding grounds for new initiatives and environments for a kind of natural selection of initiatives (discussed in greater detail in Chap. 4). The "ecosystems" that these labs represent (Westfall 2010), where political, technological, and scientific interests meet and interact to create and breed new large projects in new or known areas, have made them into natural homes for Big Science, both old and transformed. Although many facilities have also been built outside existing systems, it is in fact also hard to imagine the existence of neutron scattering, synchrotron radiation, or free electron laser facilities without the breeding ground of the US National Laboratories and their counterparts elsewhere.

In Western Europe, as noted, national nuclear energy research programs were established in most countries after the war. By 1958, 14 Western European countries had established their own atomic energy councils, commissions or cabinet-level departments: Austria, Belgium, Denmark, Finland, France, the Federal Republic of Germany, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the UK (Herman 1986: 17). In order to achieve true long-term competitiveness on the international scene, however, collaboration was necessary, and CERN was established in 1954 to be the locus of such collaboration. In the long run, as the size and cost of accelerators necessary to remain competitive in particle physics grew (and particle physics entered the state of "megascience," see below), CERN virtually monopolized European national particle physics budgets and became the only experimental particle physics lab on the continent, with the exception of DESY in Hamburg. Later, a number of European collaborative efforts in Big Science have emerged, but only after tense and difficult negotiation procedures. Having been conceived and established as an international treaty organization, CERN offers little organizational or political precedent to build on in the creation of new collaborative initiatives. The 1951 Treaty of Paris that established the European Coal and Steel Community (ECSC) from which later the European Economic Community (EEC), the European Community (EC), and the present-day European Union (EU) have gradually developed, entailed no collaboration on the R&D side (Middlemas 1995: 21–22). Until very recently, the EC/EU has had no mandate whatsoever in basic research. In the 1970s, the EC began promoting specific technologies on the industrial R&D side, and in 1984 the first in a series of Framework Programmes for Research and Technological Development was launched (Grande and Peschke 1999: 45; Papon 2004: 69–70), but the European Commission has not had an active role in maintaining and/or developing the broader research base in Europe. Thus, while it is clearly necessary for the European countries to collaborate in some sciences to achieve international competitiveness, no political frameworks have ever been put in place to create coherence and establish precedence across disciplinary and technological boundaries. The collaborative projects that have been launched have therefore been dependent on ad hoc solutions and the recurring reinvention of legal arrangements and organizational structures, which has created a situation where facility and lab projects are often delayed several years due to protracted negotiations, but where almost all successful intergovernmental projects have been true scientific successes. The lack of precedent and pre-established structures has allegedly prevented bureaucratization and enabled every specific project to meet the demands of its particular scientific community at a specific time (Gaubert and Lebeau 2009: 38; Papon 2004; Hallonsten 2014a: 35).

To some extent, therefore, European organizations of Big Science have shown efficiency and strength to a degree that have enabled a similar institutionalization of progress and renewal capability as the US National

Labs, although in both systems, success is never a given but depends on the ability of actors to coordinate a vast variation of interests and agendas, as well as timing and some coincidence. The larger countries in Europe (France, Germany, the UK) have managed to establish some organizations of the same type as the US National Labs, and those intergovernmental collaborations that have passed through the needle's eye of European integration politics (see below) have often shown world-leading performance in their respective fields.

A Renegotiation of the Social Contract

Nuclear and particle physics rose to the throne of postwar science largely because of the rapid forming of a Military-Industrial-Scientific Complex, the elevation of the Social Contract and the Linear Model to prime principles of the postwar science policy doctrines of the United States and most of Western Europe, and the scientific and technological progress undoubtedly achieved in these fields in the early Cold War era. The progress in particle physics continued for a long time (with the recent discovery of the Higgs Boson at CERN, it can well be argued that its scientific success continues to this day), but the Complex, the Social Contract, and the Linear Model all came under attack already in the 1960s.

The Sputnik Crisis set off a "building spree" (Crow and Bozeman 1998: 114, op. cit.) and increased R&D expenditure dramatically, but the Cuban Missile Crisis of 1962 reminded policymakers and the general public of the real threats of the Cold War and the role of science in creating these threats, and as the 1960s proceeded, more and more reasons turned up for a growing public distrust in the (scientific) establishment. By 1970, therefore, "the social, political and economic landscape had changed dramatically" and "the assumptions of science policy became subject to intense scrutiny and doubt"; and although they were "cushioned somewhat from the shocks affecting the political system by virtue of the esoteric nature of their work, scientists were forced to confront an insistent and clamorous attack on premises that had once appeared self-evident" (Smith 1990: 71–72). The elitism of the scientific establishment and its part in the Military-Industrial-Scientific Complex made it a target among others for the 1960s equal rights movement, the environmentalist movement, increased consumer activism, the anti-war movements and general anti-establishment sentiment. The Vietnam War, which epitomized much of the conflict, had little direct impact on science policy but other symbolic events were more significant, such as the 1962 and 1965 publications of Rachel Carson's *Silent Spring* and Ralph Nader's *Unsafe at any Speed*. The 1960s also saw a more general "anti-science trend" that had to do with increased demands of return for investments of tax money and some signs that the Linear Model was not working as intended (Westwick 2003: 296). In 1963 the DOD had conducted a review named Project Hindsight that analyzed the R&D origins of a number of major US weapon systems, which showed that the contribution of basic research, in the report named "recent undirected research in science" was close to zero (Guston 2000: 78; Greenberg 1999/1967: 31). In other words, it gave some proof that the Linear Model did not work properly. In 1966, President Johnson held a famous speech asking for—or demanding, depending on interpretations—more visible and more clearly beneficial results from science (Smith 1990: 75). As the 1960s proceeded and Richard Nixon became president, a reshuffling and restructuring of the science policy and governance system ensued, with the main outcome being that the previously so powerful "close circle of executive-branch officials and their outside scientific advisers" lost much of their power, and political and bureaucratic institutions took over the reign (Smith 1990: 72).

A specific example of this governance shift, of special relevance here, was the 1974 abolition of the AEC and its replacement by the Energy Research and Development Administration (ERDA) and the Nuclear Regulatory Commission (NRC). The AEC had been a very powerful governmental agency, with monopoly on oversight and governance of all atomic energy and weapons R&D, operation, and distribution within the United States. It had simple decision-making structures and was supervised only by the congressional Joint Committee of Atomic Energy (JCAE), where the launch of new projects and programs were debated in closed sessions (Smith 1990: 63). Particle physics "enjoyed a privileged status" under the AEC, mainly because its big projects could be shepherded through the White House and Congress largely in secret and by a politically agile group of insiders who had little or no accountability demands to live up to (Riordan et al. 2015: 3). By the 1974 abolition of the AEC, the stewardship of the National Labs became part of a broader battery of missions within ERDA, which was also more bureaucratized and partly attuned to needs other than the AEC, but was short-lived, and in 1977 was integrated into the newly founded cabinet-level Department of Energy (DOE). The DOE was "very much larger and more bureaucratic" and "combined the nuclear weapons program, the operations of National Laboratories, and a wide array of energy policy, regulatory and applied research programs" (Marburger 2014: 231–232; Smith 1990: 92). The abolition of the AEC also changed the decision-making structure for the National Labs and put major new projects under the scrutiny of the separate House and Senate Appropriations Subcommittees on Energy and Water Development, whose members had less specialized knowledge of scientific issues, and were more interested in bringing pork back to their districts than sustaining the scientific productivity and excellence of particle and nuclear physics (Riordan et al. 2015: 3). The first secretary of energy, appointed by President Carter, was Arthur Schlesinger, who had been President Nixon's last secretary of defense but was dismissed from that post by President Ford, and who was described as having an "aggressive and demanding style" that was to become "an asset in implementing the 'zero based budgeting' management approach" for the DOE that was apparently advocated by the president (Marburger 2014: 232). In the last few years of the 1970s and the beginning of the 1980s, several new presidential and congressional oversight programs were implemented, together with several new targeted R&D efforts in specific fields, and new pieces of legislation were introduced that would facilitate commercialization of federally sponsored R&D and enhance environmental protection at the National Labs and other federal research establishments (Smith 1990: 90–97). But the policy changes also increased the bureaucracy of the already bureaucratic DOE, and made the governance of large scientific facilities projects more complex and fragmented (Marburger 2014: 232).

This change in the governance of Big Science in the United States can be rewardingly interpreted as stemming from the identification of a risk, on the level of policymakers, for adverse selection and moral hazard as conceptualized as part of principal agent theory (see Chap. 1). This risk was apparently judged as minuscule during the years of supremacy of the Linear Model and the Social Contract, but grew increasingly real to policymakers as public impatience with existing structures grew and new demands for more visible outcomes of the vast investments in R&D were voiced. Indeed, the distrust in the ability of the scientific establishment (or the Military-Industrial-Scientific Complex) to meet the needs of society and contribute to resolving its challenges is an ultimate form of (perceived) adverse selection. Some of the criticism was also quite likely voiced on the theme of moral hazard: thus could the lack of transparency and accountability in the science policy and governance system certainly be viewed.

In quantitative terms, as seen in Fig. 2.1, the inflation-adjusted growth of federal US governmental R&D expenditure in the post-World War II era shows three cycles of growth-decline/saturation. From the mid-1950s through the next ten years, propelled by the Sputnik Crisis and interrupted by the increased federal expenditures for the Vietnam War and President Johnson's Great Society policies, vast growth took place that tripled the total appropriations. The decline in the late 1960s and early 1970s, amplified by the Oil Crisis and the recession, was largely restored in the second half of the 1970s, and turned into a rather dramatic growth in the 1980s. The downturn after the end of the Cold War in 1989–1991 was only leveled out towards the end of the 1990s and not replaced by growths until after the turn of the millennium.

In qualitative terms, Irvine and Martin (1984a) were early in identifying a "strategic turn" of science and technology policy in the late 1970s and early 1980s that van Lente (1993: 10) has argued especially boosted the role of expectations and promises in such policymaking (see Chap. 6). In the initial postwar period, (over)confidence in science and technology allowed for (over)optimism to govern, with generous sponsorship and little performance evaluation. From the mid-1960s onward, a "darker vision replaced the innocence and optimism," while public pessimism and (perceived) absence of progress and breakthroughs (relative to promises and expectations) forced accountability and harsh priorities, along with the weighing of investments in R&D against social, environmental, and economic interests (Smith 1990: 3). The "strategic turn" in the late 1970s and early

1980s not only brought policies devised to mitigate or counter adverse selection and moral hazard, but also increased demands and expectations of what science could and should contribute to economic growth and social progress. The idea of "strategic science" (Irvine and Martin 1984a)—basic research carried out with some kind of implicit expectation that it will produce utilizable results—meant partial restoration of a prominent role for basic science in the model for economic development, though with a rather complex set of rationales and attributes, and most of all a significant shortening of time frames (and patience) and intensified performance evaluation and strategic priority.

Elizabeth Popp Berman (2012, 2014) has analyzed the federal US policy reforms of this era, from the late 1970s onward, and has challenged the widely popular assumption that publicly funded science has been (negatively) affected by an agenda of "neoliberalism" that has sought to minimize governmental steering, open up science to market logics, and compensate by strengthening (intellectual) property rights (all three are hallmarks of "neoliberalism"). Berman (2014: 405, 410ff, 419) shows that several of the legislative reforms in this period were proposed and carried through from the political left rather than from the right, and thus originated in "state-interventionist" agendas rather than "neoliberalism." Therefore, Berman argues, "economization" is a far more accurate term for the overall doctrinal policy shift in this period: both the "neoliberal" and "state-interventionist" agendas for legislative reforms in the United States pursued a public science that was more market-oriented and employed to sustain and provide input to the economy "and related economic abstractions (e.g., growth, productivity, the balance of trade)" that are also associated with certain indicators (e.g. GDP, national R&D expenditures). The left and the right united, it seems, in a quest to turn publicly funded science into, above all, a source of input to the larger economy. The policy shift coincided and cohered with the enlarging knowledge base of economics as an academic discipline that placed an increasing number of factors into the equation of national (or regional, or sectorial) economic performance: for example, science and its hoped-for offspring, technological innovation. "Economization," writes Berman (2014: 399), "is always linked to the idea that the main purpose of government is to affect positively the larger economy."

The concept of "commodification" of (academic) science was briefly mentioned in Chap. 1, in connection with economization and the concept of the Knowledge Society. "Commodification" is used to describe a recently developed state of publicly funded science where economic criteria dominate the evaluation of quality, relevance, and success (Radder 2010; Kleinman and Vallas 2001). It is a twenty-first-century term, but it bears striking similarity with the use of the word "commodity" in Marxist analysis, which means something that is given a monetary value although normally, previously or originally it has had (an)other type(s) of value(s) attached to it and has not been considered a good in the traditional sense. The commodification of (public) science is a process similar to Berman's (2014) "economization": Scientific knowledge has moved from being viewed primarily as a public good to being viewed primarily as a financial good (Pestre 2005) and a driver of economic value-creation rather than wider or deeper advancement of civilization or culture (cf. the innovation system perspective and its basic assumption that innovation is the ultimate goal for science; Chap. 1). Radder (2010: 14) argues that current science policy, governance, and organization are under strong influence of the ideals brought by commodification as well as its mundane effects, seen for example in how productivity, quality, and relevance are measured (cf. Chap. 5). That measures of performance are important is a key feature of the Audit Society (Power 1997), which is the conceptualization of a society so reflexive that appraisal has become interwoven with culture and allowed to govern public institutions and social life, with the risk that the purpose of appraisals is forgotten and the values that audits were originally devised to secure are lost in the process.

Berman's analysis of economization and the cited conceptualizations of commodification are instrumental here, because they point to a profound shift in policy doctrine that is not possible to reduce to a left–right issue, but transcends the political spectrum and is experienced globally. Guston (2000: 113) points specifically to the concern for what was called the "innovation problem," a questioning of the factual productivity and benefit of basic science that grew strong in the 1970s and cohered with the recession that forced a demand for policy reforms that would increase the (measurable) economic turnouts of the vast federal basic research program (cf. the "European Paradox," above and Chap. 6) (Johnson 2004: 219; Greenberg 2001: 15). For the US National Labs, the increase in federal spending on R&D in the 1980s as part of Reagan's renewed efforts in both weapons-related and civilian R&D provided some relief after the downturn in the 1970s. But there was also a "squeeze" of sorts (Westfall 2008a), namely a continuation of the doctrinal shift in federal science policy (economization) that forced National Labs and other federal R&D centers to prove their worth and demonstrate their productivity and contributions to the economy (Johnson 2004: 219–221; Hallonsten and Heinze 2012: 454). During one of several reviews of National Labs and their R&D programs, Silicon Valley entrepreneur David Packard who headed a review panel reportedly "chilled the hearts of laboratory directors across the nation" (Westfall 2008b: 571) when he uttered the (in)famous phrase "Preservation of the laboratory is not a mission" (Holl 1997: 401; Hallonsten and Heinze 2016). As shown in analyses of the whole life cycle of the system of National Labs in the United States as well as their counterpart in the Federal Republic of Germany, this remark was rather untruthful: from a governmental point of view, it seems the preservation of labs is indeed a superior objective of policy (Hallonsten and Heinze 2012, 2016).

Megascience, the Superconducting Super Collider, and the Fall of Big Physics

At least until the early 1990s, particle physics continued to grow almost unaffected by the cycles shown in Fig. 2.1 and the doctrinal shift in science policy theorized by the authors cited at the end of the last section. But as the size of accelerator complexes grew, so did the concerns across other parts of the R&D system that particle physics would "monopolize the increasingly limited resources" and damage the prospects of development in other fields, especially other areas of physics (Kevles 1995/1977: 422). The heavy investments in SLAC, Fermilab, DESY and CERN in the 1970s seem to some extent to have crowded out other areas (Holl 1997: 328; Widmalm 1993; Ritter 1992), and did, among other things, lead to a concentration of efforts in particle physics that forced some National Labs and their counterparts in Western Europe to redirect and invest in other programs (Chap. 4).

But the discipline of particle physics, as it developed in the 1950s and on, had an irrevocable progress built into its development that also, in a sense, foretold its eventual decline and death, since its continuous scientific advancement was inseparable from the growth in the size and cost of accelerator complexes. In 1963, the AEC had convened a panel chaired by Harvard physicist and later (1989) winner of the Nobel Prize in Physics, Norman Ramsey, to assess the current situation and future needs for US particle physics, and to make some recommendations within the framework of certain restrictions on the growth of federal investments in the field (Greenberg 1999/1967: 243–244). The panel's main recommendation was for particle physics to aim for higher energy ranges at the expense of higher intensities, which would mean a faster development to larger machines—in other words, costlier machines (Holl 1997: 217). In the long run, this development would have to provoke a concentration of federal efforts to a very limited set of lab sites. Also, without the budgetary strains of the late 1960s and early 1970s, federal funding for particle physics would have to cease duplication of efforts within the system of National Labs and focus on the construction of only one flagship machine at a time, and incentivize collaborations between labs (Westwick 2003: 285). A natural arena for such collaboration soon emerged: Not only did accelerator complexes grow but so did the detectors used to record particle collision data (see Appendix 1), and labs that were not endowed with their own forefront machine could put efforts in particle physics into detector development. Thus, they could both exploit and contribute to the accelerator-based experimental particle physics programs at sibling National Laboratories.

In their historical analysis of Fermilab, Hoddeson et al. (2008) conceptualize this development of particle physics in the 1970s as its transition to "megascience," with single experiments prolonged over several years and engaging teams of 100s of researchers. The development was driven both by the intrinsic scientific push towards higher energies and thus larger equipment beyond the capacity of smaller teams to fund and operate, and by the concurrent slowdown of spending increases. The advent of "megascience" was, hence, micro-sociological, organizational and political (Hoddeson et al. 2008: 281), but there was a corresponding epistemological development in the field that is best described as the consequence of Weisskopf's (1967, op. cit. in Chap. 1) "intensification," namely a widened disconnect between the discoveries of particle physics and the rest of science, as the hunt for subatomic particles and forces reached deeper and deeper levels far from the focus of the rest of the natural sciences on electronic, atomic, and molecular structures and interactions.

But megascience also had a macro-level counterpart in the resource concentration that it necessitated and that first made SLAC and Fermilab the only two national particle physics labs (with a complementary role given to Brookhaven for a period of time, see Chap. 4), until the 1980s saw movement towards one national priority in the shape of only one national megaproject. In addition, megascience seems also to have had a bureaucratic dimension: the abolition of the AEC moved the oversight of construction and operation of large research infrastructures and the stewardship of the National Labs into another realm of Washington politics and administration, where bureaucracy ruled, and decision-making followed predefined structures and rules of procedure. This lay in contrast to the AEC officials' smooth maneuvering on Capitol Hill and often direct access to congressmen. By the late 1970s, large scientific infrastructure projects were handled by lower levels of government, and in combination with the propagating demands for cost-cutting and increased accountability, this led to the development of robust but cumbersome standards at the DOE Office of Management led by Edward Temple in the shape of review procedures, benchmarks, and project management plans. In order to proceed from idea to concept and further towards approval and eventual construction, all new DOE projects had to pass a Temple Review, "a careful assessment of the technical feasibility of its plans" (Westfall 2008b: 578). The Temple Reviews, and their sequel Lehman Reviews (named after Temple's successor at the DOE, Daniel Lehman) clearly "marked a new level of DOE project management, oversight, and scrutiny of research funding" (Riordan et al. 2015: 48).

Much evidence exists to support the suspicion that these new project management standards were what ultimately killed the Superconducting Super Collider (SSC), the largest scientific infrastructure project ever launched. The idea took its first steps towards realization in 1982, at a meeting organized by the American Physical Society's Division of Particles and Fields in Snowmass, Colorado. This was, allegedly, "the first time all four US accelerator laboratories met with each other and the university particle physics community to plan a collective future" (Marburger 2014: 222), and in a sense it was therefore a showing of the policy-organizational side of the development of particle physics into "megascience." The SSC was originally not much more than a "romantic vision" (Hoddeson and Kolb 2000: 275), but soon emerged as the natural response of US particle physics to the growing European world dominance in the field: The recently completed Super Proton Synchrotron (SPS) machine at CERN produced groundbreaking results in late 1982 and early 1983 that earned its project manager Carlo Rubbia and chief accelerator constructor Simon van der Meer the 1984 Nobel Prize in Physics (Krige 2001: 427–428). The SSC itself was enormous, an elliptical accelerator long enough to encircle Manhattan Island (Hoddeson and Kolb 2000: 275) and with early (1984) cost estimates of $3 billion. Launched as a project in 1983, after a panel had endorsed it at the expense of Brookhaven's ISABELLE project (see Chap. 4), the SSC engaged physicists and engineers from several National Labs and universities in the technical design and the development of a scientific case (Hoddeson and Kolb 2000: 276). In 1986, a sharp proposal was presented, paving the way for presidential approval the same year, and congressional approval in 1988 (Greenberg 2001: 405). Construction at the site in Waxahachie just outside Dallas, Texas, began in 1989. In the following years, as construction proceeded, several tides turned against the project: a high-pitched debate over its usefulness and the risk that it would crowd out other, more urgent, investments for US science; a continuously rising price tag; the apparent inability of both the political realm and the institution of science to manage such a big project and the mutual suspicion bred in these two camps by the project management procedures put in place by the DOE for the project; some hubris on the side of the project's supporters, combined with a growing epistemic distance between particle physics and the rest of science; and perhaps not least, the end of the Cold War which strongly, yet perhaps indirectly, put the worth of the project in doubt (Riordan et al. 2015). After all, particle physics had been allowed to expand for 40 years mainly because of its original ties to the successful Manhattan Project and the Cold War logic of nuclear armament, but "as the Cold War wound down, (…) Washington no longer saw the relevance of expensive science to the American people" (Hoddeson and Kolb 2000: 309). The SSC advocates seem to have "misjudged the value society would place on the scientific product" of the machine, and pressed ahead although the project's cost "simply outweighed the benefits as perceived by the public" (Marburger 2014: 228). Riordan et al. (2015: 77) conclude that for non-physicists, "it was hard to grasp how a gargantuan colliding-beam machine that created fundamental but esoteric subatomic particles could have any impact on their livelihood other than to cost huge sums that might otherwise be spent in ways more closely aligned with more pressing human needs." When the costs soared, political support waned. In October 1993, the US Congress finally terminated the project.

It would be far too simplified to mark October 1993 as the point at which the era of the old Big Science ended and the transformed Big Science took over. As discussed in greater detail below, neutron scattering and synchrotron radiation had grown into important features of many national science systems already by the mid-1980s, and particle physics is still (2014) a field of science that enjoys $796.5 million of annual federal US funding and at least €943 million[3] of annual funding in Europe. But the symbolism of the downfall of the Superconducting Super Collider is important: this was the first time that particle physics did not get its next big machine funded. While still existent, the discipline has lost most of its former glorious status. As noted briefly in Chap. 1, the Cold War and post-Cold War eras should be separated by a more nuanced dividing line than the year 1989 or 1991: For science policy, for the governance and organization of Big Science, and for the transformation of Big Science in Europe and the United States, the end of the Cold War clearly played a role, by altering some fundamental framework conditions. But the contours of the post-Cold War science policy and governance system, less dominated by military spending and military connections, and more oriented towards innovation-based economic growth, grand challenges,

[3] US figure: the FY 2014 appropriation (DOE BES 2013). Europe figure: total CERN expenditure, 2014 (CERN Annual Report 2014).

and sustainability, had begun to take shape long before 1989. Not least attesting to this was the emergence and growth of neutron scattering and synchrotron radiation as experimental techniques, and as spinoffs of nuclear reactor and particle accelerator technology, as outlined below. The historical evolution of the politics of Big Science into a transformed state more attuned to the transformed Big Science is a continuous process of gradual renewal.

Neutrons "Disarmed and Commercialized"

Historian of science Thomas Kaiserfeld (2016) has summarized the development of neutron scattering research under the headline "disarmed and commercialized," and points to three important and intertwined processes in the history of the use of neutrons in scientific research. The first was the broadening of the utilization of reactors "beyond military interests and nuclear-weapons applications" to the production of neutron beams that can be used in materials science, including applications with commercial potential. Second was a drift of the global center of gravity of neutron scattering research from North America to Europe, occurring mainly in the 1970s and 1980s. And third was the emergence of new and superior techniques to complement and eventually replace reactor-based sources, namely spallation sources.

The neutron was the third basic constituent of the atom to be detected, in 1932, by the British physicist James Chadwick, a discovery that garnered him the 1935 Nobel Prize in Physics. The next year, a discovery with even greater practical implications was made, namely that the neutron could be diffracted, and thus in principle could be used as a probe to study materials (Bacon 1986b: 1). It was the R&D efforts during World War II that provided physicists with the first real opportunities to test this principle: fission reactors of various types were constructed at several sites in the United States, and the behavior of the neutrons produced in nuclear fission was meticulously studied and documented (Hewlett and Anderson 1962: 27ff). Results of the first neutron diffraction experiments were published immediately after the war (Zinn 1947), and when in 1947 the Argonne National Laboratory in Illinois was created as one of the original five National Labs, research groups led by Enrico Fermi and Donald Hughes began work with neutron diffraction using Argonne's reactors, and were soon able to show to the global physics and chemistry communities that beams of neutrons produced by a reactor could be successfully used to study the properties of materials (Mueller and Ringo 1986: 31). At Oak Ridge National Laboratory in Tennessee, similar work was launched in the late 1940s and early 1950s to determine how the nuclei of atoms diffracted and scattered neutrons and to detect the patterns of this scattering, thus mapping the atomic structure of several materials including alloys and salts, and solving puzzles such as the distribution of hydrogen atoms in common ice (Shull 1995).

Aside from the exploration of subatomic particles, which in the early 1950s had not yet developed into a Big Science in its own right, the National Labs put most of its resources at this time into reactor development and operation for both weapons development and civilian uses of nuclear energy, which meant that a plentitude of neutrons was accidentally produced and could be put to use. Bacon (1986b: 4) describes the years around 1950 as "extraordinarily fruitful," citing the publication of breakthrough results of neutron diffraction for studies of magnetism (Shull and Smart 1949) and the two seminal articles by Shull et al. (1951a, 1951b) that demonstrated the capacity of neutron diffraction to map the atomic structure of 60 elements and isotopes. Clifford Shull would, several decades later, share the 1994 Nobel Prize in Physics with Bertram Brockhouse "for pioneering contributions to the development of neutron scattering techniques for studies of condensed matter" (Berggren and Hallonsten 2012: 23). The papers published by Shull and colleagues in the late-1940s and early-1950s, writes Bacon (1986b: 4), "greatly extended the range of interest in neutrons" and as a result, in the 1950s, the auxiliary use of nuclear reactors spread across the United States and Europe, as well as India, Australia, and the Far East. In 1955, experimental research with neutrons from reactors was under way at Harwell and Cambridge in the UK; Oslo (Kjeller) in Norway; Delft in the Netherlands; Stockholm in Sweden; Risø in Denmark; Fontenay aux Roses and Saclay in France; Jülich, Karlsruhe, and Garching (outside Munich) in West Germany; Ispra in

Italy; Obninsk, Moscow, and Dubna in the Soviet Union; Swierk outside Warsaw in Poland; and of course, Argonne, Brookhaven, and Oak Ridge in the United States (Bacon 1986a: 52–119).

A convenient retrospective means of categorizing reactors is in "generations"; both by looking at the purpose for their construction and the differences in designs. The first generation is then identified as the reactors built in the United States during World War II, followed by a second generation of reactors in the early 1950s, custom built either for research, for production of fissile material, or for electric power production. It was the reactors built to study radiation effects that proved particularly useful for neutron scattering experimentation, because they were designed to achieve as high neutron flux (flow rate per unit area) as possible, and through the late 1950s and early 1960s, progress in reactor development enabled fluxes two orders of magnitude higher than those of the wartime reactors (Bacon 1986b: 5). In different ways, both the Atoms for Peace program and the Sputnik Crisis led to an increase in the number of reactors built globally, and thus although neutron scattering was still only an auxiliary activity at reactors, there was no shortage of neutrons.

When in 1957 the three National Labs at Argonne, Brookhaven, and Oak Ridge all submitted proposals to build new reactors, the AEC was initially hesitant to make the investments, but Sputnik changed the scenery dramatically, and the March 1958 congressional budget amendments for R&D (whose effect is represented by probably the steepest growth curve in Fig. 2.1 above) gave the commission the opportunity to fund two of them, at Brookhaven and Oak Ridge (Passell 1986: 121–122). The construction of these two reactors was motivated specifically by the prospects of achieving higher neutron fluxes that would enable significant performance increases in neutron scattering for analyses of materials. Aside from the AEC-sponsored facilities at the National Labs, the US Department of Commerce also funded the design and construction of the Neutron Beam Split-Core Reactor (NBSR) for neutron scattering research at the National Bureau of Standards (NBS), and many smaller research reactors were also built at universities both in the United States and Europe (Rush 2015: 137). Several international conferences in the early 1960s showed the proliferation of technologies and competence, and testified that neutron scattering had reached a position as "a well established branch of international science," with users mostly in solid-state physics and solid-state chemistry, but with a clear course set towards "crossing of the threshold into biology" (Bacon 1986b: 5). The fluxes only increased, and new reactors built in the late 1960s and 1970s had fluxes a 1000 times higher than the first-generation (wartime) reactors (Bacon 1986b: 6–9). The total number of neutron scattering users in Europe and the United States was on the level of a few 100 individuals at this time (DOE 1993: 32).

The initial lead of the United States over Europe in the postwar buildup of R&D was demonstrated in the reactor capacities in the early- to mid-1950s, but the Atoms for Peace program, which gave smaller countries access to fissionable material and know-how, helped Europe to catch up, as did of course the booming economies of most (Western) European countries beginning in the early 1950s. The reactor labs at Jülich, Garching, Saclay, and Harwell were especially prominent, and programs to allow academic users access to reactor facilities of national atomic energy programs were launched in some European countries in the mid-1960s (Bacon 1986b: 6). In the 1960s, the newly founded Organization for Economic Co-operation and Development (OECD) began investigating alternatives for a major R&D facility with some emphasis on applications-oriented research that could be funded and organized as a (Western) European collaboration. A French-German "neutron axis" had started to form, with the strongly complementary R&D programs of Heinz Maier-Leibnitz in Munich and Louis Néel in Grenoble, the former with focus on neutron instrumentation and the latter with emphasis on applications in solid-state physics as well as reactor technology R&D (Pestre 1997: 138). Another French-German axis had formed in the late 1950s and early 1960s, through the meetings between German Chancellor Adenauer and French President De Gaulle, in which both leaders expressed the conviction that a secure, peaceful, and prosperous Europe depended on close collaboration between their two countries. This alliance launched a collaborative atmosphere between France and the Federal Republic of Germany, and gave rise to concrete plans for collaborative projects, of which a high-flux reactor for neutron scattering soon became one of the more prominent (Pestre 1997: 141; Atkinson 1997: 145).

At first, a joint British-France-German organization plan was drafted in parallel with collaborative design work on a high-flux reactor for neutron scattering. The British ambivalence towards European political integration and collaboration at the time (Sharp and Shearman 1987: 79) probably contributed to its hesitance also on this issue, and the UK initially decided to build its own reactor and was hence not a founding member when the agreement to build the Institute Laue Langevin (ILL) in Grenoble was signed on January 19, 1967. The ILL reactor started operations in 1971, and experiments with the neutron beams began a year later. In 1973, the United Kingdom decided to drop the idea of a reactor facility of its own, largely for financial reasons, and join the ILL instead, becoming an equal third partner in the collaboration on July 19, 1974 (Stirling 1986: 254; Atkinson 1997: 146).

The ILL reactor was the first-ever purpose-built neutron scattering facility and brandished an innovation that was important for the user interface: the neutron beams were channeled from the reactor through tube "guides" to an adjoining experimental hall (Bacon 1986b: 6; Rush 2015: 139). The ILL thus had a global competitive advantage already on the design stage, but its subsequent success was reportedly also due to its organization. Run essentially as a service facility for external users, the ILL became a resource not only for the scientific communities of its partner countries but also the rest of Europe, which made it into the central locus of cutting-edge scientific and technological development in the area of neutron scattering in Europe (Maier-Leibnitz 1986: 137–139). ILL was built to serve a community of external users, and became a key asset in maintaining and cultivating a large neutron user community across Western Europe, which benefited both the lab and the community in the long run. Nothing similar existed in the United States, where neutron scattering facilities were auxiliary to reactors and served mostly scientists employed internally at the National Labs. In the late 1970s, the director of the Brookhaven National Lab, George Vineyard, whose High Flux Beam Reactor (HFBR) was a direct competitor to the ILL, showed clear envy when comparing the two labs, and urged the DOE to follow the example from Europe and launch "a vigorous effort to increase outside user participation" at the HFBR (Crease 2001: 45). In its first decade of operation, the ILL "grew to be almost a synonym for neutron scattering to a large proportion of the neutron community" (Bacon 1986b: 7), lining up scientific successes not least on the side of nanoscale structures, macromolecular fluids, and several other materials of specific interest for industry (Rush 2015: 140). The ILL and its ability to accommodate large numbers of users, complemented by quite successful neutron scattering programs at reactors in Jülich and Saclay, was instrumental in the shift of gravity of neutron scattering from the United States to Europe in the 1970s. A 1993 report by a special panel on neutrons commissioned by the DOE stated up front that "[o]ver the last 20 years, the United States has fallen alarmingly behind the European scientific community in the availability of up-to-date neutron sources and instrumentation" (DOE 1993: 1).

The increase of fluxes in the 1960s and 1970s and the emergence of dedicated reactor facilities were paralleled by vast improvements in instrumentation, sample handling, and data taking/analysis that "revolutionised the practical manner of carrying out neutron experiments" and brought the time frames of many measurements down from hours or days to minutes (Bacon 1986b: 7–9), but there were still fundamental limitations to what reactors could produce: The neutron beams were continuous and of relatively low flux compared to what seemed possible with another technique that gained ground in the future planning of neutron scattering in Europe and the United States in the 1970s (Kaiserfeld 2013: 29). Exploratory work had begun in North America already in the 1950s to obtain fissile material not by nuclear reactions but by knocking out neutrons from a heavy element (rich in neutrons) by the use of intense proton beams; this technique would later be known as spallation and would be exploited beginning in the late 1970s, with the most obvious gain of having neutrons emerging to experimental stations in pulses with very high fluxes in their peaks. The spallation technique had advantages and disadvantages over reactors: producing much less waste energy (heat) than reactors and not falling under the same security regulations of reactors was on the plus side, while on the minus side was operations reliability, in that spallation sources could go down in the midst of experimental runs, whereas reactors operated continuously without interruption (Lander 1986).[4] At Argonne National Lab, a turn of events in the late 1960s and 1970s released material and human resources that

[4] For a comprehensive description of the science and technology of neutron scattering and neutron-scattering facilities, see Appendix 1.

enabled the building of a spallation neutron source by the recombination of parts of older facilities (see also Chap. 4) (Westfall 2010), and the Intense Pulsed Neutron Source (IPNS) opened at Argonne in 1981. The first European spallation source, ISIS (no acronym), was built in the UK and opened in 1985 as the world's most powerful neutron source (Stirling 1986).

ISIS was largely motivated by the oversubscription (mismatch between supply and demand) of experimental time at ILL, which was especially problematic for the UK neutron scattering community (Stirling 1986: 254). A similar realization on the European stage, because of a gradual relative decline in the availability of neutrons that reflected both the growth of the user communities and the limit to the lifetimes of conventional reactors (Briggs 1986: 145), led to the identification of a growing "neutron gap" between supply and demand in the 1980s (Kaiserfeld 2013: 31). In 1986, a panel was convened on a charge by the European Commission to study alternatives for future European neutron sources, and in its 1990 report, this panel recommended a detailed design study of a spallation source. On the initiative of the Rutherford Appleton Lab in the UK, where ISIS is located, and the *Forschungszentrum Jülich* (Research Center Jülich) in Germany, a number of workshops and meetings were held in 1991 and 1992 leading up to the launch of a collaborative ESS Council to promote and develop a concept for a European Spallation Source (ESS) (Kaiserfeld 2013: 29).

Among the available explanations for the relative decline of neutron scattering in the United States in the 1970s and onward, the competition among National Laboratories is occasionally cited, and it has also been claimed that the DOE and its Basic Energy Sciences (BES) branch rather consistently prioritized synchrotron radiation over neutron scattering in the 1970s and 1980s (Rush 2015: 141–142). A major new neutron facility, what was eventually to become the Spallation Neutron Source (SNS) at the Oak Ridge National Lab, was planned already in the 1980s but did not get construction funding approved until the late 1990s (see below) (Rush 2015: 144–147). Historian Bob Crease (2001: 51) refers to the situation in the US in the 1980s as "the neutron crisis"; studies and panels in the late 1970s and early 1980s had recommended investments but were largely ignored, and thus "although the U.S. made the first major contributions to neutron scattering research, it had fallen far behind the rest of the industrialized world," spending less on the field than West Germany, France, and the UK, separately (Crease 2001: 51). Here it is interesting to note that it was in Europe and not the United States that efforts were intensified in the 1990s to take the envisioned leap in performance that a state-of-the-art spallation source would mean. In May 1994, European neutron users began to design and lobby for the ESS project through the European Neutron Scattering Association (ENSA) founded the same year as an umbrella organization for national user associations, and in 1997 the first comprehensive ESS proposal was drafted. Kaiserfeld (2016) has highlighted the emphasis of the proposal on the commercial applications of neutron scattering as the "primary support for the realization of the facility." The United States seems instead to have been geared towards synchrotron radiation and free electron laser technology (see next section), at the expense of neutron scattering.

The OECD Megascience Forum, a platform for discussion among OECD member countries,[5] had convened a working group on neutron sources in the mid-1990s, and in 1998 this group proposed to its principals that one neutron spallation source each should be built in Europe, the USA, and Japan, respectively, to ensure that "the vast majority of scientists in need of neutrons would have access to a quality facility" (Kaiserfeld 2013: 34). In May 1999, the OECD ministerial conference endorsed the proposal, and so while there were strong scientific foundations of these facility proposals in both the USA and Europe, with highly qualified user communities ready to start using them once they became operational, it must be noted that this endorsement was a top-down initiative on a global scale. The DOE had already set the SNS on track, funding R&D work and conceptual design activities for the facility in 1996 "with the preferred alternative site being Oak Ridge National Laboratory" (DOE BES 1996: 1), but the OECD endorsement of 1999 was nonetheless a showing of top-down initiative that also may have influenced the final congressional decision in late 1999 to fund the construction of the SNS, at an estimated cost of nearly $2 billion, making

[5] Named the Megascience Forum for different reasons than, and completely independent of, the use of the term "megascience" by Hoddeson et al. (2008) and in other sections and chapters of this book.

it the most expensive new facility project in the post-Super Collider era (Rush 2015: 149). It has been claimed, however, that despite the strong effort to build a state-of-the-art pulsed neutron source at Oak Ridge, the United States still has a diminished position in international neutron scattering research. The former "mainstays of American neutron science" Brookhaven and Argonne National Laboratories are no longer active participants in the field, with the exception of some small user groups. Given the cost and scope of the SNS, it will most likely remain the only major DOE-funded neutron source for some time to come (Rush 2015: 151). There is some evidence to suggest that of all the projects in the Trivelpiece Plan launched to keep some National Labs alive by parceling out major facility projects among them (see later in this chapter), aiming the neutron source for Oak Ridge was the least perfect match given the comparable lack of accelerator expertise there. In this case, perhaps, preservation of the lab trumped scientific concerns.

Europe, it seems, is on track to continue and reinforce its lead, although many hurdles remain. In the summer of 2014, 13 European countries jointly announced that they had agreed on the funding of the European Spallation Source (ESS) to be built in Lund in Southern Sweden, and later the same year, ground was broken for the facility (Hallonsten 2015b: 417). In 2015, legally binding documents for the funding and organization of the facility were signed, and the ESS is expected to start delivering neutron beams in 2019, at an estimated total construction cost "not exceeding €1843 million in January 2013 prices" (European Commission 2015), corresponding to roughly $2 billion.

Clearly, the organizational field of neutron scattering facilities has not expanded as evidently linear (or exponential) as that of synchrotron radiation facilities (see next section), and some fundamental technical and scientific differences are probably behind. First, reactors and spallation sources have historically been both more expensive and, due to safety regulations, nuclear non-proliferation treaties, and the military secrecy around some facilities, less accessible to "parasites" (Hallonsten 2015a, see below) than have the accelerators used for synchrotron radiation research. Second, research with synchrotron radiation is essentially a continuation, a recent step, in the use of x-rays and other radiation for spectroscopic, crystallographic, and imaging studies that started with the work of Bragg and Bragg (1913) (father and son) and Friedrich et al. (1913), and that could be carried over to synchrotron radiation facilities—with vastly improved performances—once these facilities became available. Neutron scattering experimentation has a similar history but required large-scale equipment, and most of all reactors, also for its early exploitation. But there are also similarities: the routinization of experimental work, and the continuous lowering of thresholds for users with no experience of large-scale equipment, have been instrumental in broadening the user bases beyond the smaller groups of enthusiasts and to wider layers of the scientific communities in physics, chemistry and the life sciences. A reciprocal evolution of supply and demand seems to have characterized the proliferation of both these technologies across the sciences (Hallonsten and Heinze 2015; Kaiserfeld 2016). In the case of neutron scattering, the alliance with applied or strategic R&D and direct industrial interests has possibly been more important, propelling the evolution of this experimental technique to "disarmed," and later "commercialized." Granberg (2012: 116ff), in his review of the capacity of the Swedish government's proposal to host the ESS in Lund in Southern Sweden, concludes that the rhetoric surrounding the ESS facility and the main arguments for its realization were broadly oriented to a multitude of fields with close connection to all kinds of practical and commercial applications (see also Chap. 6).

Synchrotron Radiation "From Esoteric Endeavor to Mainstream Activity"

The experimental use of synchrotron radiation is historically/causally tied to the discipline of particle physics in the same way that the experimental use of neutron scattering is tied to nuclear energy research: synchrotron radiation was originally a waste product in accelerators for particle physics, something that its constructors attempted to minimize because it hampered the performance of the machines. An accelerated elementary particle whose trajectory is bent (like an electron in a round-shaped accelerator) will unavoidably lose energy in the shape of high-brightness radiation that emerges from the particle trajectory in the tangential direction. This phenomenon has been theoretically known since the work of James Clerk Maxwell, who described it as part of his famous 1861/1862 equations (Blewett 1998: 135). For particle physicists, eager to increase the energy level of the particle bunches they lead to collisions, synchrotron radiation was thus a nuisance and something meticulously recorded and documented as part of accelerator technology development. In 1947, synchrotron radiation was first observed, "as a small spot of brilliant white light by an observer looking into the vacuum tube tangent to the orbit and toward the approaching electrons" in a small synchrotron operated at the General Electric Research Laboratory in Schenectady, New York (Elder et al. 1947). It is because of the fact that the radiation was first observed as emerging from a synchrotron—the state-of-the-art accelerator type of the time and for some years to come—that the radiation bears the name "synchrotron radiation," although nowadays, in strict terms, the name is erroneous, since all experimental work done with the technique uses radiation from storage rings, another type of accelerator design that emerged in the 1960s, and whose exploitation in the early- to mid-1970s also meant the first breakthrough of research using synchrotron radiation (see below). The name has stuck, and has also given rise to the contemporary habit of referring to synchrotron radiation laboratories and accelerator complexes used for research with synchrotron radiation as "synchrotrons," which of course is even more inaccurate but nowadays commonplace.

After the radiation had been visually detected in 1947, further studies of its characteristics could be conducted and further confirmations obtained for the previously merely theoretical predictions that this radiation would be useful as a tool for spectroscopic and crystallographic measurements and experiments (Winick and Bienenstock 1978: 39–41). Studies showed that the synchrotron radiation produced by the electron accelerators in operation in the 1950s consisted of a continuous spectrum of infrared radiation, visible light, ultraviolet radiation, and x-rays of unprecedented intensity. Thus, by the early 1960s, the quality and character of the radiation was well-known, and technical advances on optics and vacuum technology enabled some initial trials exploiting synchrotron radiation for studies of materials within solid-state physics.

The synchrotron radiation program at DESY in Hamburg started in 1964, with the help of a grant from the German *Wissenschaftsrat* (Science Council) obtained by DESY research director Peter Stähelin, and by the use of the radiation emitted by the DESY synchrotron, which was officially inaugurated as the Federal Republic of Germany's new flagship particle physics machine only months after the first synchrotron radiation research there (Heinze et al. 2015: 461–462). These exploratory studies at DESY joined several other similar efforts in the 1960s in breaking new ground in spectroscopic studies of matter with synchrotron radiation, most prominently at the Frascati synchrotron in Italy and the NBS in Washington, DC (Munro 1996: 132; Hallonsten and Heinze 2015: 844). Several technical challenges stood in the way of any significant scientific contributions with the use of the radiation: First of all, accelerators were generally owned and operated by particle physicists and access was only made available at their generosity. In addition, if access was granted, the calibration of the accelerators for optimal production of synchrotron radiation did not typically cohere with the interests of the particle physicists, and synchrotrons also had a limited particle beam lifetime, which made the radiation emerge only in very short flashes. It was only with the emergence of the concept of storage rings—which keep electron bunches circulating for hours, and thus would (theoretically) emit continuous beams of radiation—that prospects improved. Also, synchrotrons could not emit radiation in the sought-after x-ray range (only ultraviolet), but chances were good that the storage rings under construction foremost at DESY and SLAC in the late 1960s and early 1970s could also supply x-rays[6] (Hallonsten 2015a: 228; Heinze et al. 2015: 455).

The growth of synchrotron radiation from a small-scale lab curiosity to a widely used experimental tool for the natural and technical sciences, as well as medicine and some applications in cultural studies, has been analyzed and conceptualized as occurring through a combination of organizational and institutional change in within organizations and science systems, and a complex interaction of scientific and technical development in disciplinary communities (Hallonsten and Heinze 2015).

[6] For comprehensive descriptions of the science and technology of synchrotron radiation, see Appendix 1.

In the initial period of emergence and formation of the new organizational field of synchrotron radiation laboratories, the lion's share of facilities were launched within preexisting research organizations, and it was not until several later facilities were launched outside of preexisting structures, that the formation of the field gained momentum and expanded beyond its initial, peripheral status. The real expansion occurred only after a consolidation of best practices on the technical and organizational side, and in later stages of the expansion, direct imitation between labs is also visible. Several different processes of change, both on the system level and on the level of individual organizations, contributed to the overall expansion of the field.

The first real technical breakthrough in the development of synchrotron radiation from "esoteric endeavor to mainstream activity" (for the origins of this quote, see below) was the advent of the storage ring accelerator design. Developments of the storage ring concept were mostly pioneered at Stanford University in the 1960s, by the work of Burton Richter, Gerard O'Neill, and W. C. Barber, who "first had the courage" to go from idea to actual design and construction of storage rings for particle physics experimentation (Panofsky 2007: 56), and whose ideas were adopted by the newly created SLAC on the hillside of Stanford University and turned into the lab's next project (Hallonsten 2015a: 227). The SPEAR machine (Stanford Positron Electron Accelerator Ring) was taken into operation at SLAC in 1972, and made available a broad spectrum of synchrotron radiation stretching well into the x-ray region. A group of Stanford professors stood ready to make use of the radiation with the help of a grant from the NSF (see Chap. 4) (Hallonsten 2015a: 234–235). In 1974, the storage ring DORIS (*Doppel-Ring Speicher*, Double-Ring Storer) at DESY in Hamburg, very similar to SPEAR, opened and provided the DESY synchrotron radiation program with new opportunities that were quick to be exploited. At SLAC and DESY, pioneering work in x-ray spectroscopy, crystallography, and microscopy was conducted that showed the capacity of x-rays from storage rings and proved the viability of synchrotron radiation as an experimental technique for several natural sciences disciplines. Both at DESY and SLAC, the activities expanded gradually but comparably swiftly in the 1970s, partly through the creation of alliances with external user groups and institutes who brought their specialist competences to the labs and achieved some quite remarkable results (Hallonsten 2015a: 238–240; Heinze et al. 2015: 478ff). In the same decade, plans for synchrotron radiation facilities were drafted at Brookhaven National Laboratory (see Chap. 4), Cornell University in New York, Daresbury in the UK, Lund University in Sweden, West Berlin, and the University of Wisconsin (Hallonsten and Heinze 2015: 843). At several of these places, the champions of synchrotron radiation took over machines deserted by particle physicists in the transformation of the discipline towards "megascience," and at others, the activities remained "parasitic" (Hallonsten 2015a); particle physics was still the star of most shows at this time. At Brookhaven, a truly multidisciplinary National Lab yet with some prominence of the particle physics program, planning took off for a dedicated, non-parasitic synchrotron radiation lab in the mid 1970s. Brookhaven historian Bob Crease (2008a: 439) has described the process as "an arduous, even traumatic experience" for the lab: the National Synchrotron Light Source (NSLS) project had to compete with the particle physics machine ISABELLE under planning and construction at the time, and therefore became both underfunded and internally under-prioritized.

A second major technical breakthrough for synchrotron radiation came with the late 1970s invention of so-called "insertion devices" that make the electrons in a storage ring oscillate up and down or left and right and thus emit radiation in a much more efficient and manageable way than at the curved sections of the accelerators (see Appendix 1). Since the mid-1980s, insertion devices are the preferred technology for synchrotron radiation production, and since the 1990s, all synchrotron radiation facilities built are designed to make optimal use of insertion devices (Hallonsten and Heinze 2015: 844). But despite these developments, almost all synchrotron radiation programs in Europe and the United States were parasitic until the mid-1980s, and research groups often struggled hard to get the radiation out of the machines properly without interfering (too much) with their particle physics landlords, and to produce scientific results on the level of what they knew was theoretically possible with the infrastructure they had (limited) access to (see e.g. Hallonsten 2015a; Heinze et al. 2015). Synchrotron radiation was still seen as mostly an esoteric phenomenon and a peripheral experimental tool, but the potential was recognized, and as the 1980s began, efforts were under way both in Europe and the United States to construct new, dedicated machines.

Synchrotron radiation sources of various types can be neatly categorized as first, second, and third generation, with the first generation being the parasitic programs of the 1960s and 1970s (e.g. at DESY and SLAC), the second generation being the early dedicated sources that did not make use of insertion devices (e.g. NSLS, SRS at Daresbury, and the early MAX-lab in Lund), and the third generation being the purpose-built facilities in the early 1990s and on that were designed to make full use of insertion devices (Hallonsten and Heinze 2015: 845). By the opening of several third generation labs beginning in the early 1990s, most prominently the European Synchrotron Radiation Facility (ESRF) in Grenoble and the Advanced Photon Source (APS) at Argonne (see below and Chap. 4), the true expansion of the organizational field of synchrotron radiation laboratories in Europe and the United States was realized (Hallonsten and Heinze 2015: 845). The "mission crises" of some US National Labs seem to have provided especially good breeding ground for some initiatives (Westfall 2008a; 2008b; 2012), and in Europe, the dismantling of national accelerator construction programs after CERN's monopolization of particle physics budgets in the late 1970s (see below) and the renewed collaborative spirit in European science in the same period allowed national and collaborative intergovernmental initiatives to flourish (Hallonsten 2011; 2014a). The scientific opportunities opened up by the technical improvements on the side of insertion device technology and the construction of very large storage rings (ESRF and APS) led to breakthroughs in several disciplines as well as a broadening of the user base of synchrotron radiation facilities. The expansion of use in the life sciences was particularly significant at these large labs, which were optimized for shorter wavelengths (so called "hard" x-rays[7]) that are especially useful for the structural determination of bio-macromolecules, an application that boomed in the 1990s (see Appendix 1).

[7] "Hard" x-rays have a shorter wavelength and a greater penetrating power than "soft" x-rays, hence these colloquial names, but in the 1980s and early 1990s the production of "hard" x-rays also required special technologies and most of all, very large storage rings. Towards the end of the 1990s, improvements of several technologies led to innovations that enabled the production of hard x-rays without these very large rings (see below). For further information on these technologies and the science they enable, see Appendix 1.

The ESRF and APS were built to accommodate 1000s of users annually, with optimized experimentation and data-taking that enabled mainstream operation and high reliability of instruments, which was also especially important for life science users. Several Nobel prizes in chemistry have been awarded for discoveries relying heavily on work done with synchrotron radiation,[8] and the numbers of synchrotron radiation users worldwide has multiplied since the mid-1990s. At the ESRF, for which user figures are both available and reliable, the number of annual users (individuals) increased fivefold in only five years, from a little over 1000 in 1995 to over 5000 in the year 2000, and has oscillated at 4000–6000 since then (Hallonsten 2013a: 505). At smaller labs, such as the Advanced Light Source (ALS) at Lawrence Berkeley National Lab, the Elettra Sincrotrone Trieste in Italy, and the expanded MAX-lab in Lund, Sweden, the somewhat longer wavelength spectrum was successfully exploited as a niche of its own (Hallonsten 2011: 199; Hallonsten and Heinze 2015: 845).

Technical improvements have been achieved on nearly all parts of synchrotron radiation facilities at a continuous pace since the 1970s. Most importantly, in the late-1990s a new type of third-generation synchrotron radiation source design emerged, usually called the "intermediary energy source," which combines the capacity of the very large labs ESRF and APS with the smaller ones like ALS and Elettra, and are able to produce high-quality radiation in the full spectrum from "hard" x-rays down to infrared without the necessity of building rings of several 100 meters in circumference, thus significantly lowering the overall cost. A number of such facilities have been taken into operation in Europe and the United States since the early 2000s, with the National Synchrotron Light Source II (NSLS-II) at Brookhaven, the upgrade of SPEAR at SLAC, the Diamond (no acronym) that replaced the SRS in Daresbury, UK, and the MAX IV facility in Lund as prominent examples (Hallonsten and Heinze 2015: 843). These intermediary energy sources not only signal a consolidation on the technical side of synchrotron radiation laboratories but also testify to a kind of saturation of best practices for accelerator and instrument operation, user support and user access (including peer review of proposals for experiments),

[8] John Walker (received the prize in 1997), Roderick MacKinnon (2003), Roger Kornberg (2006), Ada Yonath, Thomas Steitz, and Venkatraman Ramakrishnan (2009), and Robert Lefkowitz and Brian Kobilka (2012).

data management, and the continuous upgrades and expansions of existing lab resources. But while there is consolidation or saturation in these respects, it is clear that the organizational field of synchrotron radiation labs is still undergoing expansion. New labs have been built at a constant pace since the 1980s, but few older labs have been closed, and so there are at least 20 synchrotron radiation facilities in operation in Europe and the United States as of 2015, depending on the count (see Appendix 2). In a 1997 review of the federally funded synchrotron radiation laboratories in the United States, the development of the previous two to three decades within the area was summarized thus:

> 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. (Birgeneu and Shen 1997: 7)

The current late expansion phase of the organizational field of synchrotron radiation laboratories contain some important trends for the future. First, several additional large accelerators for particle physics have been deserted in recent years as part of the internal development of that field as well as organizational and political changes, and some of these (most notably PETRA, *Proton Elektron Tandem Ringbeschleuniger Anlage*, Positron-Electron Tandem Ring Accelerator Facility, at DESY) have been rebuilt into a new type of synchrotron radiation sources that deliver radiation at the edge of what is theoretically possible in terms of quality. Second, ambitious upgrades of the two very big facilities APS and the ESRF have been launched to keep these labs competitive after the opening of several state-of-the art intermediary energy sources. Third, the use of free electron lasers has emerged, a new type of radiation sources that are sometimes called the fourth generation but that are not really synchrotron radiation sources—instead, they are a rather radical refinement of some of the extreme performance parameters of synchrotron radiation by the use of linear accelerators and very long insertion devices to produce lasers in the x-ray range, to be used in exceptionally performance-intensive experiments (see Appendix 1) (Hallonsten and Heinze 2015: 846ff).

At the same time, it seems the development of storage ring technology is slowly approaching its limits, although the consolidation of the current design of storage rings makes it likely that they "remain the workhorses of synchrotron radiation science for many years to come" (Barletta and Winick 2003: 8), because of their breadth and their stability. Free electron lasers will therefore not replace storage ring-based synchrotron radiation sources but instead offer specialized experimental opportunities not available at regular synchrotron radiation facilities. Free electron lasers first emerged as a technical concept in the late 1970s (see Winick and Bienenstock 1978: 57; Pellegrini 1980: 717), but stayed on the theoretical level until the mid-1990s when the technology matured and became practically feasible. In the early 2000s, the first free electron laser user facilities were planned and constructed, with the FLASH (Free Electron Laser Hamburg) facility in Hamburg opening to users in 2005, followed by the Linac Coherent Light Source (LCLS) at SLAC in 2009 (see Chap. 4), and the FERMI (Free Electron Laser Radiation for Multidisciplinary Investigations) facility at Elettra in Trieste, Italy, in 2010. The European X-ray Free Electron Laser (XFEL) facility in Hamburg is one of the two major European collaborative Big Science facilities currently under construction, with the spallation neutron source ESS being the other.

The Politics of Big Science

Several of the US National Laboratories experienced deep mission crises in the 1970s and 1980s as particle physics transformed into megascience and no new large accelerators were built outside the two single mission particle physics labs SLAC and Fermilab. A similar development occurred in Europe, though with a slightly different logic due to the dual-level politics of science and Big Science in Europe, when CERN (and DESY) monopolized European funding for particle physics research, and many smaller countries had to close their domestic facilities and redirect funding to these two hotspots (see below) (Martin and Irvine 1984: 188; Herman 1986: 132). In the USA, the National Labs at Argonne, Berkeley, Brookhaven and Oak Ridge managed to stay alive through the 1970s by efforts in recombining material, monetary, and human resources, and retooling their lab organizations for new purposes. These four labs, however, as will be discussed in greater detail in Chap. 4, were all in states of deep crisis with respect to their mission as the 1980s began and the Superconducting Super Collider rose to its position as the next big thing in the National Labs system.

Alvin Trivelpiece, a physics professor at the University of Maryland and the director of the Office of Energy Research of the DOE in 1981–1987 (later director of Oak Ridge National Lab, 1989–2000), is usually credited as the author of the plan that made it to a high-level political deal struck in 1984–1985 between the DOE and the "Gang of Four laboratory directors" of Argonne, Berkeley, Brookhaven and Oak Ridge to save these four labs. Trivelpiece writes in a short memoir in the fall 2005 issue of the *History of Physics Newsletter* of the American Physical Society that he was "very distressed" by the realization that "if we kept on the present trajectory we would end up with a collection of laboratories and facilities at which everything was secure, everything was cleaned up, and no body was doing anything useful" (Trivelpiece 2005: 14). The plan, whether the result of a conspiracy between the Gang of Four lab directors or indeed a master plan of Trivelpiece himself, was "commensurate with the culture of the DOE in balancing capabilities at the National Labs" (Rush 2015: 145), and although it is claimed that the real reason for devising the plan was to secure the future for the SSC (which, in the long run, did not succeed), it produced a long-term pattern, a "blueprint" for how new DOE projects are realized (Westfall 2008b: 606–607). When in 1993 the Superconducting Super Collider (SSC) was cancelled, three-quarters of the Trivelpiece plan had already been launched: the Relativistic Heavy Ion Collider (RHIC) at Brookhaven, the Advanced Light Source (ALS) at Berkeley, and the Advanced Photon Source (APS) at Argonne, the latter two of which were state-of-the-art synchrotron radiation facilities with complementary performance targets and scientific niches (see Chap. 4). The fourth facility in the plan, the reactor-based neutron source aimed for Oak Ridge, had been delayed, and as time passed, it was also substituted for a spallation neutron source (the SNS) that was eventually built with much help from several other laboratories (see below) (Westfall 2010: 392–393).

Figure 2.2 shows a diagram with data from the DOE budget requests to Congress for 30 years, 1984–2014, on capital investment within the Basic Energy Sciences (BES) division, which is in charge of the neutron scattering, synchrotron radiation, and free electron laser facilities within the National Laboratories system. The diagram shows not requests but actual expenditures, and has been edited graphically to display the long-term outcome of the Trivelpiece Plan by identifying projects and their investment cost in time series. Several patterns emerge. The SNS, the biggest project of all in the time period concerned (except for the SSC, which was not a BES project), was not initiated until the ALS and APS had been completed, and the free electron laser LCLS at SLAC could not start con-

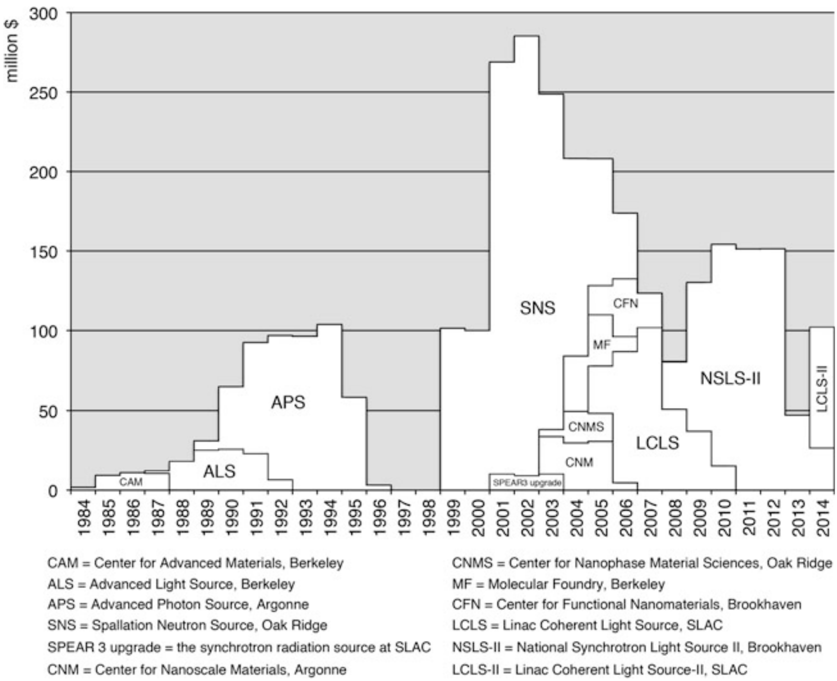

Fig. 2.2 Time series of capital investment (actual appropriations) within Basic Energy Sciences (BES) in the DOE appropriations bills, 1984–2014, with major projects identified

struction until funding for the SNS began to ramp down in 2004–2005. In the meantime, several minor projects were carried through in what seems to have been a window of opportunity between the major SNS and LCLS projects: the Center for Nanoscale Materials (CNM) at Argonne, the Center for Nanophase Material Sciences (CNMS) at Oak Ridge, the Molecular Foundry (MF) at Berkeley, and the Center for Functional Nanomaterials (CFN) at Brookhaven.

Overall, it is quite clear that projects at the four labs included in the Trivelpiece Plan, plus the LCLS (and LCLS-II) at SLAC, have succeeded each other in the DOE budget, and that labs are awarded major projects according to some formula-like queue; major investments at the same lab seldom overlap. After the APS, there is a short break in the history (which is likely due to the construction of no less than three major nuclear physics/high energy physics projects within the DOE budget but outside the BES budget and thus not shown in the figure), but from the SNS and on, the big ones succeed each other in the BES budget. When SNS wound down in 2004–2005, the LCLS ramped up. When the LCLS started to wind down in 2008, the NSLS-II was next in line, and once it wound down in 2013–2014, the LCLS-II ramped up. Minor projects such as ALS, the SPEAR3 upgrade, and the nano tech centers (CNM, CNMS, MF, and CFN) were initiated and concluded during other commitments, although it is clear that these were not initiated until the SNS had passed its 2002 peak.

There is nothing to suggest that Alvin Trivelpiece or the other officials at the DOE at the time of the launch of the plan in the mid-1980s intended for it to become policy doctrine lasting several decades. But with the benefit of retrospective analysis, Fig. 2.2 makes it clear that something like a "Trivelpiece legacy" has been institutionalized in US Big Science policymaking and serves as a basic element of the "ecosystem" (see Chap. 4) where the transformed Big Science in the United States emerged and grew to prominence. When looking at the diagram in Fig. 2.2 and comparing it to the European situation, it seems the contrast could hardly be greater. Overstretching the argument somewhat, Big Science in the United States follows a clear plan whereby National Laboratories are kept alive by being granted new big facility projects in succession. Europe is a much messier affair, both because there are two levels of European Big

Science, the national and the collaborative, and because of the unordered state of the collaborative arena. But Europe has its advantages as well.

Big Science in Europe began with CERN, the first European intergovernmental collaboration in science and perhaps the most visible manifestation of the "Marshall Plan for Science" (Krige 2003: 902); "a coproduced instrument of European and American political interests in the early Cold War" (Krige 2006: 57). Located in Geneva, Switzerland, CERN is an international treaty organization launched in 1954 and quite generously funded. It was originally a means for Europe to counter "brain drain" and start building capacity to compete with the USA (on US terms, of course) in the increasingly important field of nuclear research. In late 1959, CERN's first big machine the Proton Synchrotron (PS) started operation and took over the position of global leader in particle physics research (Pestre and Krige 1992: 80). Written into the founding documents of CERN was the principle that the lab would not compete with national efforts in particle physics or other fields, and by 1960, accelerator labs for particle physics were in operation or under planning in Britain, France, Italy, Germany, and Sweden (Irvine and Martin 1984b: 185), complementing CERN and in many cases collaborating synergistically with CERN. But the radical increase in the size and cost of the accelerator complexes that would be needed to keep up with international competition and megascience soon changed the setting by forcing dramatic increases of the CERN budget, partially at the expense of the member countries' domestic particle physics programs, which led several of them to reevaluate their commitment and change their attitude towards European collaborations in Big Science. The next big machine planned at CERN in the early 1960s was the Super Proton Synchrotron (SPS), large and costly enough[9] to suggest that a separate lab—soon called CERN II—be built to host it. The size made it very attractive for all member states to seek to host (cf. the discussion of political pork in Chap. 1), as it would bring in vast amounts of foreign capital and highly skilled labor. Several countries made their own site proposals, and the

[9] At the height of SPS construction, in 1974, the CERN budget was nearly six times higher than in 1964, when the SPS was only in planning stages and the PS machine was the lab's flagship (CERN Annual Reports).

Federal Republic of Germany and the United Kingdom went as far as issuing ultimatums that unless the new lab be located on their territory, they would withdraw completely from the collaboration (Hallonsten 2014a: 36). The situation was only resolved by the decision to build the new lab at the existing site in Geneva and make it part of the existing CERN (Pestre 1996: 73, 77–78). But some countries remained hesitant to support the vast cost increases that CERN II would bring, most notably West Germany, Sweden, and the UK, and yielded only after strong diplomatic pressure (Krige 2003: 905). In 1972, all countries had joined, and the project was on secure financial ground. In 1976, the SPS machine reached its target performance.

The balance between national interest and common good had been a parameter of some importance also when CERN was first created, in the mid-1950s, but with CERN II, it was raised to another level. European collaboration in Big Science "is not undertaken at the expense of self interest; it is rather, the pursuit of one's interests by other means" (Krige 2003: 900). This balance and its practical complications constitute one of two red threads that run through the whole history of the politics of European collaborative Big Science to this day. The other is a mirroring of overall cycles of the politics of European integration (such as UK-France strains in the 1960s, the French-German axis in the 1970s and 1980s, and the chilly atmosphere between Russia and Western Europe in the 2000s) which ultimately depend on the lack of a policy framework for science within the European Union that makes collaborations vulnerable to all kinds of political shifts (Hallonsten 2014a). Ever since the settling of the CERN II issue, European governments have guarded their national interests heavily in Big Science collaborations, fighting to become the hosts of facilities and implementing formulas and regulations for return on investment (both monetary and intellectual), and trying to come off as cheaply as possible from negotiations over funding shares. High-level politics, often completely detached from the subject of Big Science collaborations, have often directly decided their fate. The European Southern Observatory (ESO) was delayed several years because of British "euroskepticism" and strains in the relations not least between France and the UK in the early 1960s (Hallonsten 2014a: 37). The very successful ILL was originally a trilateral project, but Britain's participation was postponed largely because of the same "euroskepticism," which faded somewhat in the early 1970s to allow joining of both the ILL and the European Community (1973) (Hallonsten 2014a: 38). Several authors have noted that the emerging new collaborative spirit in the relations between France and Germany was instrumental for the creation of the ILL (Sharp and Shearman 1987: 79; Pestre 1997: 141; Atkinson 1997: 145), and this French-German axis only grew stronger in the 1970s and became the new "motor of Europe" that eventually drove the EC collaboration towards today's European Union (Middlemas 1995). Although, as noted above, the EC/EU collaboration had no mandate in science and technology (until the launch of the Framework Programmes, which were mostly on the side of applied R&D), the French-German axis and the renewed Europeanism spilled over to science and led to the creation of several collaborations in the 1970s and beyond,[10] including not least the ESRF which is the perhaps most successful European collaborative science project after CERN. It is today considered the world's leading synchrotron radiation facility (Hallonsten 2013a), and it was born out of the first wave of expansion of synchrotron radiation in the mid-to-late 1970s and multilateral discussions between the (Western) European scientific communities, to create a very large lab with capabilities both complementary to and superseding ongoing national efforts in synchrotron radiation (Hallonsten 2014a: 39). With little or no precedent, the political process of creating the ESRF had to rely on ad-hoc mobilization of scientific interest and political will, and no real progress was made on the political side until in 1984 France and Germany jointly declared their decision to build the ESRF in Grenoble. By then, a site-selection procedure had been ongoing for some time, with several countries as contenders for hosting the lab and serious attempts to make objective evaluations of the site proposals. But since the issue of site was tied to the issue of funding for the lab, no real progress was reported until the 1984 announcement by the governments of France and Germany that they had agreed to construct the ESRF in Grenoble and together fund at least 50% of the construction, inviting other countries to join. The announcement

[10] The European Science Foundation (ESF) in 1973, the European Molecular Biology Laboratory (EMBL) in 1973, the European Space Agency (ESA) in 1975, and the fusion research center Joint European Torus (JET) in 1977, to name a few (Herman 1986: 150–159; Krige 2003: 899).

caused both surprise and antipathy among the other prospective member countries, especially those with their own site proposals, but was probably decisive for the realization of the project. Within some months, most countries accepted the proposal and joined the collaboration. What is clear is that the ESRF was part of a package deal between France and Germany, by which a collaborative cryogenic wind tunnel for aerospace technology was also to be located in Cologne (Papon 2004: 64). After the 1984 announcement, a two-track development plan ensued, comprised of the scientific case and the technical design on one hand, and political negotiations over funding shares and the organization of the future lab on the other. The scientific and technical work took place in collaboration between institutes and universities in France, Italy, Spain, West Germany, and the UK, and the negotiations over funding shares followed a different path with a different logic, predominantly behind closed doors and with rules made up as negotiations proceeded.

As noted, the EC/EU and its predecessors had no mandate in (basic) science and have not been an active organizer of any collaborative R&D efforts until just recently. Thus, while the (Western) European countries have been quite successful with CERN, ESO, ILL, ESRF, and a handful of other projects, and have maintained a competitive position in international science and technology by the organization of joint efforts built on intergovernmental agreement, this has all been achieved without any coherent policy framework and completely outside the EC/EU structure. According to some analysts, this has contributed to the success of several of the collaborations by shielding them from EU bureaucracy and by letting the scientific communities and the science policy systems of Europe optimize every specific project organizationally, scientifically, and technologically at any given time (Hallonsten 2014a: 35; Hoerber 2009: 410; Gaubert and Lebeau 2009: 38; Papon 2004). But the lack of precedent and a coherent policy framework has also caused significant delays of several projects and made the policy field utterly complex, as all collaborations have had to rely on ad-hoc solutions and the ruling circumstances of the politics of European integration and national governments at the specific times of their conception. Organizational forms and legal frameworks differ vastly between different projects (they may be international treaty organizations, like CERN, or private companies, like the ESRF).

The procedures for launching and promoting projects, and the bilateral negotiations over funding and organization, are difficult to follow. Some common pitfalls seem almost impossible to avoid, and the eventual shares of construction and operation costs may reflect negotiating skills of countries' delegations more than a fair distribution of shares, but they may nonetheless remain in place for decades with little or no opportunity for readjustment. The unpredictability of negotiations may also delay realization of projects significantly, and the fact that they take place outside established frameworks makes it hard to reveal the reasons for such delays and other details of the negotiations. What is clear, though, is that the lack of established structures for these procedures mirror the cycles of European political integration (for thorough discussions on this topic, see Hallonsten 2013b; 2014a; 2015b). The two most recent European collaborative efforts in transformed Big Science are the XFEL in Hamburg and the ESS in Lund, Sweden, both of which are under construction with slightly different timeframes. Both projects have experienced delays and difficulties arising from stalled negotiations, although in different ways: the XFEL was an all-German project to which other countries were invited to join, which created difficulties of collecting financial contributions from prospective member countries that were only really resolved by a major contribution from Russia that saved the project but also altered its governance structures significantly (Hallonsten 2014a: 40). The ESS was first suggested in the early 1990s and it took almost a quarter of a century before a funding solution was reached and ground was broken for the lab; the main reason seems to have been indecision on behalf of the larger countries (Hallonsten 2015b: 417). Interesting to note, however, is that while the procedures for reaching agreements on the European level are thorny and time-consuming, once agreements are made there is a predictability and reliability in Europe that the US system lacks almost completely. While a binding agreement between European countries for constructing and operating a large joint scientific facility establishes the level of funding for the construction and operations phase of the project from which the participating governments seldom or never back off, in the United States the federal budget is set (and negotiated) on a year-to-year basis, which means that there is almost no predictability at all. Should political majorities change in Washington, construction and operations budgets can be slashed with the stroke of a pen, or a stalemate in Congress can produce a so-called continuing resolution instead of a budget, or, worst case, a government shutdown (as, most recently, in the first half of October, 2013), all of which can have devastating effects of the construction and operation of user facilities, and which also create inefficiencies by making long-term planning difficult.[11] Clearly, there are also comparable advantages of the seemingly very tiresome and unorganized state of European collaborative Big Science.

In addition to the trouble of site selection, which is closely tied to funding, the balance or tension between national interest and common good has acted out in some specific ways, most of all connected to the widespread expectations of (socio-)economic benefits for the country and region that gets to host an international research facility (see also Chap. 6), and of course also to the expectations of scientific benefits for the local research communities. A mechanism devised to distribute some of the host country's economic benefits among the other member countries is the principle of Fair Return, usually applied on procurement but sometimes also implemented (in modified form) to balance the use of facilities with the respective countries' relative contributions to operations costs. Fair Return on procurement was invented at CERN and is used at all the European collaborative facilities discussed here, with small variations but generally organized so that the share of procurement and service contracts (sometimes including hiring of staff) awarded to supplier firms in a member country in the long term corresponds to that country's share of the operations costs (Hallonsten 2014a: 43–44). Another scheme for allowing investments to benefit all member countries, and not only the host, is the use of in-kind contributions during the construction phase (and later upgrades), a principle invented by DESY in the 1980s to allow foreign investment in the big HERA (*Hadron Elektron Ring Anlage*, Hadron-Electron Ring Facility) accelerator while maintaining full German ownership of the machine. In recent years, with XFEL and ESS, in-kind contributions have been used extensively to let member states replace some of their cash investment in a facility with the delivery

[11] The LCLS project was hurt by the continuing resolution of 2007–2008, which occurred during a critical phase of construction of the project and caused delays of some important elements of construction (Hallonsten 2009: 147).

of components, thus being able to both spend their money domestically and let domestic competences come to use in the construction of a facility. The ESS will, according to estimations, rely on in-kind contributions for half of the total investment (Hallonsten 2014a: 44).

A modified version of Fair Return has also been implemented at some labs to distribute scientific access among the research communities of member states in accordance with their budget shares. This idea runs counter to the principle of open and free access on the basis of competition with peer review-based assessment of applications on purely scientific and technical grounds, but the use of scientific Fair Return is in place both at ILL and ESRF, where sophisticated algorithms have been implemented that redistribute slots of experimental time among the lowest-graded applications that are granted access in the competition, to balance out inequalities between countries' relative use and budget shares.

The Politics of Big Science Transformed

Comparing the examples of the United States and the collaborative European level as outlined in the previous two sections, it would seem as if the US system renews itself and produces transformed Big Science primarily through the evolutionary progress of recombination of physical, intellectual, organizational and political assets within what authors have called an "ecosystem" (see below and Chap. 4). In contrast, on the European level, cases where collaborations have made concrete achievements have subsequently lead to great strength due to the pooling of resources (e.g. in the cases of ILL and ESRF). But since such achievements hinge on political "luck" they are also threatened by "unluck" that can delay projects several years (e.g. ESS) and ruin prospects entirely. In the US ecosystem, projects must gain the support of relevant scientific communities and politics to become viable. A lab organization that proposes a project must brandish strong in-house competence in technology, science, and organization of user facilities, and then the project must have the right timing both in relation to internal lab timetables and roadmaps and the overall timetable of the DOE that follows roughly what in the last section was called the "Trivelpiece legacy". In Europe, large projects need either to stay on the national level or must build strong scientific and technical credibility across borders, before trying to "fly" politically. Once decisions are made (if they are made), project management needs to return to the scientific communities to get the job done, with the direct help of the politicians who negotiate the intergovernmental deals to fund the projects, and remain faithful to the deals struck in the negotiations over shares of the investment and returns for the same.

Of course, the previous few paragraphs only dealt with the collaborative European projects. Several neutron scattering, synchrotron radiation and free electron laser labs exist outside of this realm, as national laboratories in European countries (see Appendix 2). Many of these have also become remarkable scientific successes, not least after the introduction of some technologies that vastly improved the performance of such labs while simultaneously lowering costs (Hallonsten and Heinze 2015: 845–846).

By the developments that led to megascience in the 1970s, the old Big Science almost became exclusive to big countries. The only European country that could afford an experimental particle physics program of its own after CERN embarked on the CERN II upgrade and quadrupled its budget in only ten years was the Federal Republic of Germany, which constructed another two large accelerator complexes at DESY in Hamburg (the PETRA and HERA rings, built in 1975–1978 and 1984–1991, respectively), which was the third-to-last lab organization in the world to close its final particle physics experiment, HERA, in 2007 (before SLAC in 2008, and Fermilab in 2011), leaving CERN as the lone star of the global particle physics show (Lohrmann and Söding 2013: 322; Riordan et al. 2015: 285). The same is not the case for the transformed Big Science; although it seems only larger countries such as Germany, the UK, and the USA have the capacity to host nationally organized state-of-the-art neutron sources, synchrotron radiation has developed into a state where not only the larger countries and the intergovernmental collaborations dominate (e.g. APS, ESRF), but indeed smaller countries can also be competitive, and capacity is now also spread to new regions (Hallonsten and Heinze 2015: 845ff).

This is not to say that costs are negligible. A relatively modestly priced but high-class facility design like MAX IV in Lund, Sweden, opening to users in 2016, places demands on its host country's funding system and raises questions about proper priorities. The issue is connected to the current rhetoric of the increased competition of a globalized knowledge economy where small countries are said to be forced to mobilize in certain strategic areas to remain in the game. The theme is not entirely new, although perhaps globalization (or the view on globalization) has accentuated it. Small countries have always faced a policy dilemma of how they should organize their R&D activities, choosing between an attempted even resource spread over a wide range of areas, in order to maximize absorptive capacity and hopefully gain from advances made abroad, or instead specialization and a targeted effort to increase the international competitiveness and visibility of certain areas, with almost complete negligence of others. Phrased differently and more specifically, the question is whether small countries should try to build their own neutron scattering, synchrotron radiation, or free electron laser facilities, or maximize participation in international collaborative ones. The game of priorities, when transferred to this specific case of user-oriented Big Science, has the particular twist that investments to build these labs, and investments to secure their use and contribution to scientific output, are not the same. The construction of a neutron scattering, synchrotron radiation, or free electron laser facility with groundbreaking technical performance in a specific country and at the expense account of that country's national R&D budget does not guarantee that the scientific benefits are reaped by the country in question, unless there is either a well-established world-class user community ready to make use of the new spectacular experimental resources offered by the new facility, or a timely and targeted effort some time in advance to boost particular parts of the existing scientific communities in the country to become such a world-class user community. In addition, success is short-lived and durable efforts are required both on the infrastructure side, to maintain performance and make required upgrades along the way, and on the user community side, to spur further progress in the scientific use of the facility among domestic communities. The risk of negligence is obvious: a world-class user facility, in principle open to anyone on the sole basis of scientific merit, can easily become a resource for everyone else in the world unless the advantages of proximity for the national user community is matched by funding programs that keep users busy at the lab and outside it. The United States is not entirely spared from this potential challenge, although its very large scientific community certainly has a critical mass and the federal science policy system can rely on the system of National Labs and the ecosystems of scientific and political support, and the intersecting resource economies of science, technology, and science policy, to secure the use of facilities on long term. The collaborative European facilities like the ESRF and ILL draw much of their strength scientifically from the fact that they are connected to the scientific communities throughout Europe (see also the discussion on the rapid rise of ILL to world leadership, in a previous section), whose general capabilities (and financial backing) are great enough to secure the quality of the scientific output of the collaborative labs, as seen not least in the competition for experimental time at these facilities (Hallonsten 2013a). For smaller countries, however, the issue can be severe.

In Sweden, which indeed is a small country, the MAX-laboratory has been in operation since 1987, with significant upgrades made at important points in the near 40-year history, the last of which is the major MAX IV facility set to open to users in 2016. In several reviews through the years, MAX-lab has been lauded for its cost-effectiveness and nimbleness, and for remarkable scientific and technical achievements in spite of a limited budget and only a very rudimentary lab organization. It has been shown how the devoted users of MAX-lab, on practically no money at all, managed to produce some remarkably strong results in the late 1980s, and that accelerator operation, instrument development, and user organization are indeed all extreme in their cost-effectiveness (Hallonsten 2011). This, it has been argued, is probably the only way in which MAX-lab could have come into being, given Sweden's decentralized and bottom-up-oriented science policy system. But there is another lesson to be learned from the MAX-lab case: the pattern by which the MAX-lab facilities have been funded, though a series of council grants and substantial additional funding from private foundations, has left the governance of the lab without coordination of efforts. Instruments are built and upgrades are made without a long-term plan for how (and by whom) the facilities shall be used. The "willingness or eagerness of the council to support good science—which is their chief mission—has led to the paradoxical situation of a constant lack of sufficient resources for the operation of MAX-lab," a problem which is "acknowledged in the council" but "not perceived as being part of the council responsibilities to resolve." Therefore, long-time MAX-lab users have observed that since the investments in MAX-lab infrastructure that are supposed to benefit Swedish science are not matched by any substantial funding to secure maintenance of the infrastructure, or its use, the long-term effect can be that high-class instruments are not operated and used at their maximum capability. The Swedish user community has, apparently, been strong anyway and managed to secure funding for its synchrotron radiation experimentation in ordinary competitive funding schemes, but there is a fear of what will happen if the several 100 million dollars spent by the research council on the new MAX IV facility is not matched by substantial funds for increased use of MAX IV by Swedish scientists (Hallonsten 2011: 205–206).

The issue is general. It extends far beyond the boundaries of Sweden and adds another dimension to the politics of Big Science, or makes up a core challenge of the transformed politics of Big Science. It is well illustrated by the extensive accounts on the "ecosystem" of US national Big Science and its role in the renewal of the National Laboratories and the gradual emergence of a transformed Big Science in the United States, which will be further explored in Chap. 4, but also by European countries and the dilemma they evidently face in choosing between specialization and generalization, and between collaboration and national domestic efforts.

3

Organization

Extensiveness and Functional Interdependence

A key difference between the old and transformed Big Science, in their fundamental purposes and in an epistemic sense, was illustrated with the help of Weisskopf's (1967) distinction between "intensive" and "extensive" sciences. The distinction can be translated to an organizational analysis of old and transformed Big Science and used to reveal several additional key features of these labs and the sciences they serve. As the brief historical exposé in the previous chapter shows, particle physics has gradually moved towards epistemic intensification (this was also the example used by Weisskopf 1967) and organizational concentration, the latter most evident in the transition to megascience in the 1970s and later. In Chap. 4, the system-wide consequences of this trend in the United States, that is, the renewal it provoked, will be discussed further. In Europe, the transition to megascience is most evidently seen on the level of smaller countries, where domestic programs in particle physics were cancelled on a broad front in the 1970s and funding redirected to the increasingly expensive CERN collaboration (Martin and Irvine 1984: 188; Herman 1986: 132; Widmalm 1993: 125–126). In more recent

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times, the organizational concentration has continued and moved to the global scale. Since the closing of the last particle physics machines at DESY, SLAC and Fermilab in 2007, 2008, and 2011, respectively, experimental particle physics is undertaken only at one place in Europe and the United States, CERN in Geneva. Besides science policy, a sociological consequence of megascience that has received some attention in popular science is that individuals virtually drown in the large collaborations in particle physics: the two articles in *Physics Letters B* that announced the simultaneous discovery of the Higgs Boson in two separate experiments at CERN in 2012 contained lists of 2932 and 2891 authors, respectively.

In the transformed Big Science, nothing of the sort exists.[1] On the contrary, the type of science done at transformed Big Science labs is epistemically "extensive" and organizationally dispersed, not only by the still ongoing growth of the use of neutron scattering, synchrotron radiation, and free electron laser laboratories around the world (see Chap. 2), but also because it seems that the breadth of the areas of use is still growing and thus specialization of equipment and its use continues. Historically, neutron scattering and synchrotron radiation have moved from small lab curiosities that were mostly the interest of some specialisms in condensed matter physics, to an extreme breadth that nowadays includes the life sciences and cultural studies—in other words, extensification and dispersion. The user communities of neutron scattering, synchrotron radiation, and free electron laser labs in Europe and the United States are today counted in tens of 1000s (exact numbers are hard to compile, see below), and the users come from many different fields of research and are found at most universities and research institutes in the natural sciences.

[1] In Chap. 5, a sample of all journal articles listed in the joint publication database of the European Synchrotron Radiation facility (ESRF) and the Institute Laue Langevin (ILL) for the year 2014 (which amounts to a total of over 2000 articles), and the publication list for 2014 at the website of the free electron laser facility Linac Coherent Light Source (LCLS) at SLAC (amounting to 67 articles), were used in a bibliometric analysis to calculate several key figures (see Tables 5.3 and 5.4 in Chap. 5), among them the average number of authors of journal articles based on work done at the facilities. For the ESRF and the ILL, this average lies between seven and eight, and less than one-sixth of all the articles in the sample have more than ten authors. For the LCLS, the figures are slightly different but remain on modest level compared to the experiments at CERN: The average number of authors is just over 20, and two-thirds of the articles have more than ten authors. On the other hand, none has more than 60 authors.

Interestingly, the disciplinary complexity of the user communities is great enough to evade straightforward categorization, as will be shown below.

In his 1984 classic, reprinted with a new introduction in 2000, Richard Whitley made a formidable effort of theorizing "the social and intellectual organization of the sciences." In key parts, this work broke with the then relatively young field of science and technology studies (STS) that had diverted from the sociological path and developed into something of a postmodern theory of science instead of sociology of science as a social field (Bourdieu 1975), a social system (Luhmann 1992), or an institution (Merton 1938, 1942, 1957). Whitley's (2000/1984) contribution is strongly analytical, outlining the organization of work and knowledge production/dissemination in publicly funded science by examining differences and commonalities between disciplinary fields on the basis of some assumptions of their fundamental social organization. Disciplinary fields, writes Whitley (2000/1984: 8–9), "are the social contexts in which scientists develop distinctive competences and research skills so that they make sense of their own actions in terms of these collective identities, goals, and practices as mediated by leaders of employment organizations and other major social influences." They are "the major forms of social organizations which structure the framework in which day-to-day decisions, actions, and interpretations are carried out by groups of scientists primarily oriented to public intellectual goals." Going back to the "essential tension" as discussed in the beginning of Chap. 1, it is the constant interplay between novelty and conventionality that gives scientific work its fundamental principles and its basic organizational and institutional framework. Whitley notes that this also singles out scientific work among other professions or organized human activities: "Task uncertainty" is greater in science, although it varies within and between disciplines, but in general it is continuously growing because "techniques and procedures" are constantly refined "so that practitioners frequently have to change their working practices" (Whitley 2000/1984: 11; cf. Ziman 1994).

Importantly, for the current discussion, Whitley (2000/1984: 14) is keen on pointing out that the governance of science is ultimately the task of trained individual scientists, because task uncertainty makes authoritative central planning and bureaucracy inefficient: "Work planning and execution in the public sciences are decentralized to individual workers who maintain considerable control over low-level goals and the use of particular procedures." There are exceptions from Whitley's analysis: the old Big Science, especially the weapons programs of World War II and its immediate aftermath, but also particle physics after the advent of megascience was centrally planned, bureaucratic, and extremely authoritative. In this respect, the transformed Big Science is a return to ordinary science (i.e. non-Big): science done with the aid of neutrons, synchrotron radiation, and free electron lasers is simply ordinary science that happens to be conducted with the help of some machines that require different planning and modes of work—or organization (verb)—to be operated.

On the other hand, as has been discussed in the introductory chapter and as Whitley (2000/1984: 54–55) also points out, a general development of science and science organization and policy in the (second half of the) twentieth century was a growth in the number of stakeholders with an interest in the knowledge production of public research organizations (cf. the concept of the Knowledge Society, as discussed in Chaps. 1 and 6). This means both an increased functional dependence (see below) between scientific fields and between science and society's other actors and institutions, and that existing disciplines, still organized in principally the same departments and institutes in universities, become "internally highly differentiated," altering their modes of work and their objectives to the extent that "departmental tides bear little relation to the research strategies being pursued by employees" (Whitley 2000/1984: 56). The transformed Big Science has no small part in this: Users of neutrons, synchrotron radiation, and free electron laser come from subfields of the traditional disciplines that develop and distinguish themselves in congruence and reciprocity with the evolution of lab techniques and lab organizations at the neutron scattering, synchrotron radiation, and free electron laser facilities. New user groups are attracted to the labs in part because of specialization of experimental methods within their disciplines. When subfields of ordinary science develop accordingly, and start utilizing experimental resources like neutrons, synchrotron radiation, or free electron laser, they have transformed their work modes although they are still bound to their disciplines, their home labs, their home university departments, their journals, their disciplinary communities in a general sense, not least by the fundamental research questions they ask and the answers they seek. One of the major developments within the natural sciences in the last decades of the twentieth century was their growing dependence on instrumentation (Ziman 1994: 43ff; Shapin 2008: 165ff), and the new "instrumental communities" that therefore developed, in parallel with, or adjacent to, traditional disciplines (Mody 2011: 10–19; Shinn and Joerges 2002). Transformed Big Science is part of this development, as it organizes utilization of instruments that are too big or too complex for departments or even whole universities to afford and operate. But the users are ordinary scientists, members of traditional disciplines, and professional communities at universities and institutes.

To describe the relationship between these ordinary researchers and research groups and the transformed Big Science facilities that they use, Whitley's (2000/1984: 88) concept of "mutual dependence" is useful. It comes in two flavors: "functional dependence" refers to "the extent to which researchers have to use the specific results, ideas, and procedures of fellow specialists in order to construct knowledge claims which are regarded as competent and useful contributions," and "strategic dependence" refers to "the extent to which researchers have to persuade colleagues of the significance and importance of their problem and approach to obtain a high reputation from them." The mutual dependence between scientists and the facilities is of both types: the scientists are functionally dependent on the labs because they need to use the instrumentation made available there, but also strategically dependent on them, because in order to gain access to the instruments, they need to convince peer review panels of the originality and feasibility of their research projects (see below). Note, however, that the dependence is mutual: the labs are both functionally and strategically dependent on their user communities because they are most of all service facilities. They need external users, and they also need the external users to perform well and lend them credibility, because this is how the labs perform scientifically (see below and Chap. 5).

The gradual increase of the disciplinary breadth of the user communities of neutron scattering, synchrotron radiation, and free electron laser labs also means a growing functional dependence between the disciplines represented in the user communities. Physics was first; both in neutron scattering and synchrotron radiation, the initial experimental work was done in solid-state physics and its predecessors. The basic infrastructure, reactors and accelerators, but also the vacuum and optics technologies needed for practical exploitation of the neutrons and the x-rays, had their origins in physics. The gradual broadening of the user communities (see Chap. 2) brought in chemists, biologists, earth scientists, and several subfields in the border areas between these and physics. One way of framing this development is to say that chemistry, biology, and other fields became more functionally dependent on both theory and techniques of physics, which also coheres with a general growth of the functional dependence between these fields through the whole twentieth century (Whitley 2000/1984: 268). Today, the laboratory practices of the transformed Big Science are less owned by physicists, and instead rather transcend disciplinary categories and occupy what Shinn and Joerges (2002: 213ff) called an "interstitial" arena between fields and between science and technology.

In the long run, growing functional and strategic dependence between fields reduces the strength of intellectual and organizational boundaries between them, and "trans-disciplinary techniques and procedures" emerge, which leads to "the formation of sub-groups based on specialized problems and combinations of skills organized for specific issues" (Whitley 2000/1984: 269, 273). There will be fragmentation, and what remains and unifies the sciences represented is the "joint good" of infrastructures, instrumentation, and experimental opportunities that are worth collaborating to produce (see next section). The transformed Big Science, and the growing disciplinary breadth of the user communities of neutron scattering, synchrotron radiation, and free electron laser labs is a prime example of this development. Thus, these facilities and the instruments and techniques they make available, function as unifiers in an institution of science that undergoes fragmentation and differentiation (cf. Hacking 1996: 69).

In the old Big Science, particle physics developed in "splendid isolation" (Whitley 2000/1984: 61, 268), constantly moving in the direction of "intensiveness" (Weisskopf 1967: 24) and with a mutual continuous reinforcement between theory, experiment, and instrument "subcultures" (Galison 1997) which cut the discipline off from previous functional and strategic interdependences in science and society. In the transformed Big

Science, the experimental resources neutron scattering, synchrotron radiation, and free electron laser develop in constant strategic and functional dependence with the scientific disciplines that use them, expanding the user base by a constant move towards epistemic "extensiveness" (Weisskopf 1967: 24) and of course in symbiosis with several other institutions and actors in its environment (such as funding agencies and politicians, etc., and the wider "ecosystem," see Chap. 4). Therefore, one way of interpreting the growth in number of users and number of areas of use of these techniques is to say that the functional dependence grows between the resources neutron scattering, synchrotron radiation, and free electron laser on one hand, and on the other hand the fields that increase their use of them.

But the disciplinary broadening of the user community was not always smooth and straightforward; several user groups needed to be convinced and "proselytized." Whitley (2000/1984: 268) warned that the identity and reputational status of fields can (be feared to) come under threat if they grow too functionally dependent on other fields or on resources (such as access to instrumentation) that are (perceived as) controlled by other fields. In the case of both synchrotron radiation and neutron scattering, it is no surprise that life scientists to a great degree viewed the reactor and accelerator facilities as under the control and ownership of physicists, and only after far-reaching modifications to both technologies and organizations (see below and Chaps. 2 and 4), have they become entirely comfortable users. Also, when it became clear that the opportunities offered by neutron scattering and synchrotron radiation (and later, free electron laser) would make possible quite extraordinary experimental leaps in biology and chemistry (see below), the labs and their infrastructures were too identified with physics (and certainly the Military-Industrial Complex) to be perceived by biologists and chemists as welcoming to their disciplinary cultures. It took several years of active work to breach this (seemingly predominantly mental) barrier. The late entrance of biology users into the synchrotron radiation community appears puzzling without this background knowledge: Results were obtained and published already in the mid-1970s that showed dramatic increases in performance of measurements and experiments with direct relevance for the life sciences in general, and the structural determination of macromolecules specifically.[2] However, it was not until the early 1990s that this area of use really took off, not least as seen in the graph in Fig. A.6 in Appendix 1, and this was only after purposeful work on several fronts and for several years on behalf of staff scientists and scientific directors of synchrotron radiation laboratories.

It is also important to remember that this barrier, and the process by which new entrants to these facilities overcome it, is one of many practical or organizational enactments of the Kuhnian "essential tension" as discussed above and in Chap. 1. There is a division of labor built into the strategic and functional dependence between user communities and the facilities that, in a somewhat idealized (and stereotypical) manner, can be described thusly: The visionaries of pushing the boundaries of what is technically and scientifically possible are mostly the instrument developers, and the conventionalists that secure the continuation of science from a disciplinary perspective are mostly the users. There are exceptions: not least do users sometimes engage in boundary-breaking instrument development and design of completely new technologies, and it is also not uncommon for users to take up employment at labs, as staff scientists or in leadership positions to develop new concepts and ideas, and vice versa, for facility employees and instrument developers to take a momentary or permanent step into the user community.

The Organization of User Facilities

An analysis of the social organization of a heterogeneous research laboratory, where several actor groups with varying (and sometimes potentially conflicting) interests coexist and coproduce a resource valued by each and every one of the actor groups, can draw a lot of strength from the conceptual work of sociologists interested in "joint goods" and "group solidarity." A rational choice-based view is that actors engage in collective activities

[2] Already in 1974, as the Stanford Synchrotron Radiation Project (SSRP) had just come online, a group of Stanford chemists got access to experimental time and made some very rudimentary diffraction studies. The results, published in 1976 in the *Proceedings of the National Academy of Sciences of the USA*, showed that the resolution of the obtained images were a factor of 60 better than what had ever been achieved before (Phillips et al. 1976).

in order to reach a common objective, or more specifically attain "joint goods" that they would not be able to attain individually, at least not as efficiently as through collaboration (Hechter 1987: 33). This means, by extension, that collaboration requires the existence of shared interests and also rudimentary means of identifying that interests are shared, and some means of organizing the collaboration. But the production of joint goods always involves choices, coordination, and allocation, which means that it will have to rest upon some rules or at least agreed-upon (and institutionalized) practices that govern the collaboration in a fundamental sense (Hechter 1987: 33–34). For the organization to function and attain its goals, all involved parties must obey the rules. But in the specific case of large science facilities, the parties are actor groups with very disparate interests, who all need the joint good and who therefore cooperate and obey the rules voluntarily, out of self-interest, although the nature of this self-interest varies greatly between the groups. The organizations are therefore "cooperative games," in contrast to "non-cooperative games." The classic "free rider problem," the basic premise that rational individuals won't contribute to the common goal unless coerced (Olson 1971/1965), and the outcome of the "prisoner's dilemma game," that individuals in non-cooperative games have a rational incentive not to cooperate (Ostrom 1990)—both key concepts in the analysis of group solidarity and the production and distribution of joint goods—generally do not apply in cooperative games. What does apply, though, is the necessity of coordination, delegation, and allocation. Importantly, also, the collaboration needs to be based on a joint agreement to pursue the optimal use of the specialist knowledge and skills of the various actor groups, and needs to be organized so as to make such optimal use of these as well as the mutual adjustment of interests, all of which are also important basic rationales for collaboration (Arrow 1974). Built into this is of course the information asymmetry problem of principal-agent relations (see Chap. 1), that operates on a micro-level in all the relationships of mutual dependence between actor groups in a heterogeneous collaboration. The division of labor based on task heterogeneity and the exploitation of complementary specialist competences, for the sake of achieving a joint good not obtainable by other means, involves delegation and basic, mutually trustful relationships. It is rational to cooperate on these grounds, also if the goals that actors seek to attain by the cooperation are completely selfish, such as eponymous scientific discovery or the reaching of a prestigious lab leadership position. Clear is, also, that institutions facilitate cooperation by building mutual trust, lowering transaction costs, and thus help enabling participants to benefit from the joint good.

Transformed Big Science labs are highly heterogeneous organizations and prime examples of how individualist interests may well be best pursued by cooperation to produce joint goods. The lab organizations must be understood as follows: they have no centrally planned objective or collective goal other than providing the best possible conditions for the fulfillment of a myriad of individually formulated goals of actors that come and go and that may have little or no knowledge about each other's objectives—partly because they don't have to, and partly because they are way too specialized, cognitively (scientifically) as well as practically (in terms of professional tasks).

The basic organizational structure of synchrotron radiation laboratories can be conceptualized as created and sustained by the interaction of two primary forces: the unification provided by the central physical infrastructure (the accelerator) and its operation, and the disunification of the dynamic and varied scientific program, consisting mainly of external users who come and go (Hallonsten 2009: 101–107; 257–261). This identification of two counteracting (but symbiotic) forces is somewhat oversimplified but has some clear explanatory value. It is principally valid also for neutron scattering and free electron laser facilities and can therefore serve as a useful starting point here.

Most neutron scattering, synchrotron radiation, and free electron laser labs have a machine division or accelerator division whose responsibility it is to operate the machine at the highest possible performance, and thus deliver beams of neutrons or radiation to the experimental stations (sometimes called beamlines).[3] In normal cases, this requires what is

[3] Experimental stations are located around the accelerator (synchrotron radiation labs), or the reactor or target area (neutron scattering labs), and the neutrons/x-rays reach them through neutron guides/beamlines. Free electron laser labs use linear accelerators, and their experimental stations are placed together at its end. Technically speaking, a beamline is only the vacuum pipe through which synchrotron radiation is transported from the accelerator to an experimental station, but in colloquial use of the word it sometimes refers to experimental stations, not only at synchrotron radiation labs but also at neutron scattering and free electron laser labs. See Appendix 1 for a more comprehensive technical description.

described as an "industrial" mode of work (Hallonsten 2009: 257–258), in comparison with the scientific program; high figures of uptime are one of the measures of quality of these labs (see Hallonsten 2013a and Chap. 5). There are exceptions; smaller labs with closer links to a university may have a machine division that is also engaged in academically oriented accelerator R&D work and the training of doctoral students, and these labs may have to accept some risk-taking in the operation of the accelerator complex that can reduce uptime figures slightly. The larger labs such as the European Synchrotron Radiation Facility (ESRF) in Grenoble, the Spallation Neutron Source (SNS) at Oak Ridge National Lab in Tennessee, or the Linac Coherent Light Source (LCLS) at SLAC National Accelerator Laboratory in California, whose missions are completely focused on meeting the needs of their (external) user communities, reach uptime figures of 98–99% (Hallonsten 2013a). Their machine divisions obviously have a radically different organizational culture compared to the dynamic research program whose orientation towards the external users make them highly transient and dynamic (Hallonsten 2009: 259–261). But regardless of the character and size of the lab, and the mission of the machine division, the key difference between it and the experimental program shows in that designing, constructing, operating, maintaining, and upgrading an accelerator complex is an academic discipline of its own—accelerator physics—which developed in close harmony with the evolution of particle physics in the 1950s, 1960s, and 1970s, and which still bears traces of the large-scale organizations that were created back then to accomplish megaprojects. The experimental program at a transformed Big Science lab, on the other hand, represents a wide spectrum of academic disciplines from solid-state physics to structural biology and even beyond the typical natural, medical, and engineering sciences faculties of a university.

Disciplinary Breadth and Complexity

Consequently, it is hard to make general statements about the missions of the experimental divisions of these labs unless they are kept broad and describe a responsibility to run and maintain experimental equipment and provide it to users on the basis of competitive time allocation schemes.

This of course says little about the actual work of the staff scientists and the scientific directors. The experimental programs of transformed Big Science facilities are essentially disunified, both in formal (disciplinary) and practical (task heterogeneity) terms, and the disciplinary breadth of the user communities and experimental programs of these labs can be enormous. The roughly 92% of the publication output of the synchrotron radiation facility ESRF in 2014 that appeared in academic journals indexed in the Web of Science (WoS) database—in total, 1669 articles—were spread over a group of 422 journals representing no less than 103 of the subject categories defined by the WoS database and assigned to the journals indexed.[4] The figures for the neutron scattering facility Institute Laue Langevin (ILL) are similar: roughly 88% of its publication output of 2014 was in journals indexed in the WoS, and these 146 journals represented 50 subject categories. The free electron laser facility LCLS at SLAC, significantly smaller in its publication output volume, saw 90% of its publications in 2014 emerge in 23 WoS-indexed journals that represented 18 subject categories. In all three cases, the variety of subject categories was wide, ranging from "Neurosciences" and "Oncology" over "Microbiology and Plant Sciences" to "Metallurgy & Metallurgical Engineering" and "Condensed Matter Physics" (ESRF), "Physical Chemistry" and "Polymer Science" over "Geochemistry & Geophysics" to "Biochemistry & Molecular Biology" (ILL), and from "Cell Biology" and "Biochemical Research Methods" over "Multidisciplinary Materials Science" to "Atomic, Molecular & Chemical Physics" (LCLS).[5]

The scientific programs of these facilities are subject to various attempts at categorization, if not for any other reason than administration or advertisement, and the categorizations can in fact serve rather well to show the diversity and fragmentation of the user communities. Table 3.1 combines three different subject-categorizations found in the annual report ("Highlights") of the ESRF for 2014, and demonstrates the

[4] One journal may have up to six such disciplinary category labels assigned to it, but most have only one.

[5] See also Table A.1 in Appendix 1. An exhaustive analysis of this bibliometric data, and the wider and deeper implications that it has, is found in Chap. 5, where the data is also represented in more comprehensive form.

Table 3.1 Different ways of subject-categorizing the experimental program at the ESRF

Chapter headlines in the 2014 "Highlights" report
Soft condensed matter
Structural biology
Electronic structure and magnetism
Structure of materials
Dynamics and extreme conditions
X-ray imaging
Categories used in experimental time statistics, 2014 "Highlights" report
Soft condensed matter
Medicine
Structural biology
Life sciences
Chemistry
Earth sciences
Hard condensed matter science
Applied materials science
Engineering
Environment
Cultural heritage
Methods and instrumentation
Beamline groups in the experimental division, January 2015
Structure of soft matter
Structural biology
Electronic structure and magnetism
Structure of materials
Dynamics and extreme conditions
X-ray imaging

Source: ESRF Highlights 2014

variety with which the scientific activities at a transformed Big Science lab can be sorted.

While the chapter headlines in the highlights report (top of table 3.1) have not changed much in recent years, the categories used in reporting experimental time statistics are changed recurrently, for reporting reasons, as balances shift between different areas. When the ESRF opened for users in 1994, only six categories were used, and these have since been split up and changed in the reporting as user numbers have grown and new areas have become more important. Most recently, in 2013, the categories were reshuffled so that they represent disciplines or scientific areas more than techniques; for example, "macromolecular crystallography" was abolished along with "surfaces and interfaces"; instead "life sciences," "structural biology," and "earth sciences," among others, were introduced. The opposite change was made in 2012 to the proposal review panels in charge of evaluating applications for experimental time. Before 2012 these were organized in scientific fields (roughly, but absolutely not exactly, corresponding to the categories used in experimental time statistics as seen in Table 3.1), but now each panel allocates time on a specific set of beamlines, and they bear the names C01, C02, C03, and so on. This change likely had organizational reasons: the allocation and scheduling of experimental time is perhaps regarded as more efficient if each panel is responsible for a set of beamlines.

Not many of the categories in either of the three boxes in Table 3.1 correspond to established scientific disciplines as found in the names of university departments or tertiary education programs; some are likely to overlap with journal names and names of centers of excellence and similar cross-disciplinary entities at universities and institutes, but it is clear that the scientific programs of transformed Big Science labs do not lend themselves to simple categorizations along established disciplinary boundaries.

The picture grows yet more complicated in this regard when looking at the free electron laser and neutron spallation facilities recently opened and currently under construction, which seem to be pushing the transdisciplinarity further. A good example is the planning for the free electron laser facility LCLS at SLAC. It originated in the design of a prototype-like machine that would demonstrate the feasibility of the technical concept, but it was developed into a user facility concept much at the request of the funder, the Department of Energy (DOE) (see Chap. 4). Both SLAC and the potential or future user community of the free electron laser seem to have been slightly unprepared for the work to make the LCLS into a user facility, and when a scientific case had to be developed, this was done by the two advisory committees of the LCLS project, whose report entitled "LCLS—The first experiments" was issued in September 2000 and listed five categories of experiments that corresponded to the planned performance parameters of the facility and the very tentative interests of the emerging free electron laser user community. The 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"; in addition to "X-ray Laser Physics" which lies more on the side of methods and instrumentation (Shenoy and Stöhr 2000). The five areas, similarly to the categories at the ESRF as seen in Table 3.1 above, do not correspond to established disciplines and only partially overlap with the instruments that currently (end of 2015) run at the LCLS, which are called "Atomic, molecular & optical science", "Coherent x-ray imaging", "Matter in extreme conditions", "Soft x-ray materials science", "X-ray correlation spectroscopy", and "X-ray pump probe" (a seventh instrument called "Macromolecular femtosecond crystallography" is planned to start operation in 2016). In public information material, however, the LCLS users are sorted into quite traditional categories: "Atomic, molecular & optical", "Biology", "Chemistry", "Soft & Hard Materials", "Methods and Instrumentation", and "Matter in Extreme Conditions" (LCLS website 2016).

In addition to underpinning the previous argument about the difficulty of categorizing the scientific activities at transformed Big Science labs, the example of the LCLS also shows that it is very difficult to predict the content and categorization of scientific programs at these labs in advance, which is a discussion that will be returned to below. As noted in Chap. 1, particle accelerators are prime examples of "generic instruments" (Shinn and Joerges 2002; Rosenberg 1992): from a rather restricted use for the production of nuclear material for weapons and radioisotopes for medical research and treatment during and right after World War II, accelerators became vehicles for the exploration of the subatomic world within particle physics in the 1950s and on, only to move over almost completely, towards the end of the twentieth century, to become the servants of the very wide range (and hard-to-define category) of natural sciences research activities that make use of neutrons, synchrotron radiation, and free electron laser. The labs are, hence, generic both on this very general level, but also in a more detailed sense: the facilities operate a number of different instruments more or less simultaneously (see Appendix 1 for details) and these are highly sophisticated and complex technical setups that require specialized competence and vast experience to design, construct, and operate.

Neutron scattering, synchrotron radiation, and free electron laser labs have established themselves in the sciences as reliable providers of experimental opportunities otherwise inaccessible, through a long process. This process entailed the emergence and maturation of an actor group very much like the "research technologists" that build, operate, and promote "generic instruments" (Shinn and Joerges 2002: 207, op. cit.). These actors have become not only the expert designers of specific instruments, but also well acquainted with the related scientific areas and connected to the scientific communities that form their user constituencies. Most often, they are called beamline scientists, staff scientists, or instrumentation scientists (for reasons of simplicity, they are henceforth referred to as staff scientists). As institutional entrepreneurs, they have had an enormously important role of mediating between the technical capabilities of instruments and the scientific ambitions of users, of mediating between the performance and operation of the accelerator complex and the specific instrument, and of marketing the instrument and promoting its use in wider scientific communities. To speak with Galison (1997), they are the masters of the "trading zones" where the "subcultures" of the labs meet and interact to do science. The staff scientists typically work at one experimental station as responsible for its operation, or as member of a team with this duty, and their role is maintenance of the technical setup, user support, and research. They have a disciplinary base by their training, and they frequently show up as authors on publications coauthored with external users, both reporting on instrument development and scientific use (see Chap. 5). They are the promoters of instruments and techniques, and their role is that of the "spider in the web" or "broker."

With the (growing) technical complexity of instruments as well as the research work done with them, the need for high specialization of skills (expertise) drives team compositions towards increased heterogeneity, which might suggest that the facilities not only create unique conditions for scientific work in a technical sense, but also socially. In the general case, heterogeneity of teams has been shown to increase the efficiency (Dahlin et al. 2005; Nooteboom et al. 2007) and creativity (Heinze and Bauer 2007; Heinze et al. 2009) of collaborations. In the case of experimental work at neutron scattering, synchrotron radiation, and free electron laser labs, however, the heterogeneous composition of teams is a necessity more than a matter of choice, which means that the causality established in the cited studies is not necessarily possible to translate to this context.

How and why these heterogeneous collaborative teams form is an important issue. In one interpretation, the labs bring together high-skilled researchers that are chosen on basis of their skills and through the competitive process of allocating experimental time (see below), which makes the labs into melting pots where creativity thrives. Another interpretation is that the studies that researcher A conducts within project X require the use of experimental opportunities offered at instrument Y at lab Z, and this instrument can only be operated and successfully used by a team with a heterogeneous set of skills, including experts on vacuum technology, detectors and data taking, proper sample preparation, and so on. In the latter interpretation, the synergy effects that are so popular in contemporary research and innovation policy and management are not as evident as a main conclusion from the analysis: The melting pot is a necessity (it cannot be ruled out that in some cases it is even a necessary evil), more than the result of voluntary collaboration in heterogeneously composed teams of high-skilled professionals. There are always potential coordination challenges and a risk of high transaction costs in collaborations (cf. the production of "joint goods" above), and team heterogeneity probably elevates such risks. Fundamentally, therefore, collaborations constitute trade-offs between opportunities and transaction costs. The role of staff scientists as brokers of collaborations was discussed briefly above, and it is worth noting that brokerage has previously been shown to promote knowledge production while perhaps hindering knowledge dissemination (Fleming et al. 2007; Phelps et al. 2012), and to foster creativity at both individual levels and in groups (Heinze and Bauer 2007; Heinze et al. 2009). At neutron scattering, synchrotron radiation, and free electron laser labs, where neither heterogeneously composed teams nor the role of the staff scientist as broker are matters of choice but instead necessities, it is difficult to draw direct conclusions about what these organizational features of the experiments actually mean. Among the very few clear conclusions to be drawn is that the more advanced the instrumentation, the more crucial most likely is the role of the broker staff scientist.

Users and Output

That transformed Big Science labs are primarily user facilities, and that their whole *raison d'être* is to be service facilities for external scientific communities, must of course be added to the mix. A kind of institutionalized division of labor directly influences the organization of labs and their user communities: Somewhat simplified, labs focus all their efforts to provide the best technical (and scientific) conditions for the external user groups to make use of. The users come with their projects, conceived and designed outside of the labs and in ordinary research organizations with financing from first stream university funding, third-party grants, or corporate investment. The competences and knowledge of external users that are crucial for their own scientific work are directly and indirectly used by labs in instrument design, construction, and upgrades, and so on (see below).

Both corporate and public science users travel the world in search of the most favorable experimental opportunities for the experimental work they currently plan. The crucial resource or commodity is experimental time, which means time of access to an instrument at a lab. Experimental time is for the absolute most part awarded in a procedure of organized peer review, a formalized system whose exact structure of course also varies between different labs (not least with size) but which in the absolute most cases involves the following components.[6] A call for proposals is normally issued once or twice a year, with a fixed deadline after which the applications for experimental time (or "experiment proposals") are reviewed and graded by proposal review panels (or review committees, names vary) that are either field-specific or specific to groups of instruments (at the largest labs, there are a handful of specialized panels) or composed of representatives from the disciplines served by the lab. The panel members are expert scientists who make a classic peer review assessment that, apart from scientific quality of the proposal and experimental

[6] Policies and procedures for user access to facilities are normally found on facility websites; see e.g. the website of the Spallation Neutron Source (SNS) (http://neutrons.ornl.gov/users), the European Synchrotron Radiation Facility (ESRF) (http://www.esrf.eu/UsersAndScience), and the Paul Scherrer Institute that operates the neutron source SINQ and the synchrotron radiation source SLS (https://www.psi.ch/science/psi-user-labs).

potential, also takes into account the technical feasibility, as well as in some cases whether the proposed experiments fit with the long-term strategic scientific roadmap of the lab. In some labs whose organizations are international collaborations, such as the synchrotron radiation facility ESRF and the neutron scattering facility ILL, adjustments to the allocation of experimental time are made ex post so as to ensure that the time slots are distributed to researchers of different nationalities roughly in accordance with the relative shares of the operations budget of the facility (Fair Return) (Hallonsten 2014a: 44). All labs typically publish very detailed information on specific instruments on their websites, and on basis of this information, prospective users are able to carefully plan their proposals. Within the same lab, the popularity of instruments may differ widely, which is reflected in the oversubscription rates of specific instruments, that is, the ratio between demand and supply of experimental time, which is a fair measure of the popularity of a certain instrument setup or experimental opportunity in the concerned scientific communities (see Chap. 5). The competition varies between different instruments and different facilities (Hallonsten 2013a).

The body of academic publications that report experimental work done at neutron scattering, synchrotron radiation, and free electron laser labs is one major resource for analysis of the heterogeneity (disciplinarily as well as organizationally) of the labs' scientific activities (see Chap. 5). This publication output in itself bears proof of the enormous disciplinary variety in the user communities of these labs, but it must also be noted that while the publication lists advertised by the labs themselves are the only comprehensive collections of records of their scientific productivity, they are also highly ambiguous. Although a publication reports work that has been conducted at a specific facility, with the help of the instrumentation at one of its experimental stations, the publication does typically not report the results of the experimental run at the facility *per se* but a wider set of results from a study where use of neutron scattering, synchrotron radiation, or free electron laser made a contribution. How significant this contribution was cannot always be determined, not even by reading the full text of the article: While some authors note the specific experimental station and technique used, and the specific analysis or measurement performed with this technique, it is not uncommon for experimental stations at more than one facility to be listed. It is also not uncommon that information on the facility or facilities used is found only in the acknowledgements or in the methods section. This means that facilities are usually not easily identifiable in those publication lists and tables of contents of journals that are openly accessible and that specify mere titles, author names and affiliations, journal names, abstracts, and keywords. Further, it means that only a full-text read of a publication suffices for determining the exact relation between facility, experimental station, experiment/measurement, study, and publication, what role the facility and its experimental equipment played for the specific results reported in the publication, and whether this role is to be considered crucial or decisive. In some rare cases, publications that report scientific results obtained partly at a neutron scattering, synchrotron radiation, or free electron laser facility do not mention the facility at all.[7] The most important implication is one that is returned to at repeated times throughout this book, namely that neutron scattering, synchrotron radiation, and free electron laser facilities do not produce any science themselves—rather, it is their users that do.

Heterogeneity and Organizational Complexity

The diversity and variety that signify these transformed Big Science labs, and also their built in technical flexibility—experimental stations can be dismantled and others built up in their place as scientific demand varies (see Appendix 1)—puts enormous demands on lab organizations, both technically and administratively. The labs employ large cadres of scientific, technical, and administrative personnel to manage the process of allocating experimental time, accommodate users and their needs, and keep the whole lab and all its highly delicate components in the best shape possible in order to achieve the most favorable conditions for scientific work at the lab. Besides this organizational complexity, the labs run highly sophisticated technical systems. In order to run an accelerator or a reactor full-time and at the best possible performance level, technical

[7] This discussion is taken further in Chap. 5, together with a comprehensive analysis of bibliometric data to support the arguments.

and scientific rigor and muscle are necessary. The quality of the neutrons or x-rays delivered to the users at the beamlines depends on several things but is ultimately determined by the performance of the accelerator or reactor and not least its operational reliability—in other words, the extent to which the unavoidable breakdowns of an accelerator can be kept to a minimum. Uptime, measured as the percentage of scheduled experimental time actually delivered, is therefore one of the most straightforward and rudimentary measures of performance of transformed Big Science labs (see Chap. 5).

Different users interact with experimental equipment and with staff in very different ways and to different degrees. This shows in their level of involvement in lab affairs as well as in their work mode. Most clearly, there is a difference between users who modify and customize equipment and work together in teams with laboratory personnel, and users who run more or less automated experiments and use staff scientists mostly as consultants or assistants. In the former case, it is more adequate to speak about "experimentation," whereas in the latter case perhaps "measurements" is more appropriate; much work in materials science typically requires longer slots of experimental time to allow in-situ sample preparation and modification and iterative experimental work. At the opposite side of this spectrum are the automated crystallographic measurements of samples prepared ahead and inserted into a sample-switching robot or even sent by mail to the facility either for remote-access data collection or for staff scientists to make the measurements. There are, obviously, many varieties in between. What is clear, though, is that the differences have both technical/scientific and historical/organizational reasons. In the early days of synchrotron radiation research, it was physicists that paved the way for the development of experiments and that engaged in instrument development (Hallonsten 2011, 2015; Heinze et al. 2015), and it was only after a certain level of performance stability had been reached, and customization and automation of (some) experimental stations accomplished, that the life science users entered on a broad front (Hallonsten and Heinze 2015).

Likewise in the early days, it was necessary to involve users directly in the design, construction, and operation of experimental stations at both neutron scattering and synchrotron radiation labs (Hallonsten 2015a;

Shull 1986). This was for two main reasons: the first exploratory activities in using these techniques naturally had limited in-house competence and needed to use the expertise of their (emerging) user communities in building up experiments; also, the limits on funding made it necessary to get human and material capital in. At some labs, user groups were contracted to build, operate, and maintain whole experimental stations, both as a way to bring in expertise and as a means to fund equipment and parts of operations externally. In recent years, such solutions have been largely abolished and replaced by other ways of securing the input of the users and using their specialized competences while maintaining overall control over facilities and equipment, mostly because some experiences with too strong user involvement of this type have led to extreme compartmentalization of lab organizations (e.g. Holl 1997: 473). A relatively new phenomenon that serves to secure returns on investment for member countries in international collaborative facilities is the use of in-kind instead of cash contributions, which means (somewhat simplified) that a country contributes with a piece of equipment or a module of the infrastructure to the construction of a facility and thus lets its funds stay in the domestic economy and engage local capacity in the development work, a policy that also secures valuable expertise from a greater geographical area in the design and construction of facilities (Hallonsten 2014a: 43–44). The policy is used extensively both at the free electron laser facility the European XFEL, under construction in Hamburg, and the neuron scattering facility the European Spallation Source (ESS), under construction in Lund, Sweden.

The key themes in the histories of both neutron scattering and synchrotron radiation, as outlined in Chap. 2, are expansion and diversification. The developments on the side of technology, scientific use, and lab organizations have gradually but greatly expanded the constituencies of the facilities and made it possible for them to accommodate more and more diverse user groups with very different preferences. These developments have occurred on global level (Hallonsten and Heinze 2015: 848–852; Bacon 1986b) as well as within the walls of individual labs (Hallonsten 2015a; Heinze et al. 2015; Doing 2015). Neutron scattering, synchrotron radiation, and free electron laser labs are inherently dynamic and unpredictable in that their user communities and also their technical setups vary over time, as demands shift and technology develops. While original designs and scientific cases are developed and presented to funders in advance of go-ahead decisions and the start of construction, these seldom or never encompass all the future capabilities and utilizations of the lab. Incremental equipment upgrades and altering of technical setups that improve performance, and also the substitution of whole experimental stations and discontinuous upgrades on facility components, happen regularly. Normally, new facilities are built gradually so that the accelerator is taken into initial operation before the construction of experimental stations begin, and these are furthermore not all built at the same time but in steps. The ESRF opened to users in 1994 with nine experimental stations; in the following three years the lab opened six new stations annually, and in its fourth year of operation it opened another three, so that at the end of 1998 all 30 planned beamlines were in operation (ESRF Highlights 1994–1999). At the ESS, the aim is to start producing neutrons in the spallation process by 2019, but there is no set date for when all planned 22 instruments will be operational; 2025 is set as a year when 16 of them are supposed to be completed (ESS 2015).

That whole facilities, in fact, can be viewed as "generic instruments" makes the opportunities for incremental improvements and alterations nearly endless, but it is only with the right combination of technological possibilities, user demand, organizational capacity, and individual initiative that developments occur. The joint efforts that produce certain developments combine technology, scientific ambitions, and organizational capacity in "cooperative games" where all the actors involved have rational reasons to partake and contribute: they will thereby produce a "joint good" or achieve a goal with benefit for all that would otherwise not be attainable. Should one or a few actor groups not benefit on long or short term, generally or with respect to specific elements of their professional activities, they would not choose to be part of a (transformed) Big Science effort but rather pursue their interests elsewhere, in another collective setting or in "splendid isolation" (Whitley 2000/1984: 61, 268, op. cit.).

Materially, transformed Big Science labs are unified by their spatial demarcation and central infrastructure (the accelerator or reactor), and disunified by the variety of the scientific activities they serve and which are their main *raison d'être*. In large parts, the experimental equipment is rare or unavailable elsewhere, in some cases globally unique. As part of the launch of the conceptual framework and the work to define the central concepts of the analysis of this book, Chap. 1 detailed the key differences between old and transformed Big Science in the three dimensions of big organizations, big machines, and big politics. The current chapter shows that one key difference between old and transformed Big Science is in the meaning of big organizations. While the old Big Science was mostly about big organization of single experiments, and in many cases the operation of accelerators and the experimental work was the same or at least very closely tied also organizationally or professionally, the big organization in the transformed Big Science is mainly a service or support organization for a wide variety of activities. Transformed Big Science is little or small science that uses big machines, and the big organization works most of all to enable and sustain this little or small science. The science itself, and its output, is heterogeneous and complex on the verge of the incomprehensible.

There is, hence, a strong bottom-up side of the governance of transformed Big Science labs that originates in its heterogeneous and inherently dynamic user communities, in the professional norms, cultures, and practices of its constituent actor groups, and in the personal ambitions of the individuals that populate them. This bottom-up side of the governance serves to create a state of continuous change that reflects the evolutionary nature of science (cf. Hull 1988; Kitcher 1993) and of technological systems (cf. Hughes 1987). The practical process by which ideas and initiatives are sorted, ranked, bet on, or discarded, is usually a collective endeavor that involves several different parts of laboratory organizations, external experts, policymakers, and administrators, and large groups of users. A lot of this work, besides technical design work and the creation of a scientific case, is about establishing credibility around proposals in the shape of both demonstrable technical feasibility and usefulness in the shape of a strong enough showing of support from the relevant user communities. Workshops are routine and have a kind of constituent or even constitutional function where users and facility staff meet and align their collective priorities in plenary sessions and smaller assemblies.

But the continuous input of the users, and the continuous alignment of this input with the capabilities and agendas of the lab, also happens in the day-to-day scientific work and the throughput of users. In this regard, the facilities are in a sense self-sustaining, or "autopoietic" (Luhmann 1995) (see Chap. 1), that is, renewed, revitalized, and constantly optimized on the basis of its own elements: the technologies, the users, the staff scientists, the directors, the funding, and so on. These form resource economies or ecosystems that provide the basic prerequisites for the renewal and adaptation that both produced the transformed Big Science and sustain it through continuous optimization and calibration.

4

Resilience and Renewal

Renewal in the US National Laboratories Ecosystem(s)

Historians of science Catherine Westfall and Robert Crease have made remarkable efforts of chronicling and analyzing the histories of the Argonne National Lab, Brookhaven National Lab, and Lawrence Berkeley National Lab in the 1970s, 1980s, and 1990s. Their journal articles and books about the various big reactor- and accelerator-based infrastructures that were planned, constructed, and operated (the latter, in those cases where they have not been cancelled before completion) at these labs make up a formidable collection of secondary sources of great relevance here. Though completely restricted to the USA and the National Labs, the analyses reveal many crucial insights about how (transformed) Big Science facilities are born and raised organizationally and politically, as well as what tangible and intangible resources they need to combine and recombine—and how—in order to become viable as projects and achieve success in scientific, technological, organizational and political meaning. Westfall is the author of the very telling "ecosystem" metaphor for describing the environment in which major facility projects

O. Hallonsten, *Big Science Transformed*,
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are conceived, promoted, and (sometimes) realized in the US system of National Laboratories. The concept is best articulated in her 2010 article about the neutron scattering facility the Intense Pulsed Neutron Source (IPNS) at Argonne, which “came into existence and continued to exist at the nexus of multiple environments”, including the “multidisciplinary mix of ideas and innovations” at Argonne, a prospective user community, and several managers and officials inside the lab and in the national science policy system (Westfall 2010: 394). The “resource economy” of this ecosystem included not only the Argonne organization and its heterogeneous sets of material, social, and intellectual assets, including previous retooled and recombined infrastructures, but also the nationwide scientific community in the amorphous but broad field of materials science, and of course politics, the economy, and society in a broader sense.

The origins of the US National Lab system were outlined in Chap. 2, where it was also noted that these organizations (and the system they make up) seem to embody the logic of “problems change, solutions remain” (see also Chap. 1) that plays into the renewal and transformation of Big Science: The “chilling” statement of the review committee chairman David Packard, that “preservation of the laboratory is not a mission” (Holl 1997: 401) has been proven wrong. Although many National Labs have renewed themselves quite ingeniously, it is also a telling fact that none of them has ever been closed (Hallonsten and Heinze 2012, 2016), unless of course the Superconducting Super Collider (SSC) is counted, which formally was a newly created National Lab when its construction was halted and the whole project terminated in 1993.

Large organizations that are in control of major physical assets and employ several 100 people have a built-in inertia and resilience (Greenberg 2001: 15; Kurth 1973: 139–145; Teich and Lambright 1976: 447). Single programs and specific infrastructures are more vulnerable to the changing of political and societal tides (Gusterson 1996: 227; Holl 1997: 446ff; Westfall 2008a). In some instances in the almost 70-year history of the National Labs, the decline of fields and missions have threatened the existence of entire labs, and they seem only to have saved themselves from closing by comprehensive renewal and recombination of assets and efforts into new infrastructures and new activities. The unusual combination of institutional autonomy and large, publicly funded infrastructural and human resources (see Chap. 2) seems to have made the National Laboratories into natural harbors for renewal accomplished by recombination, which also seems to have made these renewal efforts significantly more robust once accomplished.

Historian of the US system of National Laboratories Peter Westwick (2003: 7ff) has suggested that the US National Labs should be analyzed primarily as “an institutional, not a technological, system,” where each lab functions interdependently with the others and in evolutionary harmony with the nation’s overall R&D system. Westfall (2010: 355) criticizes this suggestion, claiming that viewing each laboratory as a “monolithic entity within a single, fixed larger system” precludes proper attention to the internal complexity of the labs and how they connect to the complexity of the external environment, as intertwined resource economies that make up a broader “ecosystem.” This difference in perspective is probably a matter of the level of analysis chosen. The system-level renewal of the National Labs can be convincingly proven by system-level analysis (e.g., Westwick 2003; Hallonsten and Heinze 2012; Seidel 1986), but in order to understand renewal in a more profound sense, the inner processes and mechanisms of individual National Labs must be mapped and analyzed.

Argonne National Laboratory, outside Chicago in Illinois, was one of the five original multipurpose National Labs created in 1946–1947. Argonne’s niche as primarily a civilian nuclear power reactor R&D lab soon crystallized, and this field dominated the lab’s activities for more than four decades, with an initial emphasis on the applied side. It was not until the mid-1950s that a comprehensive effort was made to cultivate a more prominent profile in basic research with a major accelerator facility for particle physics at the center, the Zero Gradient Synchrotron (ZGS), which was funded by the Atomic Energy Commission (AEC) with $27 million in 1957 and opened for use in 1963 (with an ultimate construction cost of $50 million) (Westfall 2010: 357). But Argonne remained essentially multidisciplinary, and its materials science program appears to have been strong enough to mobilize internal lab resources to design and plan a large facility of its own. The proposal was a reactor designed to produce the world’s highest neutron fluxes and make Argonne the leader in neutron scattering among the National Labs (Westfall 2010: 358–359). The reactor, named the Argonne Advanced Research Reactor (abbreviated A^2R^2), became a flagship project of the lab along with the ZGS, but both of the facilities suffered greatly from controversy on the national stage—the ZGS mostly, it seems, because of competition that it did not manage to match (from SLAC but also CERN in Europe), and the A^2R^2 by managerial conflicts with the AEC that had to do with cost overruns and distrust over design choices. The ZGS was allowed to remain in operation until the inauguration of what was to become the Fermi National Accelerator Laboratory (Fermilab, located just nearby, outside of Chicago), but A^2R^2 was cancelled in April 1968, before the start of construction (Westfall 2010: 359–360). Out of its ashes, it seems, grew the IPNS, a means for Argonne to "prevail in contests with other laboratories" and to "provide a rationale for its existence at a time when the National Laboratory environment had grown harsh and inhospitable" (Westfall 2010: 361). Yet the project itself was just as much a result of serendipity as of deliberate planning to secure a new niche. A committee of neutron scattering experts, formed in January 1968 in order to develop new uses of the not-yet-cancelled A^2R^2, "ended up stimulating the design of a new kind of accelerator" (Westfall 2010: 361). Prospective users took part in the work and soon the aim was set on an accelerator-based, pulsed spallation neutron source. A concept involving the sharing of some accelerator modules and functions with the ZGS, to the benefit of both facilities, was developed but delayed by difficulties to get it funded (Westfall 2010: 363–364). In the meantime, the AEC had raised its awareness of materials science and the potential of neutron scattering, and began to encourage Argonne to think bigger: the proposed IPNS facility was apparently viewed as too modest from a Washington perspective (Westfall 2010: 368).

The work to develop a more powerful neutron source organized a range of resources and in-house competences at Argonne, and also involved a promotion campaign aimed at the nationwide materials science community to mobilize prospective users and build the necessary support (Westfall 2010: 365–374). In 1978, the US Congress approved the project and construction could begin (Westfall 2010: 372–375). The ZGS had been closed in 1977, which meant that many parts could be "scavenged" for the IPNS both from there and from previous reactor facilities on site. Organizational arrangements were also imported from the ZGS including a system to admit large groups of external users, which was “key to gaining the constituency needed for the political support and the funding of a large instrument” (Westfall 2010: 374).

However, the Argonne National Lab, argues Westfall (2010: 381), was not very good at handling the external political pressure that grew in the same period and found itself without a plan B—and thus, with not much of a future, should the IPNS fail. Incoming Argonne director Walter Massey consequently “raised the stakes” in 1979 by declaring the IPNS “his and the lab’s highest priority” (Westfall 2010: 381–382). The strategy seemed to work: a few years later the IPNS was lauded by reviews and named “the most reliable materials science machine in the system [of the National Labs]” (Westfall 2010: 385–386). But the success was short-lived: the National Synchrotron Light Source (NSLS) at Brookhaven came on track in 1981, and a new exciting light-source project emerged on the design stage (the Advanced Photon Source, APS, see below), which seems to have made priorities in the system shift again. Several facility projects were ranked higher than the IPNS in the 1980s and it was the APS, launched in 1987 and opened to users in 1996, that was to become Argonne’s new flagship facility (see below).

The work to sustain the IPNS by obtaining the necessary material resources and political/organizational support “was a group effort that joined Argonne and DOE officials and materials scientists,” but the long-term fate of the project “seems to have resulted from a complex of alliances that shifted in constant reaction to changing conditions” (Westfall 2010: 353, 355). Material and organizational resources of a multipurpose National Lab can be more or less generic but for renewal to occur, these resources have to be actively redirected and repurposed on the basis of determination by actors and actor groups who enjoy some influence or can work purposefully to gain influence (see especially the deeper case study of the SLAC National Accelerator Laboratory in the second half of this chapter). Competition between interests both inside labs and in the wider national context can also serve to strengthen prospects and galvanize support, although often unavoidably at the expense of other projects. Pork barrel politics, loyalty to professional communities, and local patriotism should also not be underestimated.

Brookhaven National Laboratory on Long Island, New York, was also founded in 1946–1947 as one among the original five National Labs. When Argonne early on found a niche in civilian nuclear reactor R&D, Brookhaven developed a specialty in nuclear and particle physics research with the aid of large accelerators. In 1952, Brookhaven commissioned the Cosmotron,[1] and in 1960 the Alternating Gradient Synchrotron (AGS), both of which were state-of-the-art particle physics machines when they started operation (Needell 1983: 93ff; Crease 1999). Much of the logic by which Brookhaven developed its particle physics program was drawn from what Crease (2005a: 331) calls a "gentleman's agreement" between the AEC, the lab, and its main competitor, the Lawrence Berkeley National Laboratory in California, for an "amiable way of sequencing the building of new accelerators (…) in which the two labs on opposite coasts would leapfrog new forefront accelerator projects," a kind of early micro-version of the 1980s Trivelpiece Plan (see Chap. 2 and below). The Cosmotron was, hence, superseded by the Bevatron at Berkeley in 1956, and once the AGS had opened in 1960, Berkeley was thought to be in line for the next big machine. Ultimately, in the mid-1960s, this machine ended up elsewhere and left Berkeley with its first mission vacuum (see below), but until then the two labs were "engaged in a stimulating competition" in particle physics that kept both of them at the forefront of this field's development (Seidel 2001: 150). That Berkeley missed out on its own next big machine did not prevent Brookhaven from suggesting, in the late 1960s, that its turn had come to get a major new accelerator project funded (Crease 2005a: 332). Much of the prestige of the lab hinged on this, but not all: Brookhaven already had another big machine in the works, a reactor-based neutron source called the High Flux Beam Reactor (HFBR), which was the first research reactor not designed for "general purposes" but optimized for neutron scattering (Crease 2001: 42).

[1] Since the first circular particle accelerator was invented in the 1930s and named "cyclotron," several accelerator projects in the US National Labs system have been given imaginative names with the suffix "-tron." The Cosmotron was designed to achieve particle energies (see Appendix A.1) comparable to those of cosmic rays, and the Bevatron at Berkeley was designed to reach energies of billions of electron volts (BeV), hence these names. Also, the Superconducting Super Collider had a similar name at an early design stage; because of its size, it was generally considered to only be possible to locate in the desert, and it was colloquially referred to as the "Desertron."

Facility projects at Brookhaven differ a bit from those at Argonne and Berkeley, because none of them individually had to be the locus of all the hopes of the workforce of a National Lab with a declining mission. The Brookhaven facilities of the 1970s and 1980s overlapped in time and were technically and scientifically complementary; in spite of its particle physics niche, Brookhaven was a true multipurpose lab and pursued several major projects in parallel. Thus, when the ISABELLE was cancelled in 1983 (see below), this did not put the whole existence of Brookhaven in doubt, but it seems the NSLS synchrotron radiation facility also did not suffice completely as a replacement for the 1980s and 1990s but had to be complemented by a nuclear physics machine, the Relativistic Heavy Ion Collider (RHIC) (still in operation).

Construction of the HFBR began in the spring of 1962, and it started operation and reached its first target design performance in 1966. Like the A^2R^2 reactor project at Argonne, the HFBR drew on the increasing popularity of materials science in the National Laboratories system and although it was designed for "traditional nuclear physics research," priorities were gradually redirected in order to exploit the potential of neutron scattering applications. This made the HFBR a "classic case of an instrument built for one purpose and used for another" (Crease 2001: 42; cf. Shinn and Joerges 2002, see Chap. 1). But the repurposing also caused delays, and it took until 1982 before the reactor produced the planned neutron flux. As 1982 was also the year of commissioning of the NSLS, together these facilities raised Brookhaven's image as a materials science lab and opened the way for crossover experimental programs using both x-rays and neutrons. Organizational practices were also copied from the NSLS to the HFBR, such as teams of users engaged to build and operate experimental equipment (see Chap. 3) and a proposal-based peer-review system for providing experimental time to users, to the long-term benefit of the facility (Crease 2001: 46). When an ambitious upgrade plan for the HFBR was suggested in the late 1980s, priorities had shifted on the national stage, and Brookhaven was not able to mobilize political support for the plan to make the reactor competitive. Two new synchrotron radiation facilities (that would become the APS and the Advanced Light Source, ALS, see below) and a new reactor-based neutron source at Oak Ridge National Laboratory got higher priority, and funding for the HFBR upgrade was delayed several years. In 1997, a reactor pool leak at the lab, in itself an almost insignificant event but enough to tip the balance, forced its cancellation. After the dust of the local and national political activism, media firestorms, and the ensuing political intervention had settled, time had run away from the HFBR. The user community was relatively small and disunified, and the experimental program of the facility had too little focus on applied R&D with industrial relevance (Crease 2001: 53–55). In November 1999, the DOE decided to permanently close the reactor. Crease's (2001: 54) conclusion is that a complex project like HFBR that "grows out of sync with its environment" can be brought "crashing to a halt" by a "tiny, apparently insignificant event."

The last particle physics facility at Brookhaven, the ISABELLE (Intersecting Storage Accelerator + "BELLE" for beauty), had been approved for construction in 1978, but cancelled in 1983 due to cannibalism among the National Labs, and most of all because the Superconducting Super Collider project sucked up all available resources for new particle physics facility projects (Crease 2005b: 440–449). But Brookhaven's multipurpose strength was to show itself again: when the official announcement of the termination of ISABELLE arrived in October of 1983, it took a month until the lab's mission change was declared in a new "Institutional Plan" document, where the RHIC for nuclear physics and the NSLS synchrotron radiation facility were declared the lab's prioritized projects (Crease 2008b: 565–566). Formally proposed in 1984 and funded in 1991, the RHIC was truly "recombinant," being the result of a reconfiguration of internal lab priorities and the redirection of various resources for new ends, including the re-use of many components from ISABELLE (Crease 2008b: 536–537). The NSLS, on its part, had emerged on the idea stage already in 1970, and was conceived and developed into a real project in the context of "a series of informal lunch gatherings of chemists and solid-state physicists" at Brookhaven (Crease 2008a: 442). But it took several years before a formal proposal was made to the DOE. The technologies involved were new and unproven, and the use of synchrotron radiation nationally was still minor, which made the case for building a dedicated machine not such an easy sell (Crease 2008a: 447–448). Eventually though, the experiences from the Stanford Synchrotron Radiation Project (SSRP) and its parasitic operation on the SPEAR ring at SLAC (Hallonsten 2015a: 240; see also below) showed how painfully vulnerable such “parasitic” endeavors were and demonstrated the need for a national dedicated synchrotron radiation facility (Crease 2008a: 452). In 1978, the NSLS was funded with $24 million, and construction began. The cancellation of ISABELLE in 1983 freed internal monetary and human resources at Brookhaven (Crease 2009: 38), and external prospective users were also involved in building equipment, taking responsibility for specific beamlines and experimental stations, which brought in expertise and funding and anchored the project in the relevant communities (Crease 2009: 30). The NSLS story, concludes Crease (2009: 43), is full of telling episodes about how major facility projects develop inside a National Laboratory environment, and all the tightropes that project proponents and management have to walk.

Ernest Lawrence’s lab in Berkeley—originally called the Radiation Laboratory—was one of the hotspots of accelerator development in the pre-World War II era, and the first lab to construct and operate accelerators at a scale and cost beyond a host university’s budget (Seidel 1992: 28). The 1939 Nobel Prize in physics awarded to Lawrence raised the lab’s prestige and reputation, and when the wartime nuclear weapons program began in 1941, the Berkeley accelerator lab was used for uranium enrichment (Heilbron et al. 1981: 30–32). In 1945, Lawrence got a federal grant for continuing his work in a post-Manhattan Project mode (Westwick 2003: 36), and in late 1946, the Lawrence Berkeley National Laboratory became one of the five original National Labs.

Together with Brookhaven, the Berkeley lab seized the leadership role in particle physics and became the west coast hotspot for this field at least until the end of the 1950s (Seidel 1983: 377–379). In the early 1960s, after Brookhaven had opened the so-far most powerful particle physics machine, the AGS, Berkeley began the design of the next and bigger new machine. However, in the course of its design, several coincidences contributed to an AEC decision to put the localization of the project up for open competition and eventually create a new National Lab—what was to become Fermilab outside Chicago—to construct and operate the new particle physics machine (Westfall 2003: 37). In the wake of this development and the loss of another big project (the Omnitron, an ion accelerator for nuclear science and biomedical research), the Berkeley lab rearranged internal priorities and capabilities and came up with a machine design that combined particle physics and nuclear physics/biomedicine. The resulting Bevalac[2] was adequately clever and opportune, ingeniously combining capacities already on site at a relatively low cost and with the whole lab uniting behind it, to get approved "without controversy," and "at lightning speed" (Westfall 2003: 39–40). This story is a prime and early example of how the internal ecosystem of a multipurpose National Lab, connected in crucial ways to a greater ecosystem of institutions and actors on national political level and in national scientific communities, induced a new recombinant initiative to establish a new niche outside of the dominant logic of ever-larger accelerator complexes (Westfall 2003: 49). But the Bevalac did not completely secure Berkeley's future. When David Shirley became director of the lab in 1980, its future was "up in the air": for the first time after World War II, the Berkeley lab had no plan for a new major accelerator "that would define and give purpose to the laboratory." Shirley's response was a proposal to build a third-generation synchrotron radiation source to "retool" the lab in the long run, making use of the strong internal accelerator development competence and the lab's Materials and Molecular Research Division, whose staff used synchrotron radiation at other labs, and not least, building on the recent invention of a new type of insertion device (a vital technological component of present-day synchrotron radiation facilities, see Appendix A.1) by Berkeley accelerator physicist Klaus Halbach (Westfall 2008b: 570–572). To strengthen internal synchrotron radiation competence, the Berkeley lab partnered with the synchrotron radiation facility at SLAC, not far away across the San Francisco Bay. While Shirley apparently managed to win the support of presidential science advisor George Keyworth for the new facility plans, he failed to win the support of the national materials science community (Westfall 2008b: 575ff), and the project did not take off as planned. Westfall (2012: 442) concludes that "launching a materials science project in the 1980s required much more relationship building than launching nuclear and high energy physics projects had in previous decades," and lab director Shirley had apparently not secured

[2] The name "Bevalac" was a combination of "Bevatron" and "HILAC", the latter referring to a previous Berkeley machine, the Heavy Ion Linear Accelerator, that preceded the Omnitron and which was partly used in the construction of the Bevalac (Westfall 2003: 40).

the support of the then-powerful mid-level managers of the DOE or built sufficient support in the relevant scientific communities. According to its critics, "the Berkeley proposal was nothing more than an expensive trick to keep Berkeley alive" (Westfall 2012: 442). The Advanced Light Source (ALS), which it was later called, was saved only by the Trivelpiece Plan (see Chap. 2) by which all four of the labs with mission crises were given new major missions, and kept alive for some decades to come (Westfall 2008b: 599–600).

Berkeley was, in other words, not the only lab in need of saving. As the 1980s proceeded and the neutron scattering facility IPNS (see above) showed questionable scientific success, Argonne National Lab emerged as "the sick man of the DOE lab system" (Westfall 2012: 443), in desperate need of a new mission. In the late 1970s, the European scientific communities had started evaluating the prospects of a collaborative synchrotron radiation facility that would supersede all existing sources in performance (what would become the European Synchrotron Radiation Facility, ESRF), and there were also plans at SLAC for a similar machine (Hallonsten 2015a: 257). Interestingly, the same materials science community that showed hesitance toward the ALS at Berkeley rallied around the plans for a new larger synchrotron radiation facility project, and the committee reviews that followed placed it at top priority (Westfall 2012: 443). Proposals from SLAC and Brookhaven to build the machine were nonetheless turned down (Westfall 2012: 444), but when Argonne showed interest and the contours of the Trivelpiece Plan emerged in Washington, the match appeared to be perfect. In contrast to SLAC and Brookhaven, both of whose synchrotron radiation proponents had championed the big machine, Argonne had the advantage of not running a high-profile particle physics program, which meant that the future users of the new synchrotron radiation source "would not have to fear being relegated to second-class status at Argonne" (Westfall 2012: 445–446). By summer of 1984, Argonne had become a serious contender for the project, building up user support and in-house competences and developing a scientific case. In 1989, after some years of intense work on alliance-building in the scientific communities and in Washington, funding commenced. When the Advanced Photon Source (APS) began operation in 1995 it was the largest and most expensive basic research facility in the National Laboratories system, a position it retained until in 2006, when the Spallation Neutron Source (SNS) at Oak Ridge (the project promised to Oak Ridge in the Trivelpiece Plan) opened to users (Westfall 2012: 447–448). The SNS was initially subject to harsh competition between the labs at Argonne, Brookhaven, Los Alamos, and Oak Ridge, and although it could well be argued that both Argonne and Brookhaven had better and more relevant scientific and technological capabilities and experience for building a next-generation neutron source, it was Oak Ridge that ended up doing it. Rush (2015: 147) argues that the decision of the DOE to give the SNS to Oak Ridge was that it was the lab's "turn" to get a major facility project, and to compensate for the lack of experience at Oak Ridge in accelerator design and construction, the R&D task was distributed among the four initial competitors plus Berkeley, so that five labs in the system got to share the projected $1.4 billion. Nonetheless, some years into construction Oak Ridge was criticized by Congress for poor project management. Before completion of the SNS, its price tag had been increased to $2 billion (Rush 2015: 149).

Westfall (2012: 440) concludes her analysis of the history of the APS by suggesting that Argonne succeeded in winning the project—and thus securing the lab's future—by "proving to the Department of Energy (DOE) that it was best qualified to supply the necessary resources, including innovative ideas, an array of expertise, stockpiles of equipment, and the approval of various stakeholders." New projects like the ones with histories recounted here are born and raised to success or failure in an immensely complex web of processes, decisions and resource exchanges that involves several actors and actor groups with vastly different interests, all working against an institutional backdrop of laboratory organizations, scientific communities, professions, political and economic trajectories, and long-term societal shifts.

The stories testify to the fickleness of the facility projects of the transformed Big Science, but they also show how multipurpose National Labs can renew and adapt through recombination, in ways that are severely complex and unpredictable. Sensitive to all kinds of things and connected to several different resource economies, the projects can only reach success in technological, scientific, organizational, and political terms if their proponents manage to master their ecosystems and mobilize the relevant resources. Of course, high-level politics is also a game of its own, and one where the bottom-up processes of National Labs and their internal priorities and resource recombination efforts sometimes have no influence or relevance. It cannot be unambiguously pinned down how much the Trivelpiece Plan relied on pork barrel politics, but it is clear that the national political side of the story follows a logic where it seems preservation of National Laboratories is an overshadowing objective. Still, the renewal that enables preservation is apparently the responsibility of the labs themselves to accomplish.

A Detailed Case Study of a Near-Full Transformation

Here, the story of the SLAC National Accelerator Laboratory (formerly Stanford Linear Accelerator Center) in Menlo Park, California, serves as a case in point. When it opened in 1966 after four years of construction, SLAC was a purpose-built single-mission National Lab for particle physics, and in the 50 years that have passed, it has made a near-full transformation to today's status as one of the federal US centers of so-called "photon science"[3] (which includes research with the use of synchrotron radiation and a free electron laser) and is the home of the free electron laser called the Linac Coherent Light Source (LCLS), built and operated as a DOE flagship facility. The renewal of SLAC has been gradual and incremental, and has taken place at different interconnected levels and in different dimensions. In a previously published analysis, these have been identified and conceptualized as the organizational, scientific, and infrastructural dimensions of the lab (Hallonsten and Heinze 2013), and this taxonomy will also be used in the following, together with a previously

[3] It should be noted here that "photon science" is a slightly peculiar term: SLAC is only one of two labs in the world that uses the term to describe its core activity of providing synchrotron radiation and a free electron laser to users (and as a common name for both synchrotron radiation and free electron laser; both are photon beams); the other one is DESY (Deutsches Elektronen-Synchrotron) in Hamburg, which has undergone a very similar transformation as SLAC and can be considered its German twin (Hallonsten and Heinze 2013). The term "photon science" is used in this chapter for reasons of simplicity and consistency, to contrast particle physics.

developed theoretical framework drawing on a conceptualization of organizational renewal through gradual adaptation. Using secondary historical literature for contextualization, the analysis builds extensively on a series of previous publications (Hallonsten 2015a; Hallonsten and Heinze 2012, 2013, 2016).

The previous section reviewed several cases of renewal of multipurpose labs within the National Labs system, but those transformations are less distinct as examples of renewal from old to transformed Big Science. This is mostly because these labs were multipurpose by design and thus they did not as archetypically represent the particle physics discipline at the height of its popularity, as the single-mission SLAC (and Fermilab) did, although of course they also built and operated several machines for particle physics in the forefront of the field's development in the early days. Somewhat paradoxically, it also seems the renewal of the multipurpose labs did not happen by a continuous, long-term process but rather by reinvention of lab practices and mission identities in a relatively short time frame, and mainly through recombination and "scavenging," all under quite heavy political pressure in the late 1970s and early 1980s (Crease 2008b; Westfall 2008a, b, 2012). SLAC, on the other hand, was a single-mission National Lab for particle physics, and is today an almost single-mission photon science lab. It renewed itself endogenously and in tight collaboration with a number of small-scale initiatives in its immediate surrounding, in a gradual and incremental process over more than 30 years. As a complement to the historical account in Chap. 2 and to the histories reviewed above, SLAC is therefore a formidable case to dig deeper into, because it underwent gradual and incremental renewal from old to transformed Big Science at the core of its activity, its mission(s), and its image, all within the organizational framework of a single-mission particle physics lab.

For some decades, SLAC managed to keep up with the competition from Fermilab and other labs abroad (see Table 4.1 below) by reinventing its research programs, staying resolutely within particle physics and embarking on new infrastructure projects that kept it in a competitive position. This was especially true after the transition to megascience and the concentration of efforts to CERN in Europe and Fermilab in the USA, until its final large particle physics machine was

Table 4.1 Particle physics machines at SLAC, and main competitors

SLAC	Main competitors		
	CERN	DESY	Fermilab
SLAC original linac, 1966–1974	PS, 1959–1976	DESY, 1962–1978	Main Ring, 1972–1984
SPEAR, 1972–1990		DORIS, 1974–1992	
PEP, 1980–1994	SPS, 1976–1984	PETRA, 1978–1986	Tevatron, 1984–2011
SLC, 1989–1998	LEP, 1989–2000	HERA, 1991–2007	
PEP-II, 1998–2008			

Note that the years indicated refer to when the machines began and ceased operation for particle physics experiments
Sources: Hallonsten and Heinze (2013), Hoddeson et al. (2008), Krige and Pestre (1990)

closed in 2008. At that point, SLAC had rather successfully cultivated a plan B, photon science, and was able to mobilize around it and make it the lab's new primary activity. The case of renewal at SLAC is therefore, in some ways, a cherry-picked case, but the depth and width of the transformation can give several important insights into the nature of renewal and transformation of Big Science that transcends the importance of such methodological considerations. Also, as noted in Chap. 1, every case study has the drawback of unique characteristics that makes generalization difficult or impossible, but must nonetheless be studied in its own right to render proper qualitative conclusions.

The particle accelerator project that was to become SLAC in the 1960s originated at the Stanford physics department where once a key component of linear particle accelerator technology called the "klystron" had been invented in 1937 and where development of linear accelerators became one of the major activities in the 1940s and 1950s. During the post-Sputnik boom of federal research funding, the group around department head Wolfgang Panofsky started to make plans and seek funding for a giant 3-km linac to be located in the hills west of the Stanford campus that would give Stanford physicists access to "a frontier of physics unapproachable by any other means now considered feasible," as its chief designer Robert Hofstadter reportedly told Stanford president Wallace Sterling at the time (Galison et al. 1992: 65). The first funding application for $100 million was submitted in late 1957, and in the process to grant the funding, it was decided by the AEC that investments of this magnitude warranted the founding of a new National Lab to host the machine, but that Stanford University would be the obvious contractor for its operations (Lowen 1997: 179). In 1959, the AEC and the Eisenhower administration both declared their support, and in 1961, Congress authorized the SLAC project at an estimated cost of $114 million (Panofsky 1992: 132). Ground was broken in July 1962, and in November 1966 the first particle physics experiments were conducted. Panofsky, the obvious choice for SLAC director, remembers being "often asked, after the initial completion of SLAC construction, how long the laboratory could productively operate" and giving as his "standard answer": "Ten years, unless someone produces a good idea" (Panofsky 2007: 126). The Stanford particle physicists had been joined by Burton Richter in the late 1950s, and his work in the area of storage ring technology together with Gerard O'Neill and W. C. Barber was what became the "good idea" that continued the activities at SLAC beyond the initial machine. The SPEAR facility (Stanford Positron Electron Accelerator Ring), starting operation for particle physics in 1973, was to become a formidable success not only in particle physics, where it was part of the 1974 discoveries that were later called the "November Revolution" (Hoddeson et al. 1997), but also for the development of synchrotron radiation and for the long-term renewal and survival of SLAC as a whole.

In the early 1970s, a small group of Stanford University professors of applied physics and engineering were given access to the synchrotron radiation accidentally produced by SPEAR to exploit its potential for spectroscopic experiments in solid-state physics. With the blessing of SLAC directors as well as Stanford University, and with a small grant from the National Science Foundation (NSF), this group formed the Stanford Synchrotron Radiation Project (SSRP) with the status of an outside users group at SLAC (of which there were a recurrent several in the lab's particle physics program) and could mount some rudimentary instrumentation on the SPEAR facility. In the mid- to late-1970s, on the basis of quite astonishing scientific achievements and technical innovation capacity, this project grew and made maximum use of its "parasitic" (Hallonsten 2015a) access to synchrotron radiation, proving itself worthy of some dedicated time on SPEAR and not least serving a growing community of users across the United States. When SLAC opened its next big machine for particle physics and moved its frontline research accordingly, the SSRL (which it was now called, with L meaning "Laboratory" to substitute for "Project" and organized not as a mere externally funded research project at Stanford but as an independent laboratory within the Stanford School of Engineering) was granted rights to 50% of the running time on SPEAR and grew physically, by the adding of more instrumentation, and in numbers, by attracting more and more users. The relationship to the host was mixed: The ground rules for SSRP/SSRL stipulated that SLAC accepted its presence on site as long as it did not interfere with the particle physics program in any way. Simultaneously, the synchrotron radiation activities grew in significance and visibility, in coherence with a national and global rise in awareness of its potential, and the consequential launch of several more synchrotron radiation labs elsewhere (Hallonsten 2015a), including the NSLS at Brookhaven.

SLAC was still a single-mission National Lab for particle physics and the 1980s was a decade of stark contrasts for the lab and its growing photon science branch. When particle physics got an upswing locally as well as nationally, SSRL grew gradually in its shadows, mixing scientific achievements with growing discontent over the limitations on operations stability and performance of the SPEAR ring due to the subordination of priorities under the particle physics program and its most recent major infrastructure project, the Stanford Linear Collider (SLC), which (quite adequately) drained the lab of its resources both physically and organizationally. When SPEAR was deserted completely in 1990 by the particle physicists and became fully dedicated to synchrotron radiation, the number of SSRL users had grown to several 100 annually, in spite of all technical limitations, and as SLAC entered the 1990s two major changes ensued: SPEAR was taken over by SSRL and physically detached from the rest of the SLAC infrastructure, hence fully under the control of the synchrotron radiation program, which was furthermore incorporated into SLAC by the abolishing of the single-mission status of the lab in favor of a dual-mission particle physics and photon science organization (Hallonsten 2015a: 267–269). This organizational change was allegedly driven mostly by SLAC director Burton Richter, the Department of Energy (DOE) (as funder), and Stanford University (to which SSRL belonged, organizationally), with both SSRL and SLAC particle physicists taking a rather skeptical stance (Hallonsten 2015a: 265–269). It is doubtful to what degree the orchestrators of the merger saw the diversification of the lab's mission as a means to ensure the long-term survival of the lab, but some signs of serious limitations to the future continuation of the successful particle physics program at SLAC had started to show. Most fundamentally, the SLAC site had spatial limitations that made it impossible for the lab to keep a frontline competitive position in global particle physics. Neither the Superconducting Super Collider, at the time under construction in Texas, nor its envisioned sequel the Next Linear Collider (NLC) would have fit on the SLAC site. In the wake of the 1993 demise of the Super Collider, SLAC managed to get funding for refurbishing its PEP (Positron-Electron Project) machine into a specialized experimental facility for so-called "b-physics," a subdomain of particle physics that SLAC remained the global leader of for a long time (Riordan et al. 2015: 262–263). Yet also within the United States, it was doubtful whether it would be sustainable and defensible in the long term to have two federally sponsored labs (SLAC and Fermilab) running particle physics machines at annual costs of several 100 million dollars. In spite of such warning signs, however, in the early 1990s particle physics was still the unquestionable main activity of SLAC, and its standing was also strengthened throughout the decade by the awarding of two Nobel Prizes in physics to SLAC in-house scientists Richard Taylor (1990) and Martin Perl (1995), to follow the 1976 physics prize to Burton Richter.

The photon science activities, now in charge of their own machine and part of SLAC's official mission, continued to thrive and grow throughout the 1990s, and took part in the global evolution of the field through the development of new facility concepts and the broadening of experimental opportunities foremost into several areas of the life sciences (see Chap. 2). The old SLAC linac, until 1998 used as part of the SLC, became a focus of attention for the photon science enthusiasts as the free electron laser concept gained in maturity and feasibility, showing signs of becoming the next generation light sources that could open major new experimental opportunities (Birgeneau and Shen 1997: 91–95; Leone 1999: 19–20). Free electron lasers are built with linear accelerators as centerpieces (see Appendix A.1, especially Fig. A.3) and preliminary calculations showed great potential of turning the old SLAC linac into such a machine. The work on this plan took almost a decade until the early 2000s when funding for the project commenced and the construction of the Linac Coherent Light Source (LCLS) could begin. The plan on behalf of SLAC in the 1990s was always to build a prototype machine to try the technical feasibility of the free electron laser concept. However, with support from the 1997 review report on the synchrotron radiation laboratories in the USA (Birgeneau and Shen 1997: 91–95, 118), and its follow-up review report in 1999 on next-generation light sources (Leone 1999: 19–20), the DOE demanded that a full-fledged user facility be constructed, and provided the funds.

When the LCLS opened in 2009, the last SLAC particle physics machine (PEP-II) had already been (prematurely) closed, which in effect meant that today, the experimental facilities operated by SLAC are exclusively serving photon science. Particle physics remains part of the official mission of the lab, most of all as part of the field of "particle astrophysics," which has been included in the lab's scientific portfolio since the mid-1990s. This latter field provided a new home for many of the SLAC particle physicists (theorists and experimentalists), who now work with national and international programs in particle astrophysics and cosmology, including work with satellite-based telescopes in collaboration with the National Aeronautics and Space Administration (NASA) and others.

This brief chronicle of the history of SLAC suggests that the renewal of the lab not only occurred gradually and incrementally but also came about through a broad assortment of initiatives on many different levels, sometimes smoothly passing potential hurdles and sometimes causing conflict and resentment. In the following, the history will be analyzed as exhaustively as possible to explain how and why SLAC has been able to renew itself.

Gradual Renewal on Intercalated Levels

In his 1997 classic *Image and Logic*, historian of science Peter Galison launched a model of "intercalation" and the interaction of different physics "subcultures" to explain the intellectual and institutional renewal of the broad scientific discipline of physics from its inception in the mid- to late-nineteenth century until today. Writes Galison (1997: 798), "it has become awkward to treat physics and physicists as constituting a single, monolithic structure. As historians, we have become used to treating cultures as composed of subcultures with different dynamics." Galison's realization and strong argument for attention to different levels and different dimensions speak to the necessity of looking for clues to macro-level processes deeper down on the constituent meso- and micro-levels, and to conceptualize macro-level change as an aggregation or "intercalation" of several lower-level change processes that operate according to different, yet related, logics. Galison's model for intercalation in the renewal of the physics discipline has foremost epistemological overtones, but the basic idea is possible to translate into a conceptualization of renewal of a complex organization engaged in the operation of large scientific infrastructure, though with some crucial adaptation.

Figure 4.1 shows Galison's model, which seems oversimplified and quite misrepresentative; in a sense, it could be called overly Kuhnian, as it seems to suggest that renewal and development occur only as e.g. a theory is fully replaced by a new one (cf. Kuhn 1962). This was most certainly not the purpose of the model: Galison (1997: 799) writes that its main point is that it "drops the assumption of coperiodization and separates the subcultures of physics into (at least) the three quasi-independent groupings of theory, experiment and instrument making" but that "there is nothing sacred about the tripartite

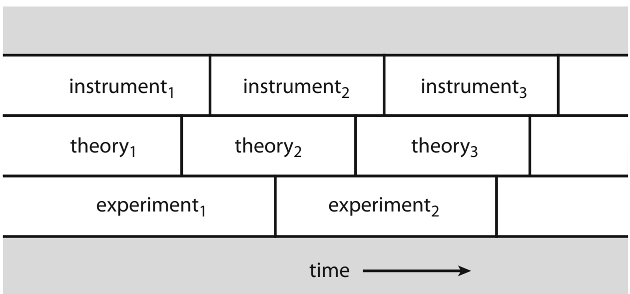

Fig. 4.1 Galison's model of intercalation for the evolution of physics (adapted from Galison 1997: 799)

division"; each of the three "may well split internally into intercalated pieces of varying duration (…) The point is that breaks in one need not coincide with breaks in the others."

An adaptation can be made that replaces instrument, theory, and experiment with scientific program, infrastructure, and organization, and that not only allows all three to "split internally into intercalated pieces of varying duration" (Galison, op. cit.) but also shows examples of this happening. Such an adaptation is seen in Fig. 4.2. Similarly with Galison's use of the model, one point here is that breaks or changes in one of the three dimensions need not coincide with breaks or changes in the others, although of course they can. But the key point is another: namely, to analyze these breaks or changes and how they contribute to overall, long-term lab renewal, how they reciprocate with each other to produce such renewal, and by extension, why the breaks or changes occur.

Institutional renewal is a core topic in a recent strand of historical institutionalism that criticizes the two hegemonic conceptions of institutions as either sustained and reinforced by long-term stability and positive feedback mechanisms, or formed and gaining strength in periods of radical change. In contrast to both, it has been argued that long-term, macro-level institutional persistence can be secured through gradual and incremental adaptation and renewal (Streeck and Thelen 2005; Thelen 2004; Streeck 2009). Given the objective of analyzing the renewal of existing organizations by intercalated developments in several

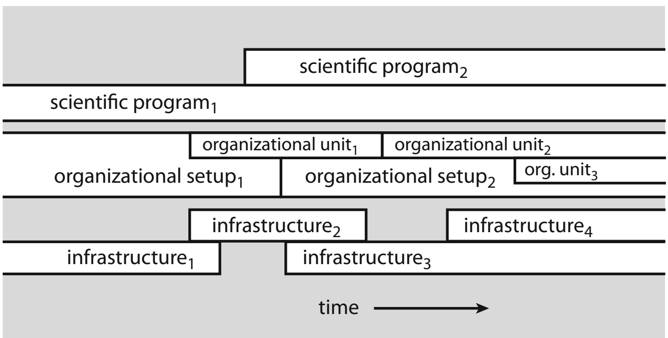

Fig. 4.2 Refined model of intercalation for the renewal of a Big Science lab

dimensions, where breaks or changes in one dimension do not necessarily coincide with breaks or changes in others, and where further renewal is likely to occur as a result of several different endogenous and exogenous change processes, a typology and conceptual scheme for various processes of gradual transformation is instrumental here.

Such a scheme was developed by Heinze and Münch (2012) and adapted and put to use by Hallonsten and Heinze (2012, 2013, 2015, 2016) in analyses of Big Science renewal at the system as well as the individual lab level. This scheme, seen in Fig. 4.3, cross-tabulates processes that remove existing capacities with processes that build up new capacities; taken together, these are exactly the type of processes that can be expected to lie behind the breaks or changes in the three dimensions illustrated in Fig. 4.2 above. The four processes are defined as follows. Layering means that new capacities are added without existing capacities being changed or discontinued, and thus that new elements are accommodated based on the logic of a preexisting system. Conversion means that capacities designed for one set of purposes are redirected to new ones, and this is a particularly interesting and important process since it neither adds new capacities nor takes down existing ones, but nonetheless achieves change. Displacement means that existing capacities are discontinued, with other new capacities taking their place; this is also an

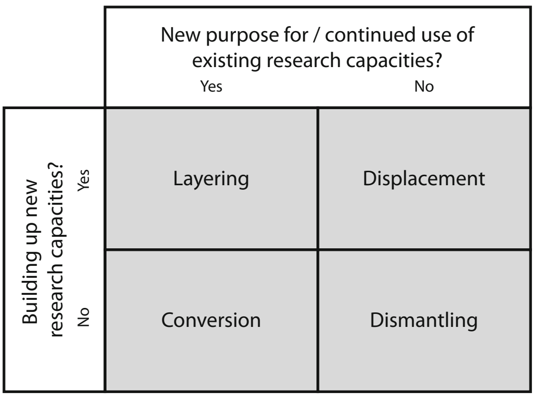

Fig. 4.3 Typology of processes of gradual institutional change (adapted and translated from Heinze and Münch 2012: 20)

important renewal process since it means overall continuity but lower level discontinuity. Dismantling means that capacities are discontinued with no obvious other capacity taking its place.

The first analytical use of this conceptual scheme dealt with the exact same case as in this chapter, SLAC, and combined it with an analysis of its sibling lab in Germany, DESY. This analysis yielded some important conclusions that have implications both for the understanding of the cases empirically, and for the conceptual framework as such. In order to distinguish between the four processes of change and use the categories in an analytically fruitful way, it is necessary to spell out a clear time frame and analytical level. For example, "short-term changes might appear as dismantling, whereas the same developments would appear as displacement from a long-term perspective" (Hallonsten and Heinze 2013: 599). This means that even if at the time of the closure of a particular research capacity it appears as if dismantling is occurring, if after a certain period of time the physical and organizational assets previously used in sustaining that capacity are put to use for sustaining another capacity (cf. recombination in the examples above), then in the long run the accurate description of the course of events can be displacement or conversion.

Crucially, therefore, it depends on the point of view of how processes of change are identified, and thus it is key to recognize that changes in the three dimensions of organization, infrastructure, and scientific fields happen at very different rates and through processes (layering, conversion, displacement, dismantling) that are not identical between the dimensions (Hallonsten and Heinze 2013, 2016). In other words, it has already been shown that the model of intercalated change (Fig. 4.2 above) is a functional tool for understanding the renewal of SLAC.

Tracing and Explaining Renewal

The history of SLAC and its contextual embedment was sketched in a previous section to give a background to why the case is particularly suitable for the task at hand. What is missing from that account is a simple but crucial detail: The lab was founded in 1961 with the mission to build and operate a linear accelerator, and this mission was essentially time-limited. When the capacity of this machine to contribute to experimental particle physics was exhausted, the expectation was that it would be closed and the lab shut down. It can be debated whether this was a realistic prospect when the investment was passed by the US Congress in 1961, that is to say, whether the politicians were actually prepared to make such an investment with only a 10–15-year time frame for its utilization (cf. the quote from Panofsky above, about a "good idea" to extend the lab's time horizon). Especially in light of the analysis and discussion in Chaps. 1 and 2 and previously in this chapter, about the survival of National Labs by mission reorientation and recombination as well as political acts of organizational life support, it seems very unlikely that SLAC would have been closed after those 10–15 years. In the context of this chapter's analysis, complementing the perspectives of the previous discussion, it is important to note that the time limit built into the structure of the lab also made renewal and reinvention of the scientific mission of the lab a necessity for organizational survival.

This renewal and reinvention was also protracted for several decades. Three additional major infrastructure projects for particle physics, with their operation cycles reaching well into the 1990s, were accomplished at SLAC before the viability and productivity of the lab as a global actor in particle physics became seriously questioned. Table 4.1 shows the main particle physics machines at SLAC until the final closure of the last one, and their main global competitors.

With the aid of the conceptual scheme as illustrated in Fig. 4.3 above, the overall scientific and organizational transformation of SLAC over 50 years can be characterized as layering (of scientific missions) or

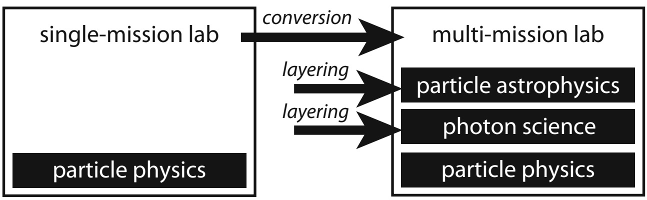

Fig. 4.4 Schematic illustration of the overall, long-term renewal of SLAC

conversion (of the lab organization). Very schematically, from the most generalized perspective and with the longest possible time frame, the renewal of SLAC can thus be illustrated as in Fig. 4.4.

Note that Fig. 4.4, already before moving any further down in level of detail, shows two renewal processes, namely conversion and layering. The layering of photon science and particle astrophysics as scientific activities on top of the existing particle physics program amounts to an overall conversion of SLAC, because the lab is intact with respect to formal organization, legal status, physical location, and general role in the federal US R&D system.

As stated above, the renewal of SLAC can be observed at several different levels, from the top level as shown in Fig. 4.4, down to the micro-actions and -negotiations of individuals as accounted for (in part) by Hallonsten (2015a). It is impossible to account for all conceivable processes of renewal that together have produced the overall renewal in Fig. 4.4, and therefore it is necessary to make an a priori selection of some important changes within the three dimensions of change identified above as the conceptual base for the chapter's analysis: organization, infrastructure, and scientific fields.

In one sense, the schematic overview in Fig. 4.4 takes care of the renewal of scientific fields, although when looking closer at the history of SLAC it becomes a very difficult task to add the missing element in the figure, namely a time stamp on the layering processes that brought photon science and particle astrophysics into the lab. The first photon science activities had begun already in 1972, in the shape of a small-scale pilot program, and two decades later the SSRL was incorporated into the SLAC organization (Hallonsten 2015a). These two dates are hence doubtlessly important, but none of them function unambiguously as a single event whereby photon science was layered on top of particle physics. The synchrotron radiation activities in the early years following their launch in 1972 were truly parasitic and small-scale; they certainly had the blessing of the SLAC director and the machine director for SPEAR but received no active support and were very far from a status equal to the main particle physics activities. This situation changed gradually over the decades, and the change in name from SSRP to SSRL in 1978 signals some increase in significance, as does the 50% takeover of the SPEAR machine in 1979–80 (Hallonsten 2015a: 243–246). When the formal diversification of the mission of SLAC occurred in 1992, SSRL had already taken over the SPEAR machine in full and optimized its performance for synchrotron radiation. Nonetheless, these activities remained comparably peripheral for several more years, as SLAC retained its official status as one of the two major particle physics facilities in the USA. Not least on the level of SSRL directors and scientists, the perception of a second-class citizen status at SLAC was vivid throughout the 1990s, although the situation never threatened the existence or health of the synchrotron radiation activities. All this shows clearly that it would be inadequate to put either 1972 or 1992 as a time stamp on the layering process for scientific fields on overall lab level, and by extension that the layering of photon science on top of particle physics in the case of SLAC can only be identified in retrospect, with a long time frame. It can also be noted here that the term photon science was not introduced at SLAC, as a description of part of its mission, until the LCLS project was underway and the research programs using synchrotron radiation and free electron laser needed a common name. For particle astrophysics the situation is similar; formal organizational units were added to the SLAC organization in the 1990s and 2000s but the field as such entered earlier, most likely as the result of individual initiative and micro-level negotiation.

Naturally, the two dimensions of organization and infrastructure provide less ambiguity when it comes to time stamps for the beginning or end of renewal processes. The layering of new organizational units and divisions as well as the dismantling, conversion, or displacement of existing ones can be traced by looking at series of organization charts, and the dates for the start and stop of operation of infrastructures are similarly noticeable in official sources. For the organization, however, it must be noted that only the formal sides show up in organization charts; that is, there is no real way of knowing the extent to which formal organizational units and divisions have significance in reality or are mere administrative constructs (cf. the discussion in Chap. 1). SSRP/SSRL was part of Stanford University and allowed on site by SLAC management under the premises of it being treated as any outside user group, and the internal organization of SSRP/SSRL followed the Stanford University standards of a "project" until 1978, when it became a “lab,” all within the Department of Applied Physics in the Faculty of Engineering. Interestingly, however, joint appointments of SSRP/SSRL scientists between different Stanford departments, the inflow of grants and other financial support from the NSF and later the DOE, and not least the in-kind contributions of time and equipment from user groups spread all over the country, made the organization of SSRP/SSRL heavily reliant on formal and informal agreements that are difficult or impossible to map because there is little or no trace of them in official documentation, let alone the SLAC organization charts. In addition, much of the agreement with SLAC was of an informal nature. Apart from the basic regulatory document signed in 1972 and renewed in 1978 and 1982, several crucial agreements were made informally, such as the promise in 1976 by SLAC director Panofsky that SSRL be given 50% of the SPEAR running time as soon as PEP had been taken into particle physics operation, or the lack of honoring of this agreement throughout most of the 1980s, which was similarly important since it clearly inhibited SSRL operations (Hallonsten 2015a: 243ff). The question of whether the situation and the relationship between SSRL and SLAC would have been different if these and other agreements had been laid down in formal contracts is difficult to answer; the general impression from the history of SLAC is that power politics, the art of the possible, and coincidence played significant roles in the long-term evolution of the lab.

With regard to infrastructures and the scientific use of these, however, previous analyses have shown (Hallonsten and Heinze 2013, 2016) that the conceptual scheme of four renewal processes can be fruitfully applied in an analysis that yields rich and nuanced knowledge about the complex renewal of Big Science labs that is also stringent in details. Figure 4.5 shows, with meticulous detail, the varieties of change and renewal on the side of infrastructures and their scientific use, sorted on the major machines built in succession at SLAC over the course of 50 years.

There are, as the reader sees, a myriad of different processes identified in this figure. The point, besides displaying a timeline of the gradual renewal of SLAC, is to illustrate the complexity of the conclusions yielded by an analysis of the lab with the aid of the conceptual scheme and the four renewal processes, and to show how this helps in understanding and conceptualizing profound renewal within Big Science labs.

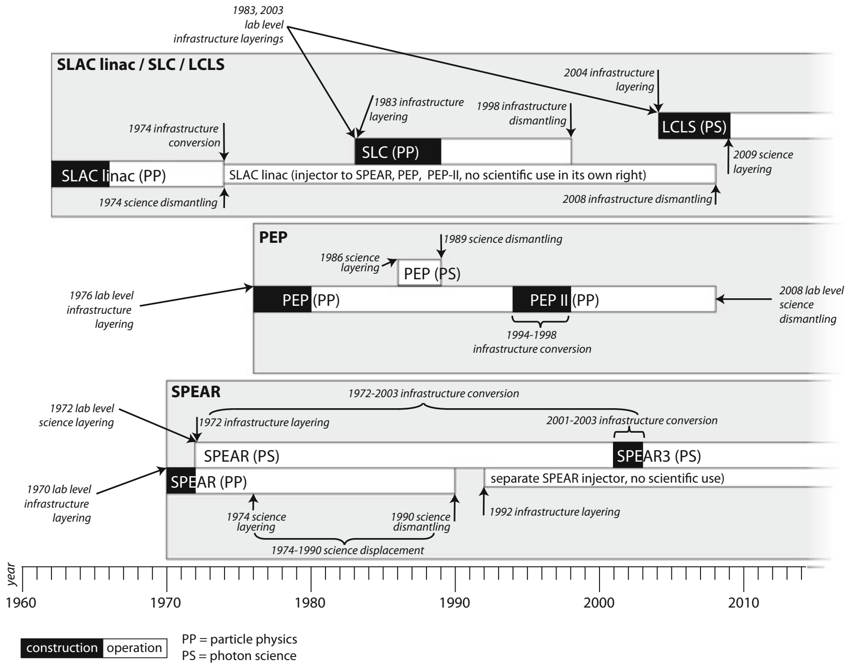

Fig. 4.5 Illustration and timeline of the renewal of SLAC, 1961–2012

In Fig. 4.5, the main pieces of infrastructures are shown as large grey boxes, within which several modifications and varieties are shown as black and white blocks denoting machines under construction (black) and in operation (white). The grouping of machines together in the same grey boxes is made partly out of analytical convenience but is also quite logical: for example, the SLAC linac, the SLC, and the LCLS (top grey box in the figure) are three vastly different pieces of research instruments that facilitate vastly different types of research, but they do share the basic piece of infrastructure, namely the SLAC linac. But to distinguish between these different uses of the machines and the technical varieties, several smaller labels are added that indicate when a piece of infrastructure has undergone a change of one of the four types layering, conversion, displacement, and dismantling, in its technical setup or in its scientific use. In addition, change processes at the lab level, that is, changes that go beyond the use of a single piece of infrastructure, are also indicated by labels that are placed outside of the grey boxes and with arrows showing the time and place of their occurrence. Each new piece of infrastructure that has been added to SLAC during its history is labeled a "lab level infrastructure layering." In addition, the entering of photon science in the shape of the first synchrotron radiation program at SLAC in 1972 is also indicated in the figure with a label saying "lab level science layering."

Proceeding to analyzing each grey box individually, starting with SPEAR, the figure shows that this machine was constructed with the sole purpose of particle physics operation, and complemented the already operating SLAC linac, thus constituting a lab level infrastructure layering at the start of construction in 1970. When the synchrotron radiation activities entered upon completion of the machine in 1972, it was by machine level infrastructure layering (by the adding of new synchrotron radiation or photon science equipment to the machine), and lab level science layering, since this was the first instance of photon science research at SLAC. SPEAR was gradually taken over by synchrotron radiation/photon science research in the 1970s and 1980s, by gradual science displacement (particle physics use ceased, photon science use continued) and infrastructure conversion (the SPEAR ring was rebuilt to fully match the requests of the photon science program); the 1990 closure of the final particle physics experiment on SPEAR was an example of science dismantling (machine level). Two years later, SPEAR was detached from the SLAC linac and connected to its own injector, this constituted infrastructure layering (by the adding of the injector infrastructure, but not science layering since the use remained the same); and the 2001–2003 upgrade of SPEAR to SPEAR3 was an infrastructure conversion of its own that in a sense completed the gradual long-term 1974–2003 infrastructure conversion that took SPEAR from an all-particle physics machine to an all-photon science machine. The SPEAR machine and its injector are still in operation—hence no final infrastructure dismantling there.

Moving on to PEP, this was a major new construction project and thus an example of lab level infrastructure layering. The machine was briefly used for synchrotron radiation experimentation (besides the ordinary particle physics program) between 1986 and 1989, and therefore the processes of science layering and science dismantling are noted inside the grey box in the middle of the figure. In 1994, PEP was closed and rebuilt (1994–1998) into PEP-II, a process of infrastructure conversion (but no science conversion or science displacement since the use of the machine was particle physics also after the conversion). In 2008, PEP-II was closed for particle physics, which did not amount to infrastructure dismantling since the machine is not torn down but awaiting decision regarding its fate, thus the PEP-II (PP) white box ends there but the larger grey box continues. However, the closing of PEP-II constituted lab-level science dismantling since it meant that SLAC no longer runs any particle physics experiments on site.

The repeated recycling of the use of the original SLAC linac over the course of 50 years is shown in the top grey box. Construction of the original SLAC linac started in 1962 and the machine was taken into operation for particle physics research in 1966. In 1974, the machine was taken out of operation for experimental particle physics (machine-level science dismantling), and adapted to use only for injection into subsequent machines (infrastructure conversion). In 1983, an expansion of the linac began by the addition of two major accelerator tunnels that turned the whole setup into a linear collider (the SLC), which meant infrastructure layering at the level of the machine as well as at the lab level. The start of operation of the SLC in 1989 did not mark any renewal process on the side of science, since it took over as the flagship particle physics facility on site; similarly, the 1998 infrastructure dismantling of the SLC did not mean any lab-level science dismantling, since PEP-II opened simultaneously and continued the lab-wide particle physics program for another ten years. When construction of the LCLS began in 2004, by infrastructure layering at both machine and lab levels, it concerned only one-third of the linac; the other two-thirds were still used for injection into PEP-II, a facility that was planned to operate for several more years but that closed prematurely (due to budget cuts) in 2008. The opening of the LCLS for users in 2009 meant science layering at the level of the linac, since this was now for the first time it was used for photon science.

SPEAR3 and LCLS are still in use, both exclusively for photon science. Plans exist to turn PEP-II into a synchrotron radiation source and thus a photon science machine; accurate additions will then have to be made to

Fig. 4.5 to account for layering, conversion, and displacement on various levels and with various time frames. Another expansion (infrastructure layering) of the linac, called LCLS-II, is also underway.

In the beginning of this chapter, the several interconnected resource economies in the ecosystems within which the transformed Big Science emerged and grew strong were outlined on a general level and with the help of some well-documented examples. The analysis here takes a different view: zooming in on one case of renewal with the aid of theoretical tools specifically adapted for this purpose. These theoretical tools have the potential of helping to explain renewal also at the system level, taking whole resource economies or ecosystems into account, although such an exercise would be methodologically and empirically very demanding. The case analyzed here, it shall be admitted, benefits from having a given identified direction of the overall change: from single-mission particle physics lab to multi-mission photon science lab, which makes the analysis and its results somewhat less cluttered.

On micro-level, this change would always begin with an individual or a small group of individuals realizing the scientific potential of a particular type of technology—in this case highly intense radiation produced by accelerators. A letter from Stanford professor William Spicer to SLAC director Panofsky dated June 18, 1968, has been canonized as the micro-level event that first set the wheels in motion at SLAC (Hallonsten 2015a: 228–229), and from this event on, the initial development moved gradually higher up in the hierarchies of the lab and into the realms where the processes at play could be named managerial, organizational, institutional, structural, and political, and in some cases also coincidental. Some kind of blessing from the executive level of SLAC was necessary for the realization of Spicer's plans, and it seems his ability to gather a relatively influential group of Stanford professors to approach SLAC director Panofsky with a proposal played a role, as did reportedly the scientific vision of Panofsky himself in realizing the potential of synchrotron radiation. It is of course reasonable to ask why these early synchrotron radiation activities were at all let onto the site of the single-mission particle physics lab SLAC, laden as it was with tremendous expectations and demands from funders, scientific elites, and other stakeholders, and in a situation of harsh international competition. A likely contributing factor is that SLAC in the early 1970s still operated within a lab system that granted more managerial discretion to the level of the directors than today; as noted in Chap. 2 as well as above, the US National Lab system was still overseen by the essentially elite-governed AEC, and SLAC furthermore enjoyed status as a key asset in the federal US operation of Big Science, which was still of vital national interest. The subsequent governance reforms to the National Laboratory system evidently created another level of rigidity and added red tape that perhaps would have inhibited the same micro-level diversification of the lab mission into photon science, if it had been proposed a decade later.

The work of institutional entrepreneurs like Spicer and eventual SSRP/SSRL directors Sebastian Doniach (director 1974–1978) and Arthur Bienenstock (director 1978–1998), and of SLAC directors Panofsky (director 1962–1984) and Richter (director 1984–1999) seems to have been absolutely crucial in the early phases of change, and these individuals also seem to have made their achievements both with the help of, and in spite of, institutional arrangements (Hallonsten 2015a; Hallonsten and Heinze 2013). This shows the absolute necessity for the analyst of these historical events of keeping an open mind towards both explanatory models building on the acknowledgement of the importance of individual (rational) action, explanatory models from institutional theory, and explanatory models from political theory. While individual actors, acting (partly) out of self-interest, are identified as crucial in the early parts of the history, the institutional environment and political frameworks emerge as especially crucial at later stages. The incorporation of SSRL into SLAC in 1992 was evidently the result of purposeful work on behalf of the funder (the DOE), Stanford University, and SLAC director Burton Richter, and it appears the process was driven (partly) against the will of leading synchrotron radiation scientists at SLAC (Hallonsten 2015a: 265–267). However, the end result was a strengthening of the synchrotron radiation activities on site and the opening of a path towards photon science as the main experimental activity of the lab.

Moving closer to today, it is doubtful whether the LCLS would have had the success it eventually got unless the DOE would have acted in its favor, as part of the department's strategy to keep labs alive by granting them major infrastructure projects according to what was called the

"Trivelpiece legacy" in Chap. 2. When the LCLS was proposed by SLAC in the late 1990s, it was a prototype facility designed to give the free electron laser concept some technical trials as a preparation for a real future user facility. As prospective funder, the DOE refused to support such a machine and urged SLAC to develop a full-fledged user facility concept out of the LCLS, which it did and which was consequently funded by the DOE in 2004 (Hallonsten and Heinze 2013: 595).

Against this background, it is also instrumental to acknowledge what the analysis conducted above manages to reveal and what it fails to reveal. Although certainly achieved by the deliberations and actions of individuals, the renewal process of SLAC was deeply institutionally entrenched. Figure 4.5 shows a timeline of infrastructures which in this case means enormous pieces of concrete, vacuum chambers in the shapes of several 100 meter-long pipes, superconducting magnets, and several other huge but technically very sophisticated components—in some cases buried several meters below the ground—and therefore the figure also implies structural determinism, or even material determinism, as a factor of explanation in the renewal. Furthermore, looking at the figure, it would seem as if the resilience of these infrastructures supersede also the inertia of institutions that has been highlighted and conceptualized by (organizational) sociologists as fundamental to any organizational field and to modern society itself. But change is nonetheless accomplished, which the figure also manages to convey.

Thus, although the writing of the history of SLAC with a one-sided focus on the infrastructures it has operated through the decades is oversimplified and would not give justice to the full range of micro-level processes that together bring about long-term change (cf. Hallonsten 2015a: 222–224), it is natural to use the succession of machines as a red thread in the analysis. The infrastructures constitute powerful symbols of lab identity and culture, but most importantly, they are the key resources in the scientific programs of SLAC. There would, in the context of this book, be no Big Science without big machines.

This is linked to the institutional inertia of SLAC (and similar organizations) in a wider sense. In the 1970s and 1980s, SLAC had long-range scientific, organizational, and infrastructural plans, partly codified in reports and documentation provided for its funding/oversight agency (the DOE) and its operations contractor (Stanford University), where synchrotron radiation played a minor role at best as an auxiliary activity. This remained so for a long time, even after the annual number of users at the SSRL had superseded the annual number of users of the particle physics program at SLAC, and after the single-mission status of SLAC was abolished. Indeed, it took until 2005–2006 before the shift of SLAC from primarily a particle physics lab to primarily a photon science lab was seen in the lab budget on the level of the DOE and federal R&D expenditure: as seen in Fig. 4.6, it was only after the start of construction of the LCLS in 2004 that the photon science share of the SLAC budget started to grow into the shoes of the particle physics share of the budget, which also in 2012, four years after the closing of the last particle physics experiment at PEP-II, was significantly larger than the photon science budget had been in 2004. What this amounts to is the realization that the renewal of SLAC (and of Big Science generally) was inhibited, slowed down, and prevented by prevailing strong institutions on the level of the lab organization, politics, and scientific communities. A quite unsurprising realization, given what the field of organizational sociology (and sociology in general) has established regarding the influence, resilience, and inertia of institutions in nearly all aspects of social life.

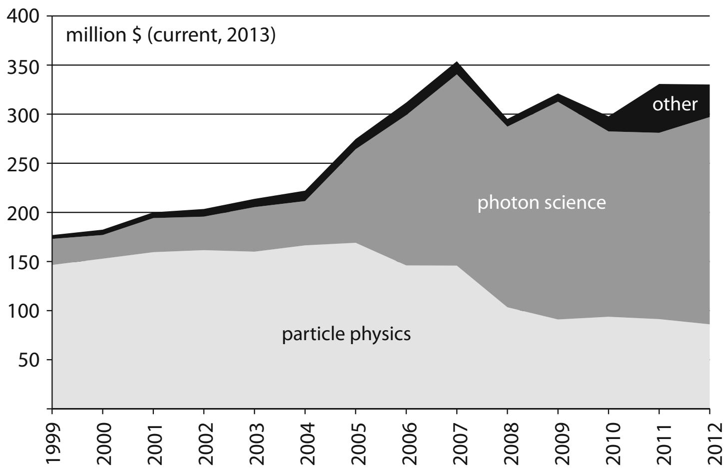

Fig. 4.6 Overall federal expenditure on SLAC, 1999–2012, divided on major budget line items (*Source:* Actual appropriations listed in the yearly budget requests from the DOE to the US Congress)

But importantly, change occurred. Institutions not only prevent change but also facilitate change. In the particular case under study here, and also in a global perspective of old and transformed Big Science, it is clear that synchrotron radiation would not have entered at all if not the basic infrastructures in the shape of particle accelerators built for whole other purposes would have been available (and made available) at SLAC (and other labs). Thus, while it can be argued that the single-mission status of SLAC was inhibitory to change in the long run, the existence of a single-mission particle physics lab, quite generously funded, was also a prerequisite for the change to begin in the first place. This should not be seen as a paradox but as a natural feature of complex change processes.

In other words, the success of the long-term renewal process of SLAC, which has taken the facility from being a single-mission particle physics lab to (almost) a single-mission photon science lab, is attributable to the gradual, incremental, and cumulative nature of the renewal. It allowed the new research activities that used synchrotron radiation and later free electron laser to gain crucial strength exactly because they were built up in a slow process that enabled the building of crucial alliances and support, or put differently, the building of the social legitimacy that institutional theory has forcefully shown is a general crucial factor for the survival of organizations. Thus, while the partial replacement of old Big Science by transformed Big Science perhaps occurred partly as a result of radically changed framework conditions—of which many have been briefly mentioned above and not least discussed in Chap. 2—the analysis here clearly shows that the change from the old to the transformed was facilitated and enabled in part by gradual, incremental, and cumulative renewal of existing lab organizations like SLAC, or the Argonne, Berkeley, and Brookhaven National Labs.

The connection to "generic technologies" (Shinn and Joerges 2002, see Chap. 1) should be noted: organizations like the National Labs exhibit a similar pattern as generic instruments, and could perhaps be thus described as "generic organizations." The comparison is intriguing not least because the key to the resilience of labs like SLAC is their infrastructures, which are evident examples of generic technologies. This chapter has shown that infrastructures are the means by which these organizations can renew and survive, by recombination and thus gradual but necessary renewal.

5

Users and Productivity

User Facilities on Global Markets

A key feature of transformed Big Science that distinguishes it from the old is that the big organizations are support organizations for small or ordinary science projects. As noted especially in Chaps. 2 and 3, the primary areas of use of neutron scattering, synchrotron radiation, and free electron laser labs, and thus the ultimate motivation for their existence, are the materials and life sciences (broadly defined). The users are ordinary academic (as well as institute and industry) scientists, and the labs are attuned to the experimental needs of this broad and varied user community, which makes the scientific output of the labs crucially dependent on the performance of the users. In one strict sense, it is even inadequate to talk about a scientific output from neutron scattering, synchrotron radiation, and free electron laser labs, because they are most of all tools used by research teams from universities and institutes who make temporary visits to the labs to do experiments or measurements, often of a cutting-edge character, as part of their ordinary work in home labs and institutes. These researchers are the ones creating scientific output, and there is a clear

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division of labor between users and facilities, which has its exceptions (see Chap. 3), but which is defining for the transformed Big Science.

In combination with globalization and the internationalization of science (Crawford et al. 1992; Geuna 1998; Mayer et al. 2014), the division of labor between users and facilities has created a global market where labs compete for users that can secure their scientific output and thus guarantee that they fulfill their missions. To illustrate this, data from the Department of Energy (DOE) shows that only 11% of the total users at the six major neutron scattering, synchrotron radiation, and free electron laser facilities in the system of US National Labs[1] in fiscal year 2014 (October 2013–September 2014) work at the respective lab's host organization, and that 17% of all the users are from abroad. The free electron laser LCLS, so far a facility that provides some unique experimental opportunities and which thus should expectably have a global user community, stands out with no less than 51% of its users affiliated with non-US institutes and organizations. For the neutron scattering and synchrotron radiation facilities, the figures are lower: between 80 and 87% of their users are affiliated with US universities, institutes, and other R&D organizations. But only 36% of the total users are based at institutes and organizations within the home state of the labs, all of which suggests that their main mission lies in serving national and international user communities (DOE User Statistics 2014). Seen from the perspective of the users, these statistics show that spatial proximity shall not be overestimated but also not completely discarded (see also Chaps. 6 and 7): users are prepared to travel far to get access to the right instrumentation, but neutron scattering and synchrotron radiation facilities exist in several parts of the world and have naturally developed user communities with some regional and national emphasis.

Certainly, user facilities existed in the era of old Big Science as well. Several nuclear and particle physics labs of the Cold War era had a mission

[1] The neutron scattering facility Spallation Neutron Source (SNS) at Oak Ridge National Lab; the synchrotron radiation facilities Advanced Light Source (ALS) at Lawrence Berkeley National Lab, Advanced Photon Source (APS) at Argonne National Lab, National Synchrotron Light Source (NSLS) at Brookhaven National Lab, and the Stanford Synchrotron Radiation Lightsource (SSRL) at SLAC National Accelerator Laboratory; and the free electron laser facility Linac Coherent Light Source (LCLS) at SLAC National Accelerator Laboratory.

to serve external users on the basis of open competition and peer review assessment of proposals, but in a significantly more limited sense. The US National Labs were in a basic sense "user facilities" at their inception; one of the two missions given to the multiprogram labs created in 1946–1947 was the operation of lab resources and research infrastructures too big for universities and industrial firms to afford, and the provision of these for use by the nation's scientific communities, academic as well as commercial (Westwick 2003: 119–120; see also Chap. 2). As long as particle physics accelerators and detectors were of sizes that allowed experiment groups of tens of people, the labs were at least in part run as service facilities for the nationwide and international particle physics communities. But the presence of militarily classified work on the sites and some lack of clarity regarding the regulations for openness to outsiders complicated the picture, as noted by Westwick (2003: 149): "In principle, the labs intended to provide large facilities for use by visiting academic scientists. In practice, outside users did not enjoy easy access to them." Bodnarczuk and Hoddeson (2008: 516) note that particle physicists who visited facilities within the National Labs system to do experimental work often complained about their needs being secondary to those of the in-house scientists of the labs.

The real exception seems to have been Fermi National Accelerator Laboratory (Fermilab), inaugurated as the second single-mission US National Lab for particle physics (after SLAC) in 1974. The first Fermilab director Robert Wilson was determined to accommodate the needs of external user groups (Hoddeson et al. 2008: 157ff) and thus "redefined the concept of national lab" by making the provision of facilities for visitors the core meaning of "national" (Westwick 2003: 272). But the time frame was short. The transformation of particle physics into "megascience" necessitated larger teams, longer experiment runs, experiment "strings" (Hoddeson et al. 2008: 262–280), and long-term (several years) planning, all of which made it practically impossible to continue routine day-to-day operation of Fermilab and other particle physics labs as user facilities; instead, it merged experiments with instruments (detectors) to create centrally planned conglomerates of longer-term and larger organized teamwork (Knorr Cetina 1999: 159ff). The development was similar at SLAC, DESY in Hamburg, and CERN in Geneva, and seems to have been propelled by the squeeze on spending in the late 1970s and beyond in both Western Europe and the United States (see Chap. 2). As a result, teams sought to hold on to their investments and monopolize the output of their instrumentation as long as they could, and if possible, prevent other groups from capitalizing on them (Bodnarczuk 1997).

Neutron scattering, synchrotron radiation, and free electron laser labs, on the other hand, are at least nowadays almost completely directed to the cause of providing services to outside user communities. As described in Chap. 3, the principle of open competition for experimental time and its allocation on the basis of peer review committee evaluation is key to most of these labs, although some exceptions exist: companies can buy time, and in some cases local or national user communities are prioritized in the allocation. But fundamentally, transformed Big Science labs are big scientific facilities used for a wide breadth of ordinary science projects from ordinary science organizations. As was also mentioned in Chap. 3, this has some implications for the issue of defining, measuring, and evaluating productivity and quality of output of the labs.

Productivity

All neutron scattering, synchrotron radiation, and free electron laser labs are technically, scientifically, and organizationally unique, but they share some fundamental features (see Appendix A.1). All operate a certain number of independent experimental stations that provide specialized and sometimes unique experimental opportunities. Synchrotron radiation and neutron scattering labs typically operate all or most of these stations in parallel, so that several teams in very different disciplines can conduct their work simultaneously in complete independence of each other. Free electron lasers, on their part, typically only operate one station at a time, and can therefore normally accommodate a significantly lower number of users, which also shows in the comparably lower quantity of their scientific output[2] (see also below).

[2] For comprehensive technical descriptions, see Appendix A.1.

Obviously, the size and fundamental technical setup of a lab matters. A key difference between neutrons and x-rays/light is that it is difficult and extremely technically demanding to focus neutrons, whereas x-rays/light can be both focused and diverted comparably easily with lenses, gratings, and mirrors (see Appendix A.1). The performance of neutron scattering facilities therefore depends mostly on "brute force," that is, the effect of the accelerator/reactor, which determines the amount of neutrons possible to produce at once, and thus the crucially important flux (flow rate per unit area). This sets a fundamental cap not only on performance (in a qualitative sense) but also on productivity because the experimental stations need to share the source and the neutron bursts that it produces. Not much can be made to improve the neutron beams after they have been extracted from the reactor or spallation target station. Nonetheless, the Institute Laue Langevin (ILL) in Grenoble operates over 50 experimental stations in parallel.

Synchrotron radiation and free electron laser laboratories have a similar fundamental restriction, although it has only to do with type and size of the accelerator; the quality of the particle beam inside it and the various technologies that produce, focus, and optimize the radiation beams before they reach experimental stations can be enhanced dramatically to improve the quality of the radiation. What sets a limit to the productivity (in quantitative meaning) of synchrotron radiation laboratories is simply how big their storage rings are, which determines how many beamlines they can host. The circular storage ring that makes up the technical centerpiece of the European Synchrotron Radiation Facility (ESRF) is 844 m in circumference and serves no less than 40 separate beamlines (as of January 2015), which means that in principle, at any given time, the ESRF can support the simultaneous work of at least 40 independent user groups (some beamlines support several stations that can operate simultaneously). In sharp contrast, the LCLS at SLAC which is the world's first x-ray free electron laser has six experimental stations placed in a row, served by a 1 km linear accelerator, which means that the LCLS can only support one running experimental station at a time (see Fig. A.3 in Appendix A.1). A comparison between the ESRF and the LCLS is therefore fundamentally unfair but in a narrow sense relevant: Technically, the ESRF can facilitate more than 40 times as much experimental work than the LCLS.

This also shows in the output of these labs: While in 2014, the operations costs of the ESRF were €102.2 million or roughly $113 million, well on par with the operations costs of $127 million of the LCLS, the ESRF had a publication output of 1782 journal articles, which is more than 25 times higher than that of the LCLS (67 journal articles) in the same year (ESRF Highlights 1994; Hallonsten 2014b: 492–493). Clearly, however, as the LCLS facility itself advertises, the role of the LCLS in the science system is significantly different than that of the ESRF: while the latter is built to serve a broader scientific community (including of course also some cutting edge experimental work), the former is the first facility of its kind globally and was built to support only the absolute cutting edge experiments in physics, chemistry, and biology, and not in the least to foster entirely new and revolutionary experimental work at the intersection of these fields (cf. Heidler and Hallonsten 2015: 305).

Just like most organizations in the public science system, the transformed Big Science labs experience a pressure to demonstrate their productivity, output, and money's worth. This is most of all a consequence of the developments that have been described in previous chapters with the help of the concepts of "economization" (that investments in science are motivated by expected contributions to economic development/growth), "commodification" (that science's output is measured by economic criteria), and with reference to the more general concept of the Audit Society, where performance monitoring and appraisal become virtues in themselves. The most rudimentary measure of performance of a Big Science lab is its technical reliability, which from the perspective of the users translates to availability or the percentage of scheduled operation that is actually delivered without interruption. Some labs advertise a high reliability, between 98 and 99%, and this also seems to correspond (in part) to a high reputational regard for the labs in its user communities, which in itself is a non-negligible factor in assessing the quality or performance of a Big Science lab from a user perspective. The science system is to a large extent a "moral economy" where the abstract but highly valuable "credibility" (as Latour and Woolgar 1986/1979 would call it, see also Hessels et al. 2009) or "reputation" (in the words of Whitley 2000/1984) is the key resource and the key marker of success. Credibility can also be seen as a commodity that buys most other vital commodities such as funding or formal recognition (Hallonsten 2014c), and this connection will also be explored in more detail below. Here, it should be underscored that technical reliability is obviously no measure of scientific productivity. There is probably a correlation, as high technical reliability should logically attract highly skilled users and the competition for access should also logically be greater at labs with higher technical reliability, which should favor "better" users. But there is no absolute causal relationship between high technical reliability and high quality of the research activities of the users. Furthermore, the reliability measures advertised by labs themselves (i.e. percent of uptime) refers merely to the accelerator/reactor and says nothing about the reliability or performance of all those instruments and technical setups that are just as crucial for successful experimental work and also are the technologies that users interface directly with. Therefore, while high technical reliability surely is a sign of strength for those labs able to achieve it, it is not an adequate measure of productivity, let alone quality.

Another marker of quality or performance of a facility is demand for experimental time, suitably measured by oversubscription rates (demand in relation to supply, which can be measured in different units, see below), which are slightly more dynamic and describe a kind of market value of the collected experimental facilities at a neutron scattering, synchrotron radiation, or free electron laser lab. Table 5.1 shows the demand and supply of experimental time at the synchrotron radiation facility ESRF, as categorized and noted in the annual report (ESRF Highlights), and the corresponding oversubscription rates, in 2014. Some remarks are necessary. First of all, as discussed in Chap. 3, disciplinary categorizations like this one are highly ambiguous and should not be given too much weight. Furthermore, also noted in Chap. 3, this particular categorization does not correspond to groups of beamlines but runs across them, which means that Table 5.1 says nothing about the relative popularity of specific beamlines and experimental stations at the ESRF. But the table nevertheless manages to convey that oversubscription rates vary, which can be interpreted in different ways: A lower oversubscription rate would imply a better match between supply and demand of experimental time in a specific disciplinary category, and thus a high oversubscription rate is either due to extreme popularity of some instruments, or lack of prudence or ability of the lab to foresee shifts in the demand of experimental

Table 5.1 ESRF oversubscription rates in 2014, divided into main scientific areas, calculated on the number of requested and allocated eight-hour shifts of experimental time

	Number of requested shifts	Number of allocated shifts	Oversubscription rate
Chemistry	4338	1775	2.44
Hard condensed matter	9380	3165	2.96
Applied materials science	5055	1652	3.06
Engineering	237	117	2.03
Environment	832	249	3.34
Cultural heritage	332	144	2.31
Earth sciences	1835	819	2.24
Life sciences	1423	549	2.59
Structural biology	3061	2077	1.47
Medicine	1058	465	2.28
Methods and instrumentation	492	201	2.45
Soft condensed matter	3157	1077	2.93
Total	**31200**	**12290**	**2.54**

Source: ESRF Highlights (2014)

time in various disciplinary communities, or a combination of the two. Another way of calculating oversubscription rates, by which other features of the supply and demand of experimental time is expressed, is the ratio between total number of applications and the number of approved applications, and this is a measure that also says more about the general competition, because it is neutral with respect to the differences in average amount of experimental time of different disciplines and experiments. Table 5.2 contains overall numbers for the ESRF and its US counterpart (and main global competitor), the APS.

At first sight, the comparison between these two labs yields that the ESRF is significantly more popular overall than the APS (cf. Hallonsten 2013a), and perhaps, by extension, that it is a better user facility on the whole, taking technical, organizational, social, and other factors into account. But the ESRF and APS were both once built as major new additions to the synchrotron radiation capacity of the European and North American continents, and to serve very large user communities. In light of this, a somewhat lower oversubscription rate suggests a better match

Table 5.2 Oversubscription rates for ESRF and APS, counted on the basis of submitted and granted proposals, for calendar year 2010

	ESRF	APS
Submitted proposals	3242	1671
Granted proposals	1306	883
Oversubscription rate	*2.48*	*1.89*

Source: Hallonsten (2013a: 506–507)

between supply and demand, and thus the difference seen in Table 5.2 could also be read as a sign that the APS is somewhat better attuned to its user community than the ESRF, and that more purposeful and adequate work has been done at the policy level to match user communities with the new opportunities opened by the investment in the APS. This relates to the discussion at the end of Chap. 2, where the case of MAX-lab in Lund, Sweden, was used to illustrate that there is a need to cultivate user communities through policy measures and funding programs separate from the investment in facilities and labs. A neutron scattering, synchrotron radiation, and free electron laser facility will not be made to produce science that matches its technical capacity unless the skills and competences of its user community are actively cultivated and allowed to thrive accordingly.

High oversubscription rates may also have other complementary causes outside the popularity of instruments. They may be caused by behaviors in the user communities such as excessive hedging of bets, meaning that some users apply for experimental time at several facilities for the same study or experiment, in the hope that at least one of their applications is successful. By extension, there is also a risk of "asymptotic behavior" (Hallonsten 2013a: 511), that is, excessive oversubscription rates may scare users off and make the rates drop suddenly, something that would impact the overall measure but would be a hidden variable.

Oversubscription rates are of course also deeply (but vaguely) tied to the internal and informal ranking systems of science by which scientific communities keep track of available experimental resources and rank them in value and prestige. These are crucially important to the social organization of science but typically incomprehensible for the outsider, and a wide and varied range of factors play into it; in the case of infrastructures and experimental resources like transformed Big Science labs, the technical performance and reliability counts (uptime, discussed above, as well as the performance of specific instruments) in addition to the skills of the technical support personnel, and also the safety rules that restrict the use of certain samples or certain modifications on the instruments. The oversubscription rate is therefore, besides a general measure of the popularity of an instrument or of the balance between supply and demand of access to the type of instrument in question, also an indication of prestige and recognition that, moreover, corresponds to the prestige and recognition among peers that are conferred to those scientists that manage to get access.

The Difficulties of Measuring Scientific Output

The growing competition for funding and recognition in science that has followed from economization, commodification, and several other developments (see Chap. 1) is seemingly less direct and accentuated among (transformed) Big Science labs than, say, between universities competing for what has become known as "excellence funding" and other performance-based funding and markers of prestige[3] (Wildavsky 2010: 88–95; Münch 2007: 73ff). But harsh competition and meticulous performance measurement are also key features of contemporary Big Science, and distinguishes transformed Big Science from the old. Most importantly, as suggested above, the transformed Big Science operates partly in accordance with a logic shaped by economization and commodification: Labs providing scientific communities with neutron scattering, synchrotron radiation, and free electron laser compete on a global market for the best users in a variety of scientific disciplines. This is not only a distinguishing feature of the transformed Big Science that reflects overall developments in the governance and organization of science; it also suggests that broader and simultaneously more direct interfaces between labs and transnational multidisciplinary user communities have developed and are allowed to define much of their governance and organization.

[3] Obviously, performance assessment has always been an important feature of the social organization of science (see e.g. Merton 1957, 1968; Shapin 1994), but the use of evaluation in current science policy, governance, and organization is at an unprecedented scale and it does, most likely, influence the social organization of science and the content of scientific work itself, at their very cores (Butler 2003; Kyvik 2003; Weingart 2005).

This has several important implications that not least have to do with questions of how user-oriented Big Science labs can measure their performance and output as part of their efforts of proving their competitiveness, and what ramifications such measures and their implementation will have. Transformed Big Science labs compete in providing the best possible conditions for experimental work and attract the best users, and some do (by necessity or by choice) use common metrics for performance assessment, including (but not limited to) various bibliometric measures, which will have certain specific consequences, but which also have alternatives.

Publication lists are compiled and published on lab websites and publication counts are advertised in annual reports, not seldom with notes highlighting what is claimed to be very high levels of productivity. Since the users of the labs typically are ordinary research groups, the lion's share of the scientific output of the facilities emerges in ordinary journals (and edited books, conference proceedings, etc.), which makes it principally possible to compare the output of a transformed Big Science lab with any organizational unit in science, or even with journals, using standardized statistical measures such as impact factor (Heidler and Hallonsten 2015). But it is of course, once again, important to point out the fundamental organizational division of labor between the labs and their users, and what this means for measuring the productivity of transformed Big Science labs. Strictly speaking, the labs themselves produce few or no scientific results, and only a tiny fraction of the publications that communicate results obtained at these labs are authored solely by employees of the labs. Staff scientists of the labs of course conduct research of their own and publish, but such output is normally miniscule compared to the combined output of all external users. It is very common for staff scientists to be listed as coauthors on publications, which is typically due to their important role as mediators between instruments and users and thus facilitators of crucial experimental work, but if authors are counted, nearly all scientific output of neutron scattering, synchrotron radiation, and free electron laser labs is produced by scientists employed by other organizations than the labs, such as universities and research institutes. This means a fundamental practical complication for the assessment of the productivity of these labs: there is no clear-cut way of establishing the connection between a publication and the facility where experimental work was conducted that contributed to the results of this publication. Most of all, it is not possible to obtain a reliable publication count simply by using regular sources such as the Web of Science (WoS) or Scopus. Although labs are quite eager to publish lists of publications that communicate results obtained with their instrumentation, and often also have rules that oblige users to report publications, labs can never be entirely assured that all relevant publications have been collected, and there can hence never be any certainty that complete lists have been compiled.

A number of analyses were made of the data contained in complete publication lists for 2014 for the synchrotron radiation facility ESRF, the neutron scattering facility ILL, and the free electron laser facility LCLS available through the databases and lists on the websites of the respective labs.[4] These three cases were chosen because they are well established (i.e. they have been in operation for several years and should have consolidated user communities and stable operations as well as efficient user support organizations), and all three also represent the top segment of facilities in each category as defined informally but generally, on the basis of the historical exposés in Chap. 2 and other sources (Hallonsten 2013a; 2014b; Rush 2015). The analyses made involved searches in the Web of Science (WoS) database, a manual sorting and categorization of the publications in the lists to make some disciplinary classifications and identify staff scientist co-authorship, and a full-text search of a limited sample of articles from each set of publications to identify mentioning of facilities and instruments in the main text as well as acknowledgements (see below). It is important to note here that analyses of publication lists with the help of the WoS and other tools have some drawbacks that lower the

[4] The following sources and delimitations were used: ESRF and ILL keep a joint open searchable publication database online (http://epn-library.esrf.fr:9090/flora_illesrf/servlet), and the LCLS publishes a list at its website (https://portal.slac.stanford.edu/sites/lcls_public/Pages/Publications.aspx). For the ESRF, a search was made with the filter that rendered a list of publications with or without ESRF-employed authors "describing an ESRF experiment" for the year 2014. The result was a list of 1818 publications. The same was made for the ILL, filtering out publications with or without ILL-employed authors "describing an ILL experiment" for the year 2014, which yielded a list of 496 publications. For the LCLS, which separates publications reporting "X-ray Science Performed at LCLS" and "FEL & Accelerator Science Performed at LCLS," only a list of publications in the former category was extracted, which contained 70 items.

general level of reliability of the results of the analyses. Publications and publication lists and databases, and the bibliographic and bibliometric data they contain, make up mere proxies of the results produced in science, and are therefore not clear-cut or perfect sources for analyzing the quality and relevance of these results, the sociological configurations of scientific work, the organization of collaborations, and so on. The following analysis and the data presented in Tables 5.3 and 5.4 should therefore be taken only as an input to a qualitative discussion that seeks to maintain qualitative nuance. The data shall not be left to speak for itself but instead be part of a broader discussion that feeds into the overall analysis.

The publication lists obtained contained 1818 (ESRF), 496 (ILL), and 70 (LCLS) entries, of which 1782 (ESRF), 491 (ILL), and 67 (LCLS) were articles published in journals (the remainder were book chapters and conference papers). Only a tiny fraction of these journal articles mention in their titles that the work they present has been done (in part) at the ESRF, ILL, or LCLS. Not even use of the techniques (neutrons, neutron scattering, synchrotron radiation, free electron laser, x-ray laser) is very commonly mentioned in article titles, although for the ILL and the LCLS this latter figure is significantly higher than for the ESRF (see Table 5.3). In the case of the LCLS, this is expectable and highly natural: the technique is new and groundbreaking (cf. the discussion on the difficulty of defining the experiments in Chap. 3) and so results are probably to a greater degree tied to the specific technique. That something similar seems to also be the case for the ILL is somewhat

Table 5.3 Shares of the 2014 journal articles of the ILL, ESRF and LCLS publication databases/lists that mention facility names or technique in their titles

		ESRF	ILL	LCLS
Total number of articles that mention the facility name in the article title	#	15	8	3
	%	0.84%	1.62%	4.48%
Total number of articles that mention the technique used in the article title	#	74[a] (339[b])	163[c]	22[d]
	%	4.15% (19.0%)	33.2%	32.8%
Total number of articles in the sample		1782	491	67

[a]"synchrotron radiation"
[b]"synchrotron radiation," "x-ray" or "x-rays"
[c]"neutron scattering", "neutron" or "neutrons"
[d]"free electron laser," "x-ray laser"

Table 5.4 Results of full-text searches of limited sets of 2014 journal article publications from the ESRF, ILL, and LCLS

		ESRF	ILL	LCLS
Total number of articles in the sample		174	70	63
Articles with				
… coauthor affiliated with facility	#	67	52	35
	%	38.5%	74.3%	55.5%
… facility mentioned in text/abstract	#	124	56	61
	%	71.3%	80.0%	96.8%
… facility mentioned in acknowledgements	#	121	37	52
	%	69.5%	52.9%	82.5%
… facility not at all mentioned	#	7	3	0
	%	4.0%	4.2%	0.0%

surprising given that neutron scattering nowadays should be considered a conventional and well-established technique, but the figures for ILL and LCLS are only high in relation to the ESRF, and the comparison is somewhat unfair, since "neutron scattering" and "neutrons" make up much more fundamental descriptions of the technique than "synchrotron radiation." Consequently, by including "x-ray" and "x-rays" in the search on the ESRF article titles, the figure increases to 339 (74 articles mention "synchrotron radiation" in their title, 301 mention "x-ray" or "x-rays," and 36 mention both and are hence overlaps), or 19%. This is still lower than the ILL, which might be explained by the differences in pure volume of synchrotron radiation and neutron scattering use: As mentioned in Chap. 3, synchrotron radiation has an estimated three to four times more annual users in Europe and the United States than neutron scattering, and this might have the effect that the use of neutrons still, even after several decades of routine operation of facilities like ISIS, the ILL, and several smaller reactor-based sources in both Europe and the United States (see Appendix 2), remains less mainstream and less taken for granted. This could explain why roughly one-third of the ILL articles from 2014 mention "neutron" or "neutrons" in their titles.

The point of the analysis presented in Table 5.3 is, however, that any search for the full names of acronyms of neutron scattering, synchrotron radiation, or free electron laser facilities in the article title field in a publication database like the Web of Science (WoS) will yield a grossly incomplete result, and that a mere glance at the title of an article in a journal will usually not reveal if the results the article communicates have been obtained at a specific facility. For the three labs in Table 5.3, a WoS search for facility name (both full name and acronym) in the article title field and restriction to the year 2014 gave six hits for the ESRF and 22 for the LCLS. For the ILL, the name Institute Laue Langevin gives four hits for 2014, but the acronym “ILL” is a useless search term because the search engine also misreads it for the word “ill” which gives 1984 hits.

A similar WoS search, but in the “topic” field instead of title, gives access also to the abstracts and the author keywords of articles, besides title, and could hence possibly render a more representative outcome, but did not. A search for ESRF (full facility name as well as acronym) in the topic field, also restricted to the year 2014, gave a total of 126 hits, in other words less than one-tenth of the 2014 articles in the publication database. For the LCLS, the same search gave a total of 154 hits, which of course is a flawed result given that the total of 2014 articles listed by the LCLS itself is only 67. A closer look at the results explains why: the plural version of the acronym for “lymphoblastoid cell lines,” a specific type of laboratory-bred cell culture with specific relevance for molecular cancer research, reads “LCLs” and is mixed up with the acronym LCLS in the search, which means that the results are mixed with a large number of articles on genetics and oncology. For the ILL, the shortcoming of the acronym as a search string was noted above. The search for the full facility name Institut Laue Langevin in the field topic gave only 17 results, whereas the acronym ILL gave no less than 16230 hits, of which it can be assumed only very few are related to the neutron scattering facility in question, but intermingled with every article from 2014 that is indexed in the WoS and has the word “ill” in its title, abstract, or keywords. Articles can also be searched for in WoS using author names and affiliations, but also the latter would yield an incomplete result in this case since it would miss those articles that have no coauthor affiliated with the facility in question (see Table 5.4).

What remains are of course full text searches of articles to detect if facility names appear in, for example, their methods sections, but this is not provided by the WoS or any other similar publication database. The following analysis of a limited set of articles from the three facilities in question (ESRF, ILL, and LCLS) was therefore undertaken not as a method template for tracing publications reporting results obtained at these or other facilities, but as an investigation of the extent to which facilities are identified in articles, and how/where. Downloading full text versions of the whole samples of 1782, 491, and 67 articles from the ESRF, ILL, and LCLS in 2014 was not feasible, but a smaller selection was made on the basis of the following criteria. Nine high profile journals were selected, all having both a high impact factor (as measured, calculated and published in the WoS and similar databases) and a high informal reputation in the scientific communities. These were: *Nature, Science, Cell, Physical Review Letters, Advanced Materials, Nano Letters,* the *Journal of the American Chemical Society (JACS), Proceedings of the National Academy of Sciences (PNAS),* and the *Journal of Applied Crystallography*. Out of the ESRF set of 1782 articles used above, 125 are published in one of these journals, but none of the journals are among the five most common in the set (the *Journal of Applied Crystallography* is seventh, with 35 articles, followed by PNAS in twelfth place with 23 articles, and JACS is thirteenth with 21 articles). Therefore, in order to expand the sample to include close to 10% of the very large full set of 1782 articles and to increase the disciplinary breadth, three additional journals were included, namely the *Angewandte Chemie International Edition*, the *Journal of Physics: Condensed Matter*, and *Environmental Science and Technology*, all of which are also renowned publications in their respective fields. This increased the sample to 174. For the ILL, only six of the nine designated high-profile journals (above) were represented (*Nature, Science, Physical Review Letters, JACS, PNAS,* and the *Journal of Applied Crystallography*, and none of them was among the top five), and a mere 34 articles were published in these; consequently, the sample was expanded with the articles published in *Soft Matter*, *Macromolecules*, the *Journal of Alloys and Compounds*, and *Dalton Transactions*, increasing the sample to 70. For the LCLS, all articles that appeared in journals indexed in the Web of Science in 2014 were included in the analysis, which meant 63 articles. In all, 307 articles were downloaded and searched.

Table 5.4 shows that more often than not, authors note the use of experimental time at the ESRF, ILL, and LCLS as part of the methods sections of their articles (or similar), and/or in acknowledgements, and that in principle, articles can thus be traced back to the facilities. Only a very tiny fraction of the analyzed articles, and indeed none at all among the 63 articles from the LCLS, are published without any trace at all of the experimental work done at the facilities. This can have two explanations. Either these publications report results obtained at the ESRF and ILL, but do not identify the facilities; or they were reported to the publication database by mistake and have in reality nothing to do with the ESRF or ILL, other than that the author perhaps has done other experimental work there that is reported in other publications that these were somehow mixed up with. In either case, even though they are very few (roughly one out of 25 for the ESRF and ILL), these publications break away from the pattern and create difficulties for stringently tracing the output of neutron scattering and synchrotron radiation facilities.

But several additional conclusions can be drawn that are only partially represented in Table 5.4. First, there is a great variety in how authors report the connection with the facilities. Some note specific experimental stations along with other equipment in methods sections, while others note experimental stations and facilities in the acknowledgements. Very few mention facility names in article titles (see Table 5.3). All this is likely due to the great variety of experimental practices in the user communities of neutron scattering, synchrotron radiation, and free electron laser labs, and the variety in the degree to which an experimental run at a facility contributes to the end results as reported in a publication. In some cases an experimental run yields only data that is complementary to other experiments and measurements with other techniques elsewhere. Given the cutting-edge nature of the facilities and their instrumentation, however, many (or most) times the experimental run at a neutron scattering, synchrotron radiation, or free electron laser facility is absolutely decisive for the result. But even if this would be true for all work done with the help of neutron scattering, synchrotron radiation, and free electron laser (which it most likely is not), there is no consistent and clear-cut way in which the users note or acknowledge this in their publications. The full-text search of the 307 articles in the sample shows the opposite: The variety is wide; some articles mention specific experimental stations, some note facility names or only acronyms (see below). None of the forms of giving credit encountered in the articles analyzed are unequivocal in terms of establishing causalities between an experimental run at a facility, a result, and the publication of the result.

There is also a natural bias of facilities in claiming credit for results (which has to do also with the pressure they are under to demonstrate productivity and quality), which might lead to a lowering of the threshold for what counts as contribution by the facility to important results. It is therefore quite likely that the publication lists advertised at facility websites contain a wide range of articles that communicate results that surely are related to work done with the help of the instrumentation these facilities provide, but in many different ways and above all, in ways that are difficult to fully map and establish the exact causalities of. The point is exactly this: Not only is the procedure for scanning the full-text versions of articles in search of connections to facilities tiresome and tedious, it also has methodological limitations in that the articles do not always spell out the importance of the results obtained at a facility for the overall contribution of the publication, especially not in a way accessible to a non-expert. On a very concrete level, and connected to this, it shall be noted that several of the articles analyzed here show that several experiments and experimental resources, and in many cases also the instruments of several neutron scattering, synchrotron radiation, or free electron laser labs may have been used in the work that leads to one article, but there is no information given on their relative importance for the results. It is also plausible to assume that several articles can be and have been produced out of single experimental runs at neutron scattering, synchrotron radiation, or free electron laser labs.

The confusion around acronyms, as discussed above, is also a factor here. In the articles analyzed, there is quite some variety in whether authors use the full names of facilities, or the acronyms, and at what place. Sometimes a facility is mentioned by full name in the acknowledgements and by acronym in the main text, while sometimes it is the other way around, and as noted above it might be hard to achieve complete disambiguation when it comes to acronyms: the "LCLS" and the "ESRF" are fairly unusual and usually lead the reader in the right direction (as do, e.g. "XFEL," "SINQ," and "Elettra"), whereas "ILL" is a troublesome search term, as noted above. The same goes for labs like the ALS, the APS, the future ESS, and CHESS (see Appendix A.2), which in turn have the same acronyms as a neuron disease, a famous association of American physicists (among many other things), a European social science research infrastructure (the European Social Survey), and one of the world's most popular board games.

Productivity, Quality and "Excellence" in the Transformed Big Science

The key message of the previous is that it is difficult, to say the least, to measure the publication output from neutron scattering, synchrotron radiation, and free electron laser labs, because the labs themselves are not scientific production units but instead (many times crucial) resources for knowledge production. This means that if the publication numbers advertised by the labs are divided with their expenditures, in order to answer the question of "how expensive" (transformed) Big Science really is, the analysis will partly miss the target. Nominally, the calculation will show that Big Science is insanely expensive: the cost of each publication from the LCLS in its third year of operation (2012) was $9,529,700 (Hallonsten 2014b: 493), which begs the obvious question of whether the LCLS is really worth its costs.

In fairness, it should be reiterated that the LCLS is perhaps not the most representative example of a transformed Big Science facility and that the calculation of the cost of an average journal article that communicates results obtained at the LCLS is skewed; as noted above it has a basic restriction on its performance by only being able to run one out of six individual experimental stations at a time. Also, the intended role of the LCLS in the research system is not supposedly comparable with any ordinary scientific production unit. It is built to push the frontiers and open up entirely new experimental opportunities—in other words, to be truly unique as an experimental resource and thus is really not comparable with anything, especially not using standardized quantitative measures. But this also means that the LCLS is a case in point for showing the disconnect between on one hand the continuing push towards using quantitative performance assessments in science, evidently reaching also onto the area of large-scale scientific facilities (Hallonsten 2013a), and on the other hand the clear inaptness of simplified publication and citation counts to evaluate the performance and quality of these facilities and labs (Hallonsten 2014b).

The argument can be transferred to the whole category of transformed Big Science labs, because unlike old Big Science labs such as particle physics facilities, which were inseparably tied to the progress of that discipline (simply put, there would have been no discovery of any quarks in the 1970s without these machines, and certainly no discovery of the Higgs boson in 2012 without the LHC or a similar mega-machine), it is much harder to unequivocally claim that neutron scattering, synchrotron radiation, and free electron laser labs are absolutely crucial for the progress in surface physics, or structural biology, or any of the other fields they provide with experimental opportunities. Even if they might in many cases be absolutely crucial for progress in many disciplines, this connection is both very difficult to prove and very difficult to trace. When the 2009 Nobel Prize in chemistry was announced, no less than five different synchrotron radiation facilities in Europe and the United States issued press releases stating that their experimental facilities had been used in the work that led to this Nobel Prize.[5] There is nothing to suggest that this was not true—quite the reverse, it makes perfect sense that this work required repeated experimental work with several different instruments, over a long time—but it shows that it can be nearly impossible to establish a causal relationship between one specific facility, and the investment that went into it, and one Nobel Prize-winning discovery.

The performance assessment of transformed Big Science should, hence, be extended beyond the simplest quantitative metrics. Any single-sided quantitative performance measurement of the scientific output of neutron scattering, synchrotron radiation, and free electron laser facilities will be limited, and the implicit policy advice is that such straightforward counting of publications and citations is not recommendable unless it is part of a broader effort to approach productivity and quality. As a step in the development of such a broader approach, bibliometric analysis that encompasses additional measures such as impact factor, immediacy index, and the construction of citation networks can certainly be of

[5] Three laureates, Thomas Steitz, Ada Yonath, and Venkatraman Ramakrishnan, were jointly awarded the prize for "for studies of the structure and function of the ribosome." Press releases from Brookhaven National Lab and Argonne National Lab named Steitz as user of the National Synchrotron Light Source (NSLS) and Advanced Photon Source (APS); press releases from the ESRF, Argonne, and DESY named Ada Yonath as user of the ESRF, APS, and DORIS; press releases from Argonne, Brookhaven, ESRF, and the Paul Scherrer Institute in Switzerland named Venkatraman Ramakrishnan as user of the APS, NSLS, ESRF, and Swiss Light Source (SLS), respectively. The press releases are available at http://www.lightsources.org/press-releases (last accessed 18 February 2016).

help (Heidler and Hallonsten 2015). The "impact factor" was originally launched by Garfield (1955) to describe the relevance of a publication or set of publications by dividing the total amount of citations going to them with the total amount of publications. The "immediacy index" is an invention by the Institute for Scientific Information (ISI) that originally developed the Web of Science database, and describes the degree to which results communicated in these publications are of groundbreaking character and thus swiftly adopted by the community in the shape of many citations in a short time period. The LCLS has been shown to perform reasonably well in both impact factor and immediacy index compared to leading journals in the fields most prominently served by the lab (e.g. applied physics, biophysics, optics) as well as leading journals in general, such as *Nature* and *Science* (Heidler and Hallonsten 2015: 306–308).

It has also been proven, as yet another measure of impact, that the LCLS indeed fosters research in new disciplinary constellations, at the intersection of biology and physics, with optics and applied physics as bridging disciplines. As an alternative to straightforward bibliometric performance measurement, this analysis delivers little, since it provides no quantitative measure of the worth of an investment in a free electron laser facility but instead a qualitative argument (Heidler and Hallonsten 2015: 309). Nonetheless, it is a good alternative because it shows distinct contributory qualities of these labs. It has been shown with similar analyses that this kind of recombinant research in new hybrid disciplinary constellations centered on new instrumentation is an especially valuable force for the growth and development of science (Heinze et al. 2013). Big Science, old as well as transformed, is still extraordinarily or even absurdly expensive, but if purely quantitative measures of productivity are abolished or at least complemented by qualitative measurements and arguments, based on network analyses or bibliometric analyses of interdisciplinary constellations, their worth could perhaps be more adequately assessed.

The need for qualitative analysis is quite obvious: there are no two neutron scattering, synchrotron radiation, and free electron laser labs that are alike. Within the categories, some convergence of organizational practices as well as technological setups and scientific capabilities are discernible, as noted in Chap. 2 and Appendix A.1, but differences persist that have historical reasons related to global and national competition and the need to establish and cultivate niches that enhance competitiveness in various ways. Also in the case of highly comparable labs such as the ESRF and the APS, comparisons yield surprising differences that have to do with the specifics of the technical design and construction of the labs as well as their political and organizational specificities, including the historical context within which they were conceived, planned, and realized (Hallonsten 2013a: 508ff). Outside of such categories of comparable labs, the differences are even greater. The world's reactor-based neutron sources, for example, may have 50 experimental stations and several partner countries (and thus a large international user community, as in the case of the ILL), or they may be late refurbishments of old reactors for uranium enrichment (see Chap. 2) that run only one or a couple of experimental stations serving a minor, local user community. The synchrotron radiation laboratories of the world similarly vary greatly in size, technical design, and mission, which has a lot to do with their diversified origins. They may be refurbished particle physics machines or purpose-built light sources, and they may also have been designed and constructed with specific scientific purposes in mind, which also shows in their technical characteristics and capabilities (cf. Hallonsten and Heinze 2015). Translated into policy advice, the conclusion is of course that transformed Big Science labs should not be squarely compared with each other, perhaps especially not using straightforward bibliometrics that already in themselves describe and measure performance very ambiguously and with many caveats.

The conclusion extends, in a highly interesting way, to the planning and design of future labs and facilities. Since there are no two labs that are alike, and every lab needs a distinct competitive advantage that can be achieved in many different areas, it is also impossible to predict their level of performance or otherwise their characteristics with any reasonable exactness. Neutron scattering, synchrotron radiation, and free electron laser labs are by necessity innovative and must have groundbreaking ambitions in at least one of the following areas: their technical design, their scientific aims and scope, their organizational arrangements, their user policies, and their interfaces with the surrounding society, including the private sector. What this means for the planning for a new lab is not within the ambitions of this book to thoroughly discuss. At this point it suffices to note that any claims of world leadership on any account that are made in advance of the start of operation of a lab are at best premature and in the worst case erroneous. In fact, as this chapter has shown, such claims are ambiguous also when made several years after the start of operation, on the basis of comparably hard evidence. There is a correlation between, for example, building the world's most high-performing storage ring or neutron spallation source, and attracting users that are capable of producing groundbreaking science. But there is no causal relationship that skips the several steps between the two, such as securing stability of operations, employing the most ingenious and service-minded staff scientists, connecting in proper ways to the forefront of instrument development and ensuring that technology and know-how from this forefront is imported to the lab organization, building user support structures including sample preparation and handling, removing practical obstacles to temporary visits by users from far away including communications and housing, and many more issues. This discussion is instructive not least when proceeding to the analysis in the next chapter.

Interestingly, hence, while there is great difficulty in trying to quantify and prove the productivity and quality of neutron scattering, synchrotron radiation, and free electron laser labs, they are also fundamentally oriented to competing for the best users, and exploiting as far as possible the symbiosis with their user communities, as part of an overall pursuit of such productivity and quality. Sometimes this all results in a skewed image of what these labs can or are supposed to deliver in terms of scientific results (see next chapter), which seems to coexist with a self-assuredness in the organizations of the labs and their long-term and short-term development of scientific programs and activities. When it comes to the self-image and general sense of legitimacy that is so important in all organizations, the unpredictability and complexity of these labs do not seem to create uncertainty at the core of their operations. Neither does, it seems, the difficulty of finding adequate metrics for assessing and proving quality and productivity. The users are probably largely indifferent to the whole analysis and discussion on the basis of the bibliometrics earlier in this chapter: Scientific communities have their own highly informal but very clear pecking orders, also for large user facilities and what these provide and deliver in terms of conditions for excellent scientific work, and the varieties of bibliometric performance assessment explored in this chapter have likely little or no relevance there. For users, reliability and beam availability is important, as is of course the quality of user support and the reliability of other instruments in the lab including sample handling and data processing, all of which are qualities that are very difficult to quantify. Oversubscription rates are, as noted in a previous section, some kind of proxy measurement of all this, but absolutely not perfectly representative.

Overall, many things suggest that neutron scattering, synchrotron radiation, and free electron laser labs and their user communities are content with their capabilities to contribute to the progress of technology and science that they are directly engaged in. But then again, one of the main reasons for the economization and commodification of science, and the performance evaluation frenzy that has followed in its path, is an apparent discrepancy between public demands and expectations of proof that science is delivering what it is expected to deliver, on one hand, and on the other hand the actual content of the professional activities of scientists and how these translate to deliverables.

6

Socio-Economic Expectations and Impacts

Selling Promises

The gradual but rather profound shift in (Western) European and US science policy from the 1960s and on has been discussed at several points in the preceding five chapters, in terms of a gradual abolishment of the Social Contract for Science and the Linear Model of Technological Innovation through "commodification" (Radder 2010), "economization" (Berman 2014), and a "strategic turn" (Irvine and Martin 1984). These concepts are complementary descriptions of the development: Commodification means, in short, that scientific productivity is measured in monetary terms; economization means that scientific research is supported and sustained for the sake of promoting economic growth; and the strategic turn means that direct steering and strategic identification of prioritized areas become (central) parts of science policy. All are based on an increased impatience and renewed demands on science to contribute directly to progress, which in turn makes demands and expectations important governance tools of science policy (van Lente 1993: 10).

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When there is impatience in science policy and discontent on the political level regarding the degree to which public R&D investments are translated into innovation and economic growth (cf. the "European Paradox," Chap. 2), this discontent will become policy and lead to more direct steering, performance evaluation, and reform agendas for institutions and organizations. Here, policymakers have no choice but to focus on the public sector. Although the private sector and its actors, organizations, and institutions are vital parts of the innovation system as a whole, and have tremendous importance for the efficiency of achieving innovation-based economic growth, they obviously lie outside the jurisdiction of the state. Therefore, innovation policy and the reform agenda need to be directed at universities and other public research organizations. But such organizations have institutionalized practices and professional cultures that make them difficult to change, which means that the hopes and expectations of progressive, innovation-oriented policymakers to change them may not lead far. Chances are that expectations and reform agendas instead are directed at new projects whose organizations have not yet been set up, and that have high visibility, such as Big Science facilities newly established or under planning.

In the campaign to mobilize political and public support for the European Spallation Source (ESS) facility in Lund, Sweden, expectations were a key asset: the beliefs and anticipations of what the future ESS will deliver extended beyond scientific and technological breakthroughs, to economic growth and regional competitiveness in a globalized knowledge economy. In a review and analysis of the advertisement material produced by the Swedish campaign to locate the ESS to Lund[1] in 2008–2009, Agrell (2012) highlights four themes of the campaign to

[1] The ESS was a collaborative European project with no predefined site until 2009, when those countries that had pledged to contribute financially to its eventual realization came to an informal agreement to locate it to Lund. This decision was preceded by an intense campaign on behalf of several local and national initiatives; early on (before 2003), Germany and the UK were strong contenders, but as the overall process to fund the ESS was delayed, these two dropped out, leaving Lund in Sweden, Bilbao in Spain, and Debrecen in Hungary as the main competitors. Lund proved most resilient and managed to gain the support of a majority of the other countries, and after five years of detailed negotiation over funding, ground was broken in September 2014 for the ESS facility in Lund (Hallonsten 2015b). Besides this negotiating effort, the local ESS lobby in Lund campaigned heavily on local, regional, and national levels to win support for the ESS among various stakeholder groups.

"sell" the ESS to the general public: (1) the utility of the facility ("science for society"), (2) the uniqueness of the site and its surroundings (Lund as "the perfect host" for the ESS), (3) the all-encompassing range of benefits (the "win-win-win-win situation"), and (4) the metaphorical "endless frontier" of ESS-related research (the "beautiful neutrons"). In the material reviewed, the ESS is presented as "not just a source of abstract scientific data, but of important new knowledge with practical implications in fields like energy, climate, the environment, chemical products for everyday life, materials and health" (Agrell 2012: 433). The advertising material is, furthermore, "underlining the positive impact of new and existing research in the region, on scientific clusters, spin-off companies and the local community and labour market" (Agrell 2012: 434).

The campaign was, it seems, focused on mobilizing expectations among the general public by combining the unknown and the expectable. In the material, there is a remarkable imbalance between, on one hand, the positive framing of uncertainty in connection with the future scientific breakthroughs of the ESS, and on the other, a complete absence of similar uncertainty when it came to the broad social usefulness of the facility. Science is depicted as "incomprehensible and yet predictable," which brings to mind the Linear Model where the actual content of future scientific advances is unknown, but the back end, the products and improvements that will follow from scientific advances, are viewed as certainties (Agrell 2012: 434–437). In line with this, the local newspaper billboard of May 29, 2009, announcing the agreement between a handful of European countries to locate the ESS to Lund, contained a computer-rendered image of the future ESS site, with not only the facility itself but also a number of smaller buildings where, said the image legend, "spin-off companies" will be located (image reprinted in Hallonsten 2012a: 10). While it is of course a contradiction in terms to predict the localization of spin-off companies even before the potential source of spin-offs is in place, the picturing of these future companies in such a visionary image represents the combination of the incomprehensible and the predictable that seems necessary for motivating the investment in the ESS: nobody knows what scientific results the ESS will produce, but it is important that everybody believes that they will be prominent enough for spin-off companies to be formed around them.

This example of the advertising campaign in the case of the ESS is not unique. Examples from other labs show that the promotion of contemporary Big Science projects typically invoke promises and expectations of tangible impacts both in the scientific realm and, by extension, in terms of innovation, economic growth, and global competitiveness. In its 2007 brochure on priorities for new federal Big Science facilities within the US National Labs system, the Department of Energy (DOE) writes about the free electron laser at SLAC National Accelerator Laboratory, the Linac Coherent Light Source (LCLS), scheduled for opening two years later, that the scientific breakthroughs delivered at the facility "will lead to revolutionary new materials with unprecedented combinations of properties—strength and weight, flexibility and resilience, etcetera—for a host of applications throughout the American economy" (DOE 2007: 13). The same report states, about the neutron scattering facility the Spallation Neutron Source (SNS) at Oak Ridge National Lab, which at the time had just been completed and started operation, that it "has important implications not only for science, but also for U.S. economic competitiveness" (DOE 2007: 22), and about the planned but by then not yet decided National Synchrotron Light Source II (NSLS-II) at Brookhaven National Lab that it "would be transformational, opening new regimes of scientific discovery and investigation" (DOE 2007: 26).

The positive impacts of experimental scientific work that lie as much as 10 or 20 years ahead, and that are inherently uncertain, are here presented as almost already existing, or at least predictable with certainty. This is not at all uncommon for popularized technology forecasting; not least for nanotechnology, the phenomenon is well known (see e.g. Coenen 2010; Guston 2014). But it has some consequences for the interface between transformed Big Science and society, and thus also for the politics, governance, and organization of neutron scattering, synchrotron radiation, and free electron laser labs. Harro van Lente (1993: 10) has argued that the strategic turn of science policy in the last decades of the twentieth century has increased the importance of expectations and promises in such policymaking. The reason is logical: specific R&D investments can only be considered strategic, and thus be prioritized, if they have some kind of articulated expectation of utility tied to them, even if this lies several years ahead and is wrapped in uncertainty (cf. Irvine and Martin

1984: 3–5). Today's political climate of harsh prioritization between areas of policy and public expenditure embeds the R&D sector and creates an increasingly rapid turnover of promises and expectations of (preferably measurable) progress. Commodification probably also increases the role of expectations by promoting strong beliefs in oversimplified measures of productivity, excellence, and relevance.

Therefore, promises of future impact are assets in political campaigning. Scientific and technological breakthroughs that lie decades ahead, and thus are shrouded with uncertainty, are routinely presented as already existing and real in order to sell them on a market of public trust and support that is oversaturated with other similar promises but undersaturated with funding and therefore in need of harsh prioritization. Expectations of scientific and technological progress coproduce the social and political breeding ground for scientific and technological projects. Sometimes these expectations even seem to have the ability to singlehandedly chaperone projects through potentially very hostile environments of harsh political and/or economic prioritization (Brown and Michael 2003; Borup et al. 2006).

Obviously, promises and expectations do not suffice to make things happen, but uncertainty can apparently be less of a weakness and more of a resource if used wisely in political campaigning. As van Lente (2000) points out, there is an inherent imperative in promises: once a promise is presented and expectations take root, the only responsible course of action is to act to (try to) fulfill the promises and (try to) meet the expectations. The scientific or technological project at the focus of the promises and expectations becomes the subject of a kind of "self-fulfilling prophecy" (Merton 1948): the promises and expectations should in this context not primarily be seen as prospects that may or may not be fulfilled, but instead as forces for the mobilization of action and support. Some may argue that promises and expectations are worthless because they are inherently and fundamentally uncertain, but van Lente (1993, 2000) and others (Brown and Michael 2003; Borup et al. 2006) have argued that this seeming weakness seems possible to turn into direct advantage in political campaigning, exactly because it enables swift action to reduce uncertainty, which can only be done by pursuing the realization of the project that the expectations concern.

Luhmann (1968) identified expectations as a variety of trust, pointing out that the inherent uncertainty of an expectation necessitates a certain level of trust (in its fulfillment). This means, argues Luhmann (1968: 79), that expectations that go beyond the objectively justified can, paradoxically enough, become even stronger tools in the hands of a (political) campaigner, because trust does not emerge on the basis of what is justified by observation or reason but on the squarely opposite, namely insecurity. Trust in the secure "collapses at the first disappointment," while trust in the insecure is strengthened by the threat of the opposite of the expectation and by the uncertainty that makes (political) action necessary, to fulfill the expectation (Luhmann 1968: 79).

When expectations are institutionalized, a lock-in emerges that both grows increasingly severe and easily spreads: the advertising campaign for a scientific or technological project cannot escape the promises once made on its behalf, and proponents of other similar projects can also not escape these promises. Thus with time, promises and expectations for future benefits are made into a natural ingredient in all promotion campaigns for novel Big Science facility projects that helps to lend them social legitimacy and thus influence actions and organizational behavior (cf. DiMaggio and Powell 1983). This happens in reciprocity with society's need for expectations and visions of the future. In Chap. 1, as part of the discussion about the concept of the Knowledge Society, van Lente's (2000: 44) use of the concept of "ideographs" (McGee 1980) was brought in to explain how some rhetorical concepts can appear as almost completely unassailable and thus be used to motivate far-reaching political action and use of power. McGee (1980: 15) describes ideographs as prompting, almost by default, "collective commitment to a particular but equivocal and ill-defined normative goal," and points out that a particular characteristic of ideographs is that they are unifying as long as their meaning is not spelled out or defined in greater detail. The most obvious examples are "liberty" and "equality," both of which were used extensively in the Cold War era, in political rhetoric and motivation for political (and military) action in both the East and the West, but obviously with very different practical meanings in these two contexts (McGee 1980: 6).

In less extreme political settings than within the Cold War superpowers and their allies and vassal states, ideographs are also used for the long-term legitimation of political action (van Lente 2000: 45). Although the value of “progress” as an ideograph came under doubt beginning in the 1960s through social movements and as part of the postmodern/poststructuralist critique of society, the ideograph “technological progress” seems to have survived and remained almost intact, or at least been rehabilitated. In particular, the strategic turn of science policy and the redirection of priorities away from the nuclear threat/promise of the Cold War era and towards sustainability, the fighting of disease, and meeting grand challenges appear to have infused “technological progress” with renewed status. A seemingly unique property of “technological progress” as an ideograph is that it invokes very palpable past occurrences and sequences of advancement that are unmistakable parts of our society’s history and its transition to modernity. The wheel, the compass, the steam engine, penicillin, nuclear energy, and manned space voyage—to name a few—are past achievements that form part of the cultural heritage of contemporary society and thus are instantly available for those who want to mobilize support for a scientific or technological project. The project is then framed as the “next generation” or “next step” in a chain of human achievements of “technological progress” that the project’s proponents argue should not be interrupted (van Lente 2000: 46–47). Moreover, the past accomplishments can be rather freely selected so that failures, negative side effects, and false starts can be obscured or ignored. The mechanism is similar to the “invention of tradition” whereby contemporary endeavors are placed in a historical context where they do not evidently belong but which helps in mobilizing broad political and public support (Hobsbawm and Ranger 1983).

Of course, “technological progress” was used as an ideograph very much in this manner in the promotion of scientific projects in the Cold War era, especially towards its end, when the further exploration of the subatomic world was growing expensive enough to necessitate public marketing campaigns, and especially in the case of the Superconducting Super Collider. There, proponents invoked all kinds of metaphors of a “frontier outpost” with reference to not only the exploration of the origins of the universe but also exploration in general, even with direct reference to Columbus and the discovery of the New World. In public campaigns, the Superconducting Super Collider was often pictured as a vessel for such exploration (Riordan et al. 2015: 79). The use of ideographs to motivate investments in Big Science today is slightly less grandiloquent and more oriented to practical needs, but the message is similar: these investments are made to further progress and create a better world, and history shows that large projects of this type are suitable for the task, and hence justified.

Therefore, the development that has been described throughout this book as a combination of economization, commodification, and a strategic turn should not be understood as a shift of policy preferences away from basic science and towards applied science. Instead, the strategic turn seems to have allowed basic science to retain its privileged position in society on the condition that it manages to fulfill society's expectations, or at least agrees to be the subject and figurehead of such expectations. Put differently, the level of expectation in the capacity of science to solve all kinds of problems has perhaps not increased, but the expectations have been concretized, and to some degree implemented in practical policies of performance assessment, while remaining firmly attached to the idea of the inherent unpredictability of science.

Historically, as noted in Chap. 2, the expectations that allowed the old Big Science to flourish after World War II was that it would contribute to national security and an edge in the superpower competition, all viewed against the backdrop of the mighty and horrifying demonstration of the power of nuclear energy over Hiroshima and Nagasaki in August of 1945. Nuclear physics, while perhaps only the apex of a pyramid, was what gave scientists a near-*carte blanche* from their governments in the 1940s, 1950s and beginning of the 1960s. The expectations that sustain the transformed Big Science are different in their combination of the extremely abstract and the very tangible: "revolutionary new materials," "U.S. economic competitiveness," "new regimes of scientific discovery," and "powerful applications to biotechnology" (DOE 2007: 13, 22, 26, op. cit.). Old and transformed Big Science, therefore, are both exponents of *Zeitgeist* and reflect the expectations of society generally. The overarching ideograph "technological progress" is intact between old and transformed Big Science, which testifies to the hollowness of the concept: Since it encompasses all kinds of advances that are viewed as helpful in solving whatever problems society currently views as most troublesome, it can only rarely be painted as negative or harmful. In addition, it signals inevitability.

Big Science and the Regional Geography of Innovation

While the expected contributions of Big Science labs are vague in their content, the means by which impact is supposed to be achieved is in some cases very clearly envisioned: new neutron scattering, synchrotron radiation, and free electron laser facilities are supposed to become hotspots in local and regional economies, or melting pots of cutting-edge scientific work, from which knowledge will "spill" into the surrounding economy and society and thus contribute to innovation and by extension, to economic growth. There is some emphasis on the closest vicinity of the labs: expectations are that research institutes, university branch campuses, startup firms and incumbent firms will agglomerate around them. This physical location of a Big Science lab in a region and its contribution to the development of a regional hotspot are what tie it directly to the idea of the globalized Knowledge Economy where competitiveness no longer hinges upon cheap production but on the creation, accumulation and (re)combination of knowledge that will help to solve grand challenges. In addition, the local hotspot connected to a globalized Knowledge Economy is a promise and an expectation in itself, and has been used as a powerful image and selling point in campaigns for Big Science labs, along the lines of what the previous section outlines (see Agrell 2012: 434).

However, as Rekers (2013: 106) accurately points out in her treatise on large scientific facilities and the geography of innovation, these images and ideals need to be balanced by "a more nuanced understanding of the geographical foundations of innovation activities." Globalization involves both concentration and dispersal (Dicken 2007/1986: 21). Spatial proximity is important for "knowledge spillovers" to occur and for the emergence of synergetic effects of direct or indirect connections between activities. Spillovers have indeed been proven to be spatially sticky also in a "slippery space" such as the globalized world (Jaffe et al. 1993; Acs et al. 1994), and especially knowledge spillovers from (academic) research organizations to firms seem to depend on some spatial proximity (Jaffe 1989). In principle, this is evidence that contemporary Big Science facilities, with their high concentration of cutting-edge research activities in a variety of fields with relatively strong relevance for several contemporary sectors of the economy, may form hubs in regional or local knowledge economies. But the pattern needs more explanation and substantiation to be credible: it is not enough to say that "place matters" (Quelch and Jocz 2012). Other things matter as well and may in some cases make spatial proximity secondary or even unimportant. In the beginning of Chap. 5, it was noted that nearly two-thirds of the users at the six neutron scattering, synchrotron radiation, and free electron laser facilities in the US National Labs system in fiscal year 2014 had affiliations with organizations outside of the facilities' respective home states, and at the state-of-the-art free electron laser facility LCLS at SLAC, over half of the users came from universities, institutes, and companies outside the United States. Clearly, access to specific instrumentation, experimental opportunities, and competence can trump geographic proximity.

This is not to say that the several late-twentieth century conceptualizations of the concentration of economic activity, and what it means, are in any way redundant: there are undoubtedly phenomena out there that can rightfully or at least with added explanatory value be described as "clusters" (Porter 1990), "learning regions" (Morgan 1997), and "regional innovation systems" (Cooke et al. 1997). Not only pure practicalities make close distances advantageous, but also cultural factors such as the likelihood that norms and practices are shared in geographically concentrated areas and create kinship (Malmberg and Maskell 2002). A more general argument holds that open flows of information and heterogeneity in skills and competences are conducive to innovation and development (cf. the discussion on team composition in Chap. 3) and central to the "innovation systems" framework (e.g. Lundvall 1992; see Chap. 1). The notion of a geographical limit to the flow of "tacit" knowledge is old (e.g. Marshall 1890), and the relative value of tacit knowledge has been increased in economic theorizing in the past few decades. This spatially sticky asset is nowadays seen as a key factor for competitiveness (Rekers 2013: 110).

But Big Science facilities, just like firms and universities and other organizations, operate according to certain rules, have a wide range of institutionalized practices, and do things that have a certain epistemic content and material or technological structures and boundaries. The literature reviewed in the previous paragraph relies on studies of firms, and there is a fundamental difference between the interaction of firm A and firm B in the same (or similar) line of business and whether this interaction is facilitated by spatial proximity or not, and the interaction of a contemporary Big Science facility with actors, organizations, and institutions in the local and regional economy around it. First of all, and this can be generalized to all cases, knowledge "does not literally 'spill over' the edge of the large scientific research facility and flow into the minds and offices of university researchers and entrepreneurial firms" (Rekers 2013: 112). For knowledge to spread from neutron scattering, synchrotron radiation, and free electron laser facilities, it must penetrate the rather solid physical, legal, and cultural walls that surround them, and it must do so either in the minds of its users and in-house scientists, or in their published results. Therefore, any assessment of the potential of a Big Science facility to become the hub of a local or regional knowledge economy must begin with a thorough assessment of its technological, scientific, and organizational fundamentals: first, what types of research activities the facility supports; second, who conducts research at the facility, what their affiliation is, and where their loyalties lie; and third, what the procedures are for knowledge transfer out from the facility, such as publication habits in different disciplines, and the like. In other words, "it must be clarified who produces knowledge in Big Science [labs] and can transfer it to others"[2] (Meusel 1990: 365). Thereafter, organizational, legal, and political specifics must be mapped, analyzed, and understood. Differences in legal frameworks between universities, institutes, and governmental research laboratories (such as the US National Labs and their counterparts in Germany and elsewhere) regulate technology transfer from Big Science (Meusel 1990: 361), but there are of course several other elements in the organization, governance, and resource economies of these labs that influence their possible role and function in the (regional) economy.

Furthermore, the process of knowledge transfer depends not only on the producer (in this case, the Big Science facility) and the content of the knowledge transferred, but also the recipient environment. The several organizations and institutions locally, regionally, nationally, and globally

[2] Original text in German: "Zunächst ist klarzustellen, wer in der Großforschung Wissen bereitstellen und an andere übertragen kann." Translated by the author.

that supposedly will be the beneficiaries of knowledge transfer from neutron scattering, synchrotron radiation, and free electron laser facilities must be attuned to the character and specific potential of the transferred knowledge. As noted by the European Strategy Forum on Research Infrastructures' Working Group on Neutron Facilities, "it is important to note that there is no automatic route to success," and the outcome of the expected knowledge transfer depends on how "interactions evolve" between the facility and the environment. Importantly, models cannot simply be carried over from one region to another; what works in one place does not necessarily work in another (ESFRI 2003: 14).

To concretize, Rekers (2013: 113) identifies three "distinct ways" in which a contemporary Big Science facility could "contribute to regional dynamics": first, as a "knowledge pipeline" that connects the local and regional economies to global science networks; second, as a "magnet" that attracts skilled labor and talent; and third, as a "partner" that engages in the local economy and science system and contributes to local learning. In practice, all three require absorptive capacity on behalf of actors and organizations in the close vicinity. For them to be able to establish contacts through the "knowledge pipeline" they must be competitive in their own right; likewise, for the skilled labor and talent to become resources for more than the facility itself, there must be some regional attractiveness. Finally, for the facility to become engaged in the local and regional economies, there must be a reason to do so. Use of neutron scattering, synchrotron radiation, and free electron laser facilities is usually open to anyone, free of charge, on the sole basis of scientific merit, and in one way or the other, these labs provide technically, scientifically, or organizationally unique services to their presumptive users (Chaps. 3 and 5). Since these users are often not averse to the idea of traveling halfway around the globe to get access to the exact right experimental equipment for their needs, transformed Big Science labs have no a priori reason to open their pipeline to the local and regional economies, or to become a partner in these. The skilled and talented employees of the facilities likewise have little or no a priori reason to let their skills and talent be of benefit to anyone else than their employer and their collaborators in their work, and themselves in their own careers. Therefore, just like national policymakers and research funders must take responsibility for securing their domestic scientific communities' relative amount of use of a Big Science lab that they have funded, in order to avoid that it only becomes a resource for the rest of the world (see Chap. 2), local and regional government and enterprise must make sure that there is a "regional innovation system" around the lab. This "regional innovation system" and the organizations and institutions it comprises must not only function and be attractive in a general sense, but also be attuned to the scientific activities that the lab sustains, and to its organizational characteristics.

Note the potential goal conflict here: On the one hand, there is a risk that politicians demand that access to experimental time at a Big Science lab is distributed on basis of how much countries contribute to construction and operations costs. So far, there is only evidence for such scientific Fair Return policies in European collaborative facilities like the European Synchrotron Radiation Facility (ESRF) and the neutron scattering lab Institute Laue Langevin (ILL) (see Chap. 2), but the argument is in principle the same for national projects, especially as boundaries become increasingly permeable through internationalization and globalization. Why should the government of country X pay the whole bill for a cutting-edge research facility and then let scientists from country Y come and make experiments at the facility for free, and take back the knowledge to country Y where it is implemented and possibly leads to innovation from which country Y reaps the benefits? On the other hand, it is quite clear that the theory of regional geography of innovation as reviewed above, when turned into policy, stipulates that dynamic effects for the region and country X, which paid the bill, will only happen if there is an inflow of talent and skill from elsewhere. This goal conflict will presumably only grow as the pressure to demonstrate national competitiveness in a globalized knowledge economy grows.

Key to the discussion is of course that really advanced scientific instrumentation, which might even offer globally unique experimental opportunities, will be used to produce very advanced scientific knowledge almost regardless of where it is located. There is hardly any risk at all that a neutron scattering, synchrotron radiation, or free electron laser facility is built with no scientific demand to use it, given the way in which these labs are conceived, designed, developed, and constructed in close collaboration with existing and presumptive user groups (Chap. 3), which secures their relevance and the user demand. Truly boundary-breaking instruments will attract very capable users from everywhere, as demonstrated by the LCLS and its 51% foreign users (see Chap. 5), at least until competitors come online.

Rekers (2013: 118) asks whether it should be considered a failure if all the prerequisites for creating a favorable “regional innovation system” around a brand new Big Science lab are not fulfilled, and such a lab thus remains cut off from the local and regional economy. The answer, of course, depends on perspective. The positive effects of research are obviously not restricted to the local and regional. Quite the opposite: many of those expectations and promises that were discussed in the beginning of this chapter and that tend to be put in print advertisements for planned (and existing) Big Science labs (see e.g. Agrell 2012) are rather of the character of the “endless frontier”, namely completely geographically unbound in their relevance and impact. But for the funder(s) of a facility, if expectations are that most of the investment shall come back to the local and regional economies (see next section), the isolation of the lab from the local and regional economy and the continuous flow of knowledge and technology to other parts of the world would of course be a disaster.

A Holistic View on the Economic Impacts of Big Science

The concept of a “European Paradox” has attracted some scholarly interest and has notably become a topic of debate in European science policy (see Chap. 2). It is built on the practice of measuring R&D strength by total R&D investments as a share of gross domestic product (GDP), and on measuring impact with patent and spin-off data. Both are oversimplified: there is, at least in theory, no obvious correlation between large investments and high productivity, and patents and spin-offs represent only a small share of impact. A “holistic” perspective on impact from R&D is therefore necessary, and the analysis must be allowed to account for contextual factors, acknowledge indirect impact and effects that shows up with long delays, and most importantly impact that comes in long and complex "sequences" (Jacobsson and Perez Vico 2010; Jacobsson et al. 2014; Salter and Martin 2001; David et al. 1994).

The fact that neutron scattering, synchrotron radiation, and free electron laser labs are most of all service facilities for external users (see Chaps. 3 and 5) adds a layer of indirectness to any effect study: the labs do not do science themselves, but enable others to do science, and these others are employed by other organizations, funded by other organizations, and do most of their research work in other organizations. Staff scientists are of course exceptions, but the external users dominate in terms of volume, and they spend various amounts of time in the labs. Any attempt to quantify a neutron scattering, synchrotron radiation, or free electron laser lab's share of the total material and financial resources that go into any random user's research work is futile: if such a quantification would be at all possible to make in a single case, it would not be generalizable, given the diversity of user communities. The discussion about economic impacts of transformed Big Science must therefore be broad and unavoidably vague, but there are categorizations and conceptualizations that can be of guidance.

Not least, the broad and generic character of the transformed Big Science can be contrasted with the old Big Science in a comparison of sheer potential for generating practically utilizable results. In comparison, particle physics is esoteric; although its practitioners often point to the profound long-term social and economic impact of their work, such impact is indirect and has little to do with the topic of investigation, which is the subatomic world. Those technological innovations with practical use that have sprung out of particle physics have rather come from accidental technological developments as part of the design and construction of accelerators, detectors, and auxiliary equipment such as computer hardware and software. CERN advertises its role in the invention of the World Wide Web, and while that was an innovation of truly remarkable socio-economic impact, it was not a practical application of the discovery of subatomic particles and forces but a side effect of the development of an advanced computer-based communication system at CERN. Likewise, many technologies required for discovery in particle physics have by necessity been developed at the cutting edge and thus led to innovations in many areas, but these are very remote from the research questions and answers within the scientific discipline of particle physics. Moreover, accelerator development and other technological invention and refinement are not any less cutting-edge at neutron scattering, synchrotron radiation, and free electron laser labs, and so those indirect impacts are likely to be just as significant in the transformed Big Science as in the old. Add to this that the fields that make use of neutron scattering, synchrotron radiation, and free electron laser facilities are fields immensely closer to practical utilization, and there is enough argument that their usefulness can have economic and social relevance and impact (cf. "extensiveness," Chaps. 1 and 3). This does not guarantee innovation, but it logically improves the preconditions.

Returning to the concrete level, Meusel (1990: 365–366) has put the (economic) impact of Big Science labs in three categories with some overlaps: (1) knowledge production and technology transfer; (2) procurement of goods and services from the economy; and (3) industrial use of the labs. All three contain grey areas of things that are difficult to measure, prove, and demonstrate but that are logically irrefutable and hence highly expectable. As noted, the knowledge production that occurs with the help of neutron scattering, synchrotron radiation, or free electron lasers is in itself tied to institutional and organizational arrangements outside of the labs, and the transfer of knowledge from the scientific use of these labs therefore follows channels and paths that are not necessarily specific to (transformed) Big Science, but instead are specific to the home environments of the users: university departments, institutes, and commercial firms. There are of course technologies that are developed specifically for neutron scattering, synchrotron radiation, and free electron laser facilities but that prove to be of value also in a wider (and not least commercial) context, and these may turn into technical spinoffs, patents, and licenses. These may well be seen as an extension of the phenomenon of "generic technologies" (Shinn and Joerges 2002; see Chaps. 1 and 3), which should of course be acknowledged when applying a holistic view: the easily measureable impacts as seen in firm startups and patents are most likely complemented by several others in later stages of sequences, only indirectly connected to the research where it originates. Technology transfer is an ambiguous concept, not because it is in any way difficult to prove but because there is widespread conceptual confusion around it. Political rhetoric and an overemphasis of the dynamism of academy-industry collaborations and the capacity for innovation in organizations devoted to basic science (Rekers 2013; Geiger and Sá 2008; Jacobsson et al. 2014) seems almost to have given renewed status to the Linear Model of Technological Innovation, or a modified version where universities and research institutes act as monolithic knowledge production entities from which a steady stream of innovation is supposed to flow and fertilize the economy. This is of course connected to economization and commodification, but constitutes an image far from reality.

Procurement of goods and services, on the other hand, is a much simpler affair. For convenience, it can be categorized as "low-tech" and "high-tech," and these have slightly different characteristics. On the low-tech side is found civil construction, conventional services, and the like, which are somewhat more predictable than the procurement on the high-tech side, where instrument components and other specialized equipment dominate. But it is within the high-tech category that sequences of impacts, for example in the shape of dynamic multiplier effects associated with organizational learning and innovation chains, are likely to occur. Low-tech and high-tech procurement have fundamentally in common that all large (public) investments have profound positive effects on the sectors of the economy that relate to the investment. Over time, a majority of the investment in a major research infrastructure stays in the local or regional economy and creates several dynamic effects of direct and indirect nature, such as the boost of in-house skill and knowledge within those companies that win procurement contracts (especially clear on the high-tech side), which in turn can lead to new innovations and further chains of positive effects inside and outside the firm in question. A similar argument can be made about employment effects; large organizations always employ many people, and the positive effects on (local and regional) labor markets are very much like those of direct procurement.

These effects are of course very hard to measure. One indication that a significant share of the investment in a Big Science facility stays in its close vicinity both during the construction and operations phases—or at least that policymakers and managers believe this to be the case—is that policies have been devised in European collaborative projects to correct the unfair advantage this is perceived to create for the host country.

As discussed in Chap. 2, the decision-making process that led to the creation of CERN II in the early 1970s was delayed by the refusal of several CERN member countries to join the extended collaboration unless the new lab was built within their borders. The benefits of hosting were simply deemed great enough to make the relative disadvantage of not hosting a priori unacceptable to several countries, especially the big ones with the largest contributions to the CERN budget (Krige 2003: 905). The situation was resolved by building the new lab at the existing CERN site in Geneva, but the mechanism of Fair Return was also put in place to achieve more balance in relative return for investment by making the collected value of contracts awarded to firms in a member country reflect that country's relative contribution to the CERN budget. This policy was institutionalized in the ESRF organization, but for recent European collaborative projects such as the ESS and the XFEL, Fair Return on procurement has not been implemented; this is due to the competition policy of the EU's common market, which explicitly prohibits such a scheme (Hallonsten 2012c: 309). Therefore, in both cases, extensive use of in-kind contributions to the construction of the facilities have been implemented (see Chap. 2), giving member countries the opportunity and right to substitute cash contributions for delivery of goods and technology, which means that they can spend their money within their own borders and let their domestic universities, institutes, and firms contribute with high-tech (and low-tech) R&D work. There are obvious drawbacks of restricting calls for tenders to specific countries, and risks of delays and huge transaction costs, but the scheme is also an ingenious way of balancing returns for investment in international collaborations (Hallonsten 2012c: 309). The far-reaching use of these schemes—both Fair Return, which is now outlawed in Europe, and its replacement with in-kind contributions—suggests that such balancing is judged to be necessary, which in turn shows that there is at least a widespread belief in tremendous benefits for local and regional economies of hosting Big Science labs, and probably also some reality behind the beliefs.

Industrial use of neutron scattering, synchrotron radiation, and free electron laser labs is a potentially very important source of economic impact of these labs, as the use of cutting-edge instrumentation in principle gives the same competitive advantage to a commercial R&D unit as it does to an academic research group, and in the case of industrial use, with direct or indirect commercial relevance added. While there is probably a growth trend in the industrial use of neutron scattering, synchrotron radiation, and free electron lasers, given the comprehensive efforts at most labs in recent years of opening up to industrial users by launching special access programs and industrial liaison offices, it seems in most cases to remain on relatively low levels. Reliable figures on industrial use are not easy to get a hold on; labs normally do not publish these in annual reports or on their websites. But the user statistics available from the Department of Energy (DOE) for the six major neutron scattering, synchrotron radiation, and free electron laser facilities in the US National Labs system (used in Chap. 5) contain information on the affiliation of users, on the basis of which a rough estimate of the share of industrial use of these labs can be made. Table 6.1 shows these numbers, thus giving a hint (but no more) about the degree of industrial representation in the user communities.

It must be noted here that Table 6.1 counts individuals with stated industrial affiliation, that is, with a private sector company as employer. The figures therefore say nothing about the total use of the facilities for R&D with direct industrial connection, since users employed by firms can also do non-proprietary research (e.g. in collaboration with academic research groups), and many regular experimental runs at these labs may have commercial relevance without involving scientists employed in the private sector, either directly through academy-industry collaborations (where the industrial partner might or might not be represented at the actual experimental run at one of these facilities), or indirectly and with some delay if results prove commercially relevant afterwards (cf. sequences of impact).

The grey area is, hence, significant and Table 6.1 can only give a hint. Those industrial firms that regularly procure experimental time at neutron scattering, synchrotron radiation, and free electron laser labs certainly show in the table, and their share of the total use is evidently minuscule, on average 4.12%. But the largest share of industrial involvement likely occurs through collaborations between industrial and academic users, applying for access in the ordinary procedure and conducting research with combined academic and industrial relevance, and with a heterogeneous set of

Table 6.1 Numbers of users with industrial affiliation at six major neutron scattering, synchrotron radiation, and free electron laser facilities in the US National Labs system, fiscal year 2014 (October 2013–September 2014)

	Total number of users	Users with industrial affiliation	
		#	%
Advanced Light Source (ALS)	2443	163	6.67
Advanced Photon Source (APS)	5016	224	4.47
Linac Coherent Light Source (LCLS)	612	7	1.14
National Synchrotron Light Source (NSLS)	2372	110	4.64
Spallation Neutron Source (SNS)	893	12	1.34
Stanford Synchrotron Radiation Lightsource (SSRL)	1559	101	6.48
Average			4.12

Source: DOE User Statistics (2014)

outcomes in the shape of open publications, patents, and results that are directly exploitable within existing product and service development.

A great many studies have investigated the various links between research infrastructures and innovation-based economic growth and given policy recommendations to regional, national, and supranational authorities on how to maximize return for investment in Big Science (e.g. Valentin et al. 2005; Waldegrave 1993; Horlings et al. 2011; NUFO 2009; SQW Consulting 2008; Technopolis 2011; DRI 1996). With few exceptions, however, these studies are either ex-ante assessments or effect studies of particular labs, commissioned by facility organizations themselves to improve their image and increase their visibility, the lobby groups or political proponents for projects yet to receive funding and go-ahead, or other special interest groups such as scientific societies or lobby organizations with stakes in a particular lab or science system. These studies also typically fall short on the crucially important account of providing solid empirical evidence based on in-depth knowledge and awareness of the specific characteristics of the research activities of the labs and facilities in question.

As Meusel (1990, op. cit.) points out, every study of knowledge and technology transfer from a Big Science facility must begin with establishing a thorough understanding of the organization under study and its technological, legal, and administrative characteristics, as well as a basic understanding of the scientific activities facilitated by the lab, and should preferably also include case studies of actual occurrences of technology transfer, to reveal in as much detail as possible the specific capacities of the lab or type of lab under study. That all organizations that conduct R&D are not the same is a rather trivial remark, but apparently necessary. Big Science labs differ a lot from other research organizations, not least by their very high cost and visibility, and by their function as service facilities for users (Chaps. 3 and 5). These features distinguish them and influence their capability to innovate and supply enterprise and the broader society with knowledge and technologies (cf. Meusel 1990: 361). On the other hand, a longitudinal view on a neutron scattering, synchrotron radiation, or free electron laser labs would probably yield a very positive image of such facilities' wider economic and societal impacts. The concentration of talent and technology (critical mass), the inflow of capital, the continuous R&D efforts to optimize technology and scientific use, and not least the turnover of several 100s or 1000s of external users who are granted access in competition that is sometimes very hard, are in a sense self-evident suggestions of wide and deep impacts in local, regional, national, and international economies and society at large.

7

The Implications of Transformation

Continuity and Change in the Transformation of Big Science

The transformation of Big Science into its current state was a historical process that followed several paths and logics, involving not least the recombination of existing scientific, technological, political, and organizational assets. It took place largely—but not exclusively—within existing institutional frameworks, and depending on the viewpoint, these were thereby both changed and kept intact. Contrasting continuity and change is a powerful analytical approach to studying this historical transformation, and to the analysis of the science, politics, and organization of the ensuing transformed Big Science. A systematic and stepwise analysis of continuity and change from different viewpoints is a similarly powerful method to account for as many variations of the topic as possible.

From the most general point of view, continuity can be summarized thus: Although the original rationale for Big Science is long since gone, Big Science itself remains, both because institutions and infrastructure are durable, and because society retains an apparent need for grandiose scientific and technological projects to be the locus of its expectations

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and hopes. The change, in turn, has many faces but is most evidently seen, from a similarly general viewpoint, in the gradual emergence and growth of techniques and scientific utilization of these techniques, both within the overall institutional and technological frameworks of Big Science and in accordance with society's shifting priorities and the consequential adjustment of the politics and organization of public science systems. Thus, the historical chronicling and sociological analysis in the preceding chapters describes more than a profound change of Big Science within a largely persistent institutional framework. They also describe a change in the relationship between scientists and the instrumentation that they use, including an increasingly important functional and strategic dependence and division of labor between professionals in "small" or ordinary science, and the construction and operation of large complexes of instrumentation as a service to these professionals. This change in relationships can also be described as the diffusion of the use of techniques, only available with large complexes of instrumentation, across a broader disciplinary spectrum of the natural sciences. In general and basic terms, from this viewpoint, the change agents are the basic experimental techniques of neutron scattering, synchrotron radiation, and free electron laser. Continuity is represented by the (largely, or at least comparably) intact institution of science, including the disciplines of the natural sciences and the organizational structures of public science systems, the organizations of Big Science (national laboratories and the like), and science policy systems with their own institutions.

But while such an identification of continuity and change in very general terms is a relevant and accurate conclusion, it does not suffice to give the subject complete justice. This is also evident from the historical chronicle and the sociological analysis in the preceding chapters. Several changes in politics, the economy, and wider society have importance in the historical transformation of Big Science, and a certain restructuring of scientific disciplines and the organization of (academic) scientific work also clearly fed into the development. From other viewpoints there are, in other words, other additional agents of change, and other representatives of continuity. Not least have the infrastructures and institutions of Big Science shown a remarkable capacity for renewal that seems both to have been the ultimate recipe for their continued thriving, and the necessary means of their survival.

As a next logical step, the change agent can be identified as the framework and context of Big Science. The past few decades have seen a profound change to the (perceived) role of knowledge in society as well as to the stakeholder involvement in the processes of knowledge production, summarized in the concept of Knowledge Society (Chaps. 1, 2 and 6). The growth of importance of knowledge in the economy, and in society more generally, has increased the direct steering of the organizations and institutions of knowledge production, and made the public science system into an arena where several more actors and actor groups claim jurisdiction. Several governance reforms, on the level of organizations, at the state level, and internationally, have altered the conditions for knowledge production in public science systems and for science as an institution and profession. On the right level of analysis, Big Science has remained intact when the context has changed: somewhat sarcastically phrased, the long-term development of the US National Labs system (Chaps. 2 and 4) can be summarized as "problems change while solutions remain." It is a valid and useful observation to note that no US National Lab has ever closed, in spite of several mission crises, and there are examples also on the European continent, not least among the German national laboratories. But there are few clues on the macro level as to why and how the system has been kept intact in spite of radical contextual change, and so the topic must be explored from additional viewpoints, not least from the micro level. Endogenous scientific, technological, and organizational developments have contributed to produce the transformed Big Science, in resonance and interaction with the policy and governance changes that were named contextual in the previous section, and produced a renewal that ensured survival.

This renewal can be conceptualized as the decline of the epistemically "intensive" (Weisskopf 1967) and sociologically concentrated fields of nuclear energy research and particle physics, and the simultaneous growth of the epistemically "extensive" and sociologically dispersed materials and life sciences in reciprocity with technological progress in several areas. Synchrotron radiation and neutron scattering are experimental techniques that underwent tremendous development in the second half of the twentieth century (and from the former came also an offspring, free electron laser), in symbiosis with developments in the scientific communities, the organizational fields of public science systems and their institutional frameworks, the science policy systems from which they draw their resources and part of their legitimacy, and the wider society and international systems that embed them. In other words, this development and the parallel or intertwined growth of a transformed Big Science was multifaceted and interpenetrated with a broad collection of other scientific, political, economic, and societal developments. There is, hence, no clear-cut conclusion on continuity and change in content and context, and it is also clear that there has been both continuity and change in all the three dimensions of big machines, big organizations, and big politics, and quite obviously also in the institutional frameworks of all three. The discussion therefore cannot produce unequivocal conclusions, but it can reveal conceptually important and relevant insights.

Continuity and Change of Big Machines, Big Organizations, and Big Politics

The physical palpability and bigness of Big Science breeds continuity: Large pieces of technology are naturally durable, both materially and through the large investments that go into them, financially as well as in terms of political, social, and intellectual capital. This specific force of continuity should also, logically, become more important in the transformed Big Science, given that it has a broader and more flexible disciplinary base and therefore also a less unified constituency than its predecessor old Big Science. But the physical continuity of the big machines also spills over to organizations and politics. A key aspect that (the old) Big Science, inaugurated by World War II, brought to physics and to some extent also to the natural sciences in general, was to tie the organization of its research activities to instrumentation and equipment and breed a significantly more pragmatic attitude among its professionals towards funding, political alliances, and industrial liaisons (Pestre 1997: 141). This change was pivotal for the development of the postwar relationship between politics and science—the "marriage", as Greenberg (1999/1967: 51–52) called it—and secured governmental patronage at an unprecedented scale. Physics became tangible, marketable, and laden with prestige that spilled over to politics and whole nations, very unlike the pre-war, classic ideals of science with origins in the enlightenment, that cherished independence from political interests and an unquestioned loyalty towards the higher cause of scientific truth-seeking (Greenberg 2007: 5–6; Shapin 2008: 47ff). While the old Big Science was perhaps its most physical manifestation, other disciplines were also endowed with a new cultural status as they accepted the new order and adopted a new attitude towards government patronage, alliances with industrial interests, and new duties such as policy advice and investigatory work for the public sector. As part of the same process, practices and organizational modes of physics were gradually spread to other disciplines throughout the second half of the twentieth century (Kevles 1995/1977; Shapin 2008; Keller 1990). Most importantly, the new forms of embeddedness of science in society enabled Big Science not only to rise to prominence but also to become institutionalized in science, in politics, and in society, to the degree that its structures became the vessels of the transformations and gradual adaptation of other fields to new contexts and frameworks, most notably solid-state physics, chemistry, biology, and medical sciences, that were juxtaposed into the new flexible entities such as materials science and the life sciences (Cahn 2001; Keller 1990). This happened much within the framework of Big Science, which of course was also transformed in the process. A few decades after World War II, Big Science had grown so institutionally strong that when its core activities, most clearly particle physics, approached its limits of sustainability and society changed around it, other sciences and technologies could gradually take over and become elevated to the position and status of Big Science by an aggregated process of "consecration" (Bourdieu 1975, 1988). Disciplines that were new and recombinant, first materials science and later also various branches of the life sciences, were thus infused with the "symbolic capital" of Big Science, which certainly contributed to their elevated status in the post-Cold War era, although their practical utility and (expected) capabilities to solve grand challenges and contribute to innovation-based economic growth of course was and is their most important "cultural capital." In the process, Big Science was also transformed but kept in place. The big machines remain big, but change in their use, and renew and broaden their constituencies.

Continuity and change in the big organizations are both similar and directly related, but are also somewhat simpler to characterize. The organizations of Big Science remain big, but the reasons for why they are big, and need to be big, have changed. This is true as far as the scientific use is concerned: Clearly, the difference between the typical use of high-energy electron or proton beams in particle physics experiments around, say, 1964 and the typical use of neutron beams, synchrotron radiation or free electron lasers for the surface physics or structural biology experiments of 2014 are essentially different from an organizational point of view. Therefore, their specific requirements for the big organizations that support them are also essentially different. But there also significant differences within the transformed Big Science, not least between different applications of neutron scattering, synchrotron radiation, and free electron laser in different disciplinary fields, and a built-in flexibility and unpredictability with respect to this diversity, that also set fundamentally different preconditions for the organization of the labs. In the old Big Science, accelerators and reactors were the central instruments, besides detectors, and experimentation in particle physics and nuclear energy research was inseparably tied to the operation of the accelerators and reactors, and thus the big organizations of operating the machines and using them for scientific research were largely overlapping. In the transformed Big Science, they are not: operation of accelerators and reactors still require large organizations, but they do not overlap with experiment organization; in fact, the experimental work itself does not require big organizations. Instead, the division of labor is distinct between the experimental work on one hand, undertaken by quite ordinary science teams from universities, research institutes, and industry that also vary widely between themselves, and on the other hand the support functions provided by the lab. This separation is at the core of the difference between old and transformed, and what it means for the organization of neutron scattering, synchrotron radiation, and free electron laser labs has been the topic of extensive discussions in previous chapters, and will be returned to under a separate headline below.

Politics is a field where continuity and change are obviously coexistent. The basic necessity of Big Science to secure and maintain the support of politics is continuous, but the reason for the support has clearly changed. The transformation of Big Science shows a symbiosis between the growth of neutron scattering, synchrotron radiation, and free electron laser, as well as a change in the political landscape. Chapters 2, 4, and 6 have in various ways analyzed the importance of the changes in politics and society at large for the evolution of science and the transformation of Big Science. Some scholars, not least in the postmodern and poststructuralist varieties of the sociology of science within STS (science and technology studies), would almost certainly dismiss the attempt at analytical separation of science and politics as anachronistic, naïve, and possibly also straight-out erroneous, with the underlying assumption that science and politics are just two forms of exercising power and influence on behalf of human or post-human agents (e.g. Latour 1983, 1987; Star 1995). While such a view might be of assistance in philosophy (of science), it would add little explanatory value here. What will advance understanding is instead a conceptualization of science and politics as distinctly different in their (institutional) logics but symbiotic in a wider societal context. Such a view facilitates contributory analyses of this symbiosis. Thus, we can characterize, e.g. the Sputnik Crisis in 1957 and onward, as a political event (although it, in a sense, had scientific origin) and can trace a causal chain of events back to it, whereby first the federal R&D appropriations in the United States were increased dramatically. This is turn led to the construction of several new accelerator labs, within whose walls technological and scientific developments took place that both paved the way for the transition of particle physics to megascience and enabled the gradual growth of synchrotron radiation as an auxiliary resource of accelerator labs, and so on. The development of free electron laser in the 1980s and 1990s can thus also be characterized as scientific and technological, and the major steps towards implementation of these developments in a real x-ray free electron laser user facility at Stanford in the first decade of the 21st century can be identified as political, pushed by actors in Washington who declared their support for, and willingness to fund, a major free electron laser user facility and not merely the prototype first suggested by the scientific communities and the lab organization at SLAC. Similarly, the Institute Laue Langevin (ILL), generally regarded as the facility project that enabled Europe to take over the lead in experimental science with the use of neutrons in the 1970s and on, can be identified as the offspring of two important axes between France and Germany (Chap. 2): one scientific and one political. The success of ILL is attributable to both, plus of course to the capability of its pan-European user community, but both the French and West German neutron axis symbolized by Heinz Maier-Leibnitz and Louis Néel, and the French and West German political axis symbolized by Konrad Adenauer and Charles De Gaulle, played decisive roles in the process. And though they combined to produce the ILL, thus bridging the scientific and political realms in a most crucial way, they drew on two distinctly different institutional logics and resource economies.

All these examples, and many others in the book, show two important things. First, that case studies, both of facility projects and historical events and processes, should be analyzed for their specificity and only as a next step be used to draw generalized conclusions about overall changes. Second, that an analytical separation of science and politics, and possibly other separations such as between organizations and infrastructures, enable proper attention to varying institutional logics and otherwise distinct characteristics of different realms of society, and are thus highly useful.

But there is another twist on the continuity and change theme, that goes back directly to the Kuhnian "essential tension" as launched already in the first pages of this book, namely, that instruments—and by extension, whole (transformed) Big Science facilities—are drivers of change in science. More than once in the preceding chapters has the point been raised that scientific communities, bound by the institutional inertia of disciplines and old organizational arrangements (e.g. universities), are comparably conservative and conventional in their approach to new experimental opportunities, and that labs and their leaders and scientific staff are the ones that act as visionaries and pave the way for new breakthroughs. This holds as a general reflection about the role of neutron scattering, synchrotron radiation, and free electron laser facilities in the sciences, although there are exceptions (e.g. the story of the free electron laser facility LCLS at SLAC). A reference to the theme of "creating demand" within innovation studies can perhaps be illuminating: as Mokyr (1990: 151ff) persuasively argued, the rather popular notion that "necessity is the mother of invention" is unworkable for understanding why technological change occurs at a fast speed in some societies and not in others. Demand is usually invented as part of the process of putting technological inventions to use—that is, the process of innovation, and thus the aphorism should perhaps instead be "invention is the mother of necessity." Of course, some kind of need must always exist for innovation to occur, but this need does not determine the nature of the solution to the need, and the need may well go unsatisfied for a long time until someone or something responds to it (Schumpeter 1939: 85). This suggests that there are other mechanisms behind innovation than satisfying already articulated needs. The demonstration of a 60-fold increase of resolution of diffraction images with synchrotron radiation at Stanford in the mid-1970s was clearly not made as a response to a demand for such a resolution increase in the scientific communities; it took at least another decade before the use of this enhanced technique became mainstream (Chap. 3). If necessity is not the whole explanation, then, the interesting question is why some groups or individuals seek to satisfy needs of themselves or others in particular ways, and why some do not (Cipolla 1980: 181). It is not an easy task to define what the right conditions are for innovation to occur in science, and possibly an even less easy task to define the individual traits that are conducive to ideas for scientific breakthroughs and the ability to make reality out of ideas. It is clear, however, that the emergence and growth to prominence of the transformed Big Science was a process where both such conditions and such individuals played significant roles, and where also the mediators between individual ingenuity and institutional viability—in other words, institutional entrepreneurs—were very influential.

The Institutions, Actors and Politics of Big Science

The above brings the discussion to the conceptual separation of institutions, actors, and politics and the use of these concepts, side by side, as analytical tools. The institutional perspective was most evidently used in Chap. 4, where concepts from historical institutionalism were put to work in a detailed analysis and discussion on institutional persistence and institutional change. Of course, if the term is used carelessly, anything can be named an institution: science is an institution, the US National

Labs system is usually referred to as an institutionalized system and is hence an institution, and politics have their own institutionalized practices that may or may not be codified but are clearly institutions. In one meaning, therefore, institutional change is a feature of society and history that is elementary and almost trivial to identify, but as any analyst of institutional change would admit, it is not trivial to analyze. The question is what the desired explanatory purpose is of using the terms "institution" and "institutional change" for phenomena and processes.

Explaining how gradual and cumulative change occurs so that each event in a change process appears as incremental while the overall long-term result is so profound that it must be (retrospectively) identified as radical and discontinuous can be achieved with analysis of institutions and institutional change. Note, however, that this is not enough: at some point in the analysis, the conceptual relevance of "institution" will fade and it will then have to be complemented by other concepts such as actors, agents, and decisions, some of which can rely on methodological individualism and some of which instead find their conceptual foundations in political theory. The reviews of lab histories in Chap. 4 show this clearly. The analyses of history, politics, and organizations in Chaps. 2 and 3 demonstrate the strong explanatory power of combinations of institutional perspectives, actor perspectives, and political perspectives.

When remaining on the appropriate level, the analysis of the transformation of Big Science into its current state can therefore benefit strongly from the use of the concepts of institutional persistence and institutional change. For example, synchrotron radiation was a new experience to an old institution when the National Synchrotron Light Source (NSLS) was conceived, developed, promoted, constructed, and taken into operation at the Brookhaven National Lab in the late 1970s and early 1980s. Likewise was neutron scattering a new experience to institutionalized practices at Argonne when the Argonne Advanced Research Reactor (A^2R^2) was suggested as a replacement for the Chicago Pile 5 (CP-5) reactor in the 1960s. But change as such was not new to these labs; the promotion of new projects in order to renew and make progress was institutionalized in the National Labs system upon its inauguration in 1946–47, which means that the National Labs were already at their inception endowed with structures that could or would facilitate change by renewal on the basis of existing elements, existing resource economies, and within existing ecosystems; in other words, something along the lines of what Luhmann (1995) called "autopoiesis." The situation was and is similar in Europe, although the institutional framework, including the resource economies and the ecosystems, are not as easily identified (and/or not as well-documented) as those of the US National Labs system. But also facilities like the European Synchrotron Radiation Facility (ESRF) and the neutron scattering lab ILL, or the synchrotron radiation facility MAX-lab in Lund and the neutron scattering facility ISIS in the UK, are examples of renewal of scientific capabilities within their respective systems and within the overall ecosystem of Big Science and science generally in the respective countries and in Europe at the time.

An important conclusion in this realm is that facilities come into being in very different ways both in the United States and Europe, and that with or without strong institutions and well-defined resource economies, there is great variety in how a facility project can or should be promoted, and also a seeming element of chance.

What it all amounts to is the realization that continuity and change have their counterparts in institutional analysis: there is inertia in institutions but also functions that facilitate change. Thus, while it might be tempting to suggest that institutions worked for or against a particular historical process, this would be an oversimplified view. Deep analyses, such as the one in the second half of Chap. 4, are necessary to understand change, and it is important to retain an inclusive view of the causes of change, the mechanisms of change, the short- and long-term results of change, and how these interact and reinforce each other. Chapter 4 remains on the level of lab organizations but the insight is generalizable to the overall change or transformation process that this book describes and analyzes: institutions that facilitate and inhibit change are everywhere, as are all the other elements that coproduce long-term progress like that essential to science and embodied in the social and intellectual organization of science, Big or not Big.

The connection to the concept of "generic technologies" was noted at the end of Chap. 4 and deserves a note here as well. Institutional renewal and generic technologies are related concepts, though not symmetrical: a generic technology is a device or a solution that finds new uses outside of what it was originally designed for, while institutional renewal is a process whereby new procedures, norms, habits, and rules emerge within the supporting frameworks of the existing. The process by which a generic technology is spread and put to new uses is one type of institutional renewal, and it also typically induces or contributes to institutional renewal in a broader sense, on an organizational level, since new technological solutions tend to be transformative. Therefore, the combination of the two is most forceful as a conceptual description of the emergence and growth of the transformed Big Science: institutions were renewed, and technologies were put to new use, in a reciprocal process of recombination and incremental substitution (or displacement) of both institutional and technological elements. The mechanisms of the change are highly case-specific but follow some patterns; the work of "research technologists" (Shinn and Joerges 2002) is key especially in the context of the transformation of Big Science, and will be returned to below, but other actors and actor groups with influence over technological and organizational arrangements are doubtlessly important as well.

Moreover, all processes have an origin. If the chains of events that constitute gradual and cumulative institutional change are followed far enough back in time or are magnified enough, then there is not much alternative to a conceptualization of individuals as the original agents of change, even if the overall perspective is institutional (cf. Hall 2010). This realization has special validity here given that the topic of analysis is professional scientific activity. In Chap. 1, the conceptual starting point was summarized as follows: there is no such thing as a thinking organization, institution, or system. Methodological individualism, and the rational choice explanations of individual behavior that it invokes to account for the origins of change was also shown to make perfect empirical sense. The several chronicles of Big Science by historians of science that never used any institutional theory but nonetheless clearly describe institutional change are filled with individual actors, as is most of history-writing. Some of these actors can be identified as institutional entrepreneurs (e.g. Arthur Bienenstock, Heinz Maier-Leibnitz) and politicians or bureaucrats with abilities to alter agendas and achieve change (e.g. Konrad Adenauer, Alvin Trivelpiece), but the absolute majority are never identified other than through their categories or their roles, such as the promoters of generic technologies, the scientists in search of better experimental techniques, the administrators of funding applications and proposals in the state bureaucracy, or the voices of public opinion. Yet their importance is just as evident as those with their names in the indexes of history books. All these actors cause change that is enabled by institutions and that changes institutions.

Actors not only have very different roles but also act on basis of very different competences, interests and capacities. This follows logically from the use of the concepts "ecosystem" and "recombination" in Chap. 4, but it is also a conceptual necessity given the theoretical framework for this book (Chap. 1) and the analysis of the organization of transformed Big Science (Chaps. 3 and 5). The role of "research technologists" that promote generic technologies (Shinn and Joerges 2002, op. cit.) as "brokers" between the capacities of instruments and the ambitions of users was decisive in the periods of greatest expansion of both synchrotron radiation and neutron scattering in the 1970s and 1980s (see Chap. 2 and Hallonsten and Heinze 2015). But they were most likely complemented, or they simply had to be complemented, by other powerful actors that similarly acted as institutional entrepreneurs, or that took on roles of enablers or otherwise promoters of certain projects.

It would seem, on basis of the case study of SLAC in the second half of Chap. 4 as well as several other historical chronicles, that individuals and their micro-level deliberations and actions play especially important roles in the early stages of transformative processes (cf. Hall 2010, op. cit.), and that institutions have a stronger importance later on, as events and processes move to an aggregated level. However, the case studies in Chap. 4, and not least perhaps the complementary cases from the European context, suggest that institutional preconditions are also very influential in the early stages of change processes, and that individuals on various levels played enormously important roles later on. In a most rudimentary sense, existing material and organizational structures—institutions—were necessary for neutron scattering, synchrotron radiation, and free electron lasers to emerge and take root in science. For example, the comparably simple relationship between the National Labs and their steward agency the Atomic Energy Commission (AEC) up until 1974 seems to have facilitated the initial emergence and growth of both synchrotron radiation and neutron scattering, as it gave lab directors more discretion to make decisions and pursue new plans, and also free up funds. The increased level of oversight, and the demand for demonstrable relevance of activities under the DOE, most likely helped to get synchrotron radiation and neutron scattering growing in later stages of their development, so that they could gradually take over and change institutions from within and from without. But the change was driven both by internal and external developments: the shift from old to transformed Big Science or the shift from particle and nuclear physics to synchrotron radiation and neutron scattering both took place in an institutional context that was changing, partly because of political movement, partly because of scientific and technical developments, and partly because of initiatives and actions on the level of especially powerful individuals. The key point is that this institutional environment and its change facilitated or agreed with the development.

The French-German axes, in science as well as in politics, have been named as absolutely vital in the processes of establishing the two major European neutron scattering and synchrotron radiation laboratories ILL and ESRF, and historical alliances between European countries on the scientific and political arenas are good examples of "institutions." Nonetheless, the agency of Heinz Maier-Leibnitz, Louis Néel, and other influential scientists that worked to build support for the projects in the European scientific communities, and the politicians Konrad Adenauer and Charles De Gaulle (and later, in the case of ESRF, Helmut Kohl and Francois Mitterand), shall not be neglected. To conclude, both perspectives are needed to account for and understand renewal in complex systems.

Complexity is especially revealed in the functional specialization and division of labor. In concrete terms, the politicians and bureaucrats without whom Big Science projects will never even come into physical being are obviously not knowledgeable in the details of the technologies, science, and organizational challenges of these labs (and this could, by the way, also explain some of the lack of realism in the advertising material reviewed in the beginning of Chap. 6). Scientists are also not necessarily very skilled politicians and are often largely indifferent to issues of science policy and organization as long as these don't affect their work in a very direct way (e.g. Guston 2000: 86ff; Whitley 2014). These are generalizations, just like the simplified description of transformed Big Science laboratories as essentially governed by the two forces of unification represented by the machine and its operations, and disunification represented by the multifaceted experimental program (Chap. 3). The politics of Big Science is as necessary for its existence as are these two cultures internal to the lab, mostly as enabler and sustainer but also sometimes in pushing developments forward. In other words, the political side enables and demands, but it is the side of science that must make the accomplishments.

And politics follows its own logic, distinct from the institution of science. A forceful theoretical tool for understanding politics, and perhaps not least for science policymaking, is the conceptualization of principal-agent relationships that can be identified at several levels in a research system and within research organizations, and between several different actors and actor groups. The strength of the concept of principal-agent relationships is that it captures the fundamental reason for the existence of science policy: societies, represented by their rulers, need to delegate the task of scientific knowledge production to those actors and institutions with the (stated) capabilities to fulfill this task, and need to monitor how efficiently and effectively these actors and institutions fulfill it. But the arrangement between the state (principal) and the scientific establishment (agent) is only the macro-level principal-agent relationship. In all organizations, functional specialization and the division of labor create a constant need for the delegation and monitoring of goal fulfillment. Therefore, particularly in and around highly complex organizations like neutron scattering, synchrotron radiation, or free electron laser labs, myriads of principal-agent relationships exist, that also form chains of command or delegation that run all the way up to the policymakers on national, federal, or supranational levels.

At that highest level, change has occurred that was discussed in the beginning of this chapter as a contextual change that can be conceptualized in many different ways: economization, the rise of the Audit Society, dethroning of the Linear Model of Technological Innovation, renegotiation of the Social Contract for Science, and so on (see Chaps. 1, 2 and 6). All this can also rewardingly be viewed through the lens of principal-agent theory. As discussed especially in Chaps. 2 and 6, the change in science governance in Europe and the United States in the last decades of the 20th century can be interpreted as fed largely by a growing distrust in the institution of science, from the side of policymakers and ultimately the public. In one sense, the science policy doctrine established at the end of World War II, built on the Linear Model and the Social Contract, was an almost unnaturally strong display of trust—in the institution of science, in its efficiency and effectiveness, in the morality of scientists and their aversion to cheating and shirking, and in the scientific method and internal reward system of science— to not breed corruption (and if it did, that such corruption would not be detrimental to fulfilling the task of science, namely to produce results that would win wars, grow the economy, and improve life) (cf. Greenberg 1999/1967, 2001). That this trust would fade was perhaps only a matter of time. The increases of public spending on R&D in the postwar period were bound to produce suspicion among politicians, officials and the public. The criticism from policymakers and the general public towards the Military-Industrial-Scientific Complex and its perceived inability to deliver what society needed was on the theme of both adverse selection and moral hazard: Was this complex truly the right agent to carry out the desired tasks? Was it really held accountable for its spending of public funds?

The result of this unease and the shift it produced, seen from 2016, is a performance assessment and accountability mania that has had both economization and commodification as vehicles in its rise to doctrinal supremacy in science policy. This governance regime comprises transformed Big Science as well. But while the sciences that make use of neutron scattering, synchrotron radiation, and free electron lasers are generally more disciplinarily attuned to the hopes and desires of today's science policymakers, not least by their closer connection to applied R&D and areas of direct practical use and thus to the expected central mechanisms innovation-based economic growth, the labs are still most of all service facilities for academic scientific work, that is, scientific work undertaken with the primary aim of scholarly publication, not practical application and commercialization. Quite clearly, the attempts to put transformed Big Science labs under the scrutiny devised by the Audit Society and Knowledge Society fail to do these labs justice, either as resources for basic science (Chap. 5) or as originators of innovation-based economic growth (Chap. 6). They are simply too expensive, given their output, to be motivated in comparison with ordinary research activities in universities, and it is far too simplified to motivate their construction by referring to their role as motors of "regional innovation systems". Also, if the argument is laid out in full that the labs are indeed service facilities and enable research rather than produce it themselves, there are other infrastructures for science that are both cheaper and more productive, in the simplest sense. Chapter 6 also reveals a fundamental disconnect between the hopes and expectations of policymakers and pundits on transformed Big Science, and the reality of their knowledge production and interaction with the economy and society.

Many of the reasons for these disconnects between expectations/demands and reality, between the metrics of performance assessment and the organizational dynamics of the knowledge production processes being assessed, and between the view on science's role in society and its actual capabilities, stem from the quite natural lack of mutual insight and interest between the actor groups of science, politics, industry, public administration, media, and the general public. Principal-agent theory can not only help to explain why such mutually distrustful relationships breed in complex systems but can also provide tools for analyzing the challenges to governance that they give rise to. Task heterogeneity, functional specialization, and division of labor necessitate delegation and the forming of principal-agent relationships between actors, actor groups, organizations, and institutions. The inherent complexity and unpredictability of scientific knowledge production, in the general case quite eloquently analyzed by Whitley (2000/1984, see also Chap. 3), deepens its disconnect from policy objectives that emphasize swift and concrete results, and that favor comprehensible metrics for assessing goal fulfillment. In the specific case of (transformed) Big Science, politicians and pundits may be fooled by its very physical realness, and the stability and predictability of the almost industry-like operations of its central infrastructures. But neutron scattering, synchrotron radiation, and free electron laser facilities are of course no more predictable or stable when it comes to scientific output than the scientific activities they support.

Europe and the United States

Many of the historical processes, their mechanisms, and their frameworks discussed above differ greatly between Europe and the United States. The relationships between different actor groups, the role of institutions in facilitating and inhibiting change, and the politics that enables and makes demands are expectably different in these two contexts, but there are also similarities.

In Chap. 2, the postwar history of science and technology in Europe and the United States was briefly outlined, and the institutional preconditions for the emergence and growth of Big Science and its eventual transformation were described. The US system of National Laboratories was identified early on as a key institutional construct in this process, both for the growth of the old Big Science (nuclear and particle physics) and the emergence and growth of new initiatives that eventually grew to become main activities and missions of the labs as priorities shifted on national level, and that constitute the transformed Big Science. Western Europe was far from a federation at the time, and although the 28-country European Union is today perhaps closer to a federation than ever, its gradual deepening never concerned collaborations in Big Science. CERN was a response to the growing dominance of the United States in nuclear and particle physics, but also an instrument of US policymaking in postwar Western Europe. It was complemented by several national programs in nuclear and particle physics that continued to thrive scientifically until particle physics evolved into "megascience" in the 1960s and 1970s and a near-sextupling of the CERN budget in a mere decade (see Chap. 2). The strains on the collaboration caused by this increase in the size and cost of CERN took several forms that continue to plague European collaborations in Big Science to this day: site selection procedures for new collaborative facilities reveal quite nastily the self-interest of participating countries and cause delays that can seem inexplicable in light of the ubiquitous rhetorical demonstrations of unity for the common good. Of course, a parallel can be drawn to the site selection process of the Superconducting Super Collider: congressional support of the project was wide before a site had been announced, and diminished rapidly thereafter. But the Superconducting Super Collider is an exception in many ways (see also a later section), and also here. The US National Labs system and the habit of its funder to parcel out new facility projects to keep each lab with a mission (see Chap. 2) spare the United States from the site-selection difficulties that Europe experiences.

On the other hand, when it comes to the institutional contexts of these two systems, several other differences are highly relevant for the analysis. First, the lack of structure in the European case seems to also create strength: Once an agreement is made, it holds. European collaborative Big Science facilities are governed on the basis of treaties or treaty-like agreements between governments that seem almost impossible to break, either because of their formal political/diplomatic weight or because of the risk of loss of credibility and trust. Until the global financial crisis and the European debt crisis in 2008–2009, intergovernmental collaboration on the European continent seemed to have no other possible route than a further deepening (cf. the phrase in the preamble of the Treaty of Rome: "an ever-closer union among the peoples of Europe"), and while not all countries at all times participated fully in this process, it seems that its normative gravity was strong, not least perhaps on the side of keeping high-level agreements. The successes of the ILL and the ESRF are at least partly attributable to the stability ensured by this strong institutional foundation. Logically, the stability is conducive to scientific productivity because it breeds monetary and organizational predictability. The very practical difference between how the political institutions of Europe and the United States work gives a political explanation to the success of ILL and ESRF compared to their US counterparts. An agreement between several European countries to jointly construct and operate a Big Science facility contains fixed amounts of funding for both construction and operations that are in place for several years. The US federal budget is merely annual and thus renegotiated every year, and a shift of political majorities in congress (elections are biannual) can dramatically change both construction and operations budgets for the overall National Labs system and individual projects and facilities. Moreover, government shutdowns (which are rare but do occur) temporarily suspend all non-essential federal funding and thus stop vital cash flows to National Labs and their construction projects.

Second, while the mission crises of several US National Labs in the 1970s and 1980s, and the institutional resilience they showed by accomplishing the necessary renewal, was the consequence of a broad structural transformation, it was enabled, enforced, and enacted by individuals. Whether they worked largely in the shadows and on a grassroots level to promote new initiatives that can grow into world-leading scientific instruments, or were politicians and bureaucrats in charge of departments, agencies, and committees who cut funding, launched new programs, or implemented broad governance reforms, they seem to have been given more leeway in the United States than they would have gotten in Europe due to the difference in institutional arrangements and inner workings of their respective political systems. While the transformed Big Science in Europe seems to have emerged from and grown through the creation and perpetuation of strong and reliable institutions conducive of technological strength and scientific quality, it seems the institutional framework in the United States is looser and allows (institutional) entrepreneurs more latitude, but not necessarily with positive effects. The reorganization of the Atomic Energy Commission (AEC) to the Energy Research and Development Administration (ERDA) and later the Department of Energy (DOE), the installation of Arthur Schlesinger as secretary of energy, and the introduction of Temple Reviews all gave a face to the "squeeze" on the National Labs. While the Trivelpiece Plan secured the survival and renewal of several of the National Labs, the facilities that were funded through the plan and that ushered in the era of transformed Big Science in the United States for real were no particular successes. Neither the Advanced Photon Source (APS) at Argonne National Lab or the Advanced Light Source (ALS) at Lawrence Berkeley National Lab came out unquestionably strong in the broadest and most comprehensive review of the federal synchrotron radiation laboratories in the United States ever undertaken (Birgeneau and Shen 1997), and the Spallation Neutron Source (SNS) at Oak Ridge National Lab seems so far not to have matched either expectations or the price tag of $2 billion, the highest on the civilian side of the National Labs system in the post Super Collider-era (Rush 2015: 150). In Europe, at roughly the same time that the "squeeze" occurred in the USA and the Trivelpiece Plan was launched, scientific communities and national science policy systems had started to recover from the agonizing debates over the funding of CERN II, and rode on the wave of renewed Europeanism in launching the perhaps most successful transformed Big Science facility so far, the European Synchrotron Radiation Facility (ESRF). Thus, while the broader brush-strokes of history of science and science policy in the postwar era appear very similar in Europe and the United States, it is clear that the institutional renewal on the two continents were quite different.

Renewal and the growth of a transformed Big Science was accomplished the US system mostly through an evolutionary progress of recombination within an ecosystem, with some apparent discontinuous events that shocked the system but which the system's institutional resilience also seemed to withstand. The corresponding process in Europe has rather taken the shape of discontinuous launches of strong collaborations that draw strength from a pooling of resources and a culture of continuity. Institutional renewal and persistence are strong but very different in Europe and the United States. While European countries appear to be willing to let a greater purpose of stability and an "ever-closer union" reign, ultimately because of its experiences in the (not so) historical past, the United States appears geared towards swift renewal, either of its origins in scientific ingenuity or in political whim.

But there are also commonalities that enable the two systems to be analyzed side by side without direct comparison. Big Science facilities including neutron scattering, synchrotron radiation, and free electron laser labs are strongly path-dependent and tied to local and national contexts. While pushing the limits of what is possible, technically and scientifically (and to some extent organizationally, see Chap. 4), these facilities need simultaneously to be strongly anchored in institutions of various sorts: scientific communities, technological best practices, organizational and political structures, and so on. The similarities between Europe and the United States are, in this regard, greater than the differences. While in the United States, by default, a major new facility project must be conceived within the ranks of a National Lab and promoted as a priority for that lab, this is not nearly sufficient for its success. It also needs timing and, as Marburger (2014: 231) puts it, "a level of mutual understanding" among the many involved stakeholders and partners, who come from very different cultures and have very different motivations for entering into collaborative work. This is no different in Europe, and although there is less of an institutionalized system of organizations to host major projects (and clearly less political precedent for their realization), it is not certain that the resource economies and ecosystems that European Big Science projects need to tap into and connect to are more complex than those in the United States. Much would also suggest (Chaps. 5 and 6) that both systems are under the same general pressure when it comes to motivating investments, meeting expectations, and demonstrating productivity and quality.

The User Facility Revisited

In the transformed Big Science, the unifying force of operating and maintaining a central piece of infrastructure (reactor, accelerator) is not reflected in a unifying force of experimental use of the labs. Quite the reverse: the fact that neutron scattering, synchrotron radiation, and free electron laser labs are most of all service facilities for large and heterogeneously composed (and growing, in both regards) user communities means that the disunification of these labs on the side of their use is only increasing. Functional specialization and division of labor grows in reciprocity with this disunification, which necessitates new professional functions and roles. The staff scientists with their crucial broker roles (Chap. 3) are the response to a growing need to mediate between the capabilities of the technology and the ambitions of the users. Staff scientists have one foot in the lab organization and one foot in the user community, and bridge the separation between the facility and the user communities, a separation that also shows in the output: the journal publications that report work done with neutrons, synchrotron radiation, or free electron laser, and that might very well not have been possible to do without these experimental resources, may only mention this connection and the use of this crucial resource as part of the running text or in the acknowledgements, if at all. Normally, only those reading the articles carefully will note the connection; ordinary publication databases like Web of Science do not provide any clear-cut method of identifying such lab resources (Chap. 5).

The essentially multidisciplinary character of transformed Big Science and the variety between different experiments are only logical consequences of the role of neutron scattering, synchrotron radiation, and free electron laser facilities as “generic technologies” (Shinn and Joerges 2002; Rosenberg 1992). The contrast to particle physics is instructive: while accelerator labs for particle physics may run several different detectors in parallel and independently, their performance is closely tied to the performance of the whole accelerator system and its original design. In the history of particle physics, theoretical predictions of particles and forces often preceded their experimental detection (or “discovery”), which meant that some accelerator complexes were designed, and their performance modified during and between experimental runs, with very specific discoveries or fields of exploration in mind. Thus, both the design of particle detectors and the design and operation of accelerators were closely and directly tied to scientific ambitions, also involving physicists that had their eyes set on a specific discovery in the conceptualization and design of accelerators. Neutron scattering, synchrotron radiation, and free electron laser facilities are of course also designed and operated with scientific discovery in mind, although not as specifically but in a significantly more general sense. The scientific ambitions that accompany accelerator and reactor design for these labs are broader, generic, and also open-ended. Not only do these reactors and accelerators support several experimental stations that enable very different types of experimental work and operate in independence of each other, but experimental stations can also be substituted as scientific demand varies. Entirely new applications of neutron scattering, synchrotron radiation, and free electron laser are possible to add to existing laboratories over time. When many of the labs that still serve users (see Appendix A.2) initially started operation, many of the major experimental applications they support today (see e.g. Table 3.1) did not exist other than on idea stage or as a theory.

Weisskopf’s (1967: 25) distinction between epistemically “intensive” and “extensive” sciences was complemented in Chap. 3 by a distinction between sociological concentration and dispersion. These concepts capture the difference between old and transformed Big Science as represented by particle physics labs and neutron scattering, synchrotron radiation, and free electron laser facilities, and as described in the previous paragraph: the former is, in comparison, very predictable and stable in its development, whereas the latter three are inherently flexible, generic and most of all oriented towards a broad and changing (expanding) set of applications in natural sciences experimentation.

When this set of applications expands, so do the user communities, and in the process, the strategic and functional dependence between scientists and neutron scattering, synchrotron radiation, and free electron laser facilities grows in importance. Thus, the competitive procedure of allocation of experimental time at transformed Big Science labs also grows in importance, as it adds yet another instance of peer review-based allocation of credit and resources in the social system of science, besides competitive funding allocation, publishing, and the like. Grants are increasingly crucial resources for those interested in conducting independent research, and publishing is by far the most important means of establishing a career and position in science (cf. Laudel 2006; Hallonsten 2014c). In a similar vein, it may well be argued that access to experimental time at a neutron scattering, synchrotron radiation, or free electron laser facility can be vital for the conduct of some experimental studies, the obtaining of some results, and hence also the sustaining and development of individual careers in science and productivity at the group or department level. The growing use of experimental resources provided by transformed Big Science labs across the disciplinary spectrum of the natural sciences, shown not least in the steep growth in the number of users over the past decades, testifies to a growing importance of such experimental time as a critical resource in several natural science disciplines. This not only adds flavor to the continuing debates about the scientific profession, the scientific career, the social organization of science, and its internal and external reward systems. If the importance of experimental time as a resource in scientific work and in the pursuit of scientific careers grows further, it can conceivably change the whole dynamics of some fields. In fact, it is reasonable to suggest that it already has.

All this of course also makes it an arduous task to define what it means that a transformed Big Science lab is productive or "excellent," and to devise any reasonable measures of this. This uncertainty is less of a problem in the scientific communities involved in planning, construction, and operation of these facilities, because they rely on the established schemes and standards for allocating credit and reward in science. Yet it represents a greater challenge for those other actor groups (politicians, bureaucrats, lobbyists, industrialists, etc.) whose involvement grows in importance as the visibility of the lab increases, but who have not necessarily realized that there are such uncertainties. Simultaneously, Big Science labs are still big in the physical sense, and the durability and resilience of big machines provide some continuity, but their physical realness and imposing majesty also sometimes, it seems, create the false impression that they are reliable motors of growth and monolithic entities of knowledge production (Chap. 6).

As noted in Chap. 5, localization should not be overestimated, but also not completely discarded. There is both an internationalization trend in that scientists permeate boundaries freely and look for the most favorable experimental opportunities regardless of where they are to be found, and a localization trend in that scientists and whole scientific environments grow stronger in symbiosis with a nearby Big Science facility. Related to this, at least conceptually, is the discussion in Chap. 6 about the paradox associated with connecting neutron scattering, synchrotron radiation, and free electron laser facilities too much to the long-term competitiveness of a region or country. While there must be an inflow of talent to a region to create the dynamic effects that politicians and other decision-makers and strategists so deeply desire, the same policymakers may just as well act shortsightedly and attempt to restrict foreign use of the facilities they paid for. This will likely not be a problem as long as foreign use remains on reasonable levels, but the position of the threshold of “reasonable” may be moved as political tides shift, and there is certainly some kind of “pain threshold” where governments will react and demand that the several 100 million Euros or Dollars they have spent on a cutting edge lab are put to work to support science done by their domestic scientific communities. At that point, if not before, the question will undoubtedly come up as to whether the government in question, and its agencies and authorities, also have done their part of supplying their scientific communities with the necessary resources for building capacity in the relevant scientific areas so that they can be internationally competitive.

Transformed Big Science as a Normalized State

Putting the analysis into a wider historical perspective, and relating it back again to the broader theme of science and society, it cannot be neglected that one fundamental impetus for the emergence and growth of the transformed Big Science was the decline of particle physics, which opened up physical, monetary, social/organizational, and cultural spaces where the use of neutron scattering, synchrotron radiation, and free electron laser technology could thrive. The decline of particle physics was also, in one way of viewing it (and as discussed briefly in Chap. 2), foreshadowed during the discipline's whole history, because it had a built-in irrevocable growth. At some point, sooner or later, this growth would become unsustainable and lead to a devastating setback for the progress of the discipline, and to the loss of its dominance.

The change of the world order in 1989–1991 not only dramatically changed the dynamics on the world stage but also produced, or perhaps rather coincided with, far-reaching changes within countries and their national political systems, including science policy. In this period, not only did the Cold War formally end by the fall of the Soviet Union and the liberation of its vassal states in Eastern and Central Europe. Also, in the same time period, the last of the large cadres of scientists, policymakers, and administrators known as "the World War II generation" either retired, or worse, passed away (Hilztik 2015: 435). The Superconducting Super Collider (SSC) and its demise symbolizes this shift and its ramifications. The director of the SSC project, Roy Schwitters, is quoted in Riordan et al. (2015: 249) claiming that the experience of the killed SSC shows that "America is losing its will to do the risky, important endeavors" and "essentially becoming a society that wants to reduce risks at any cost." Though of course a bitter and resentful remark made right after the congressional vote to kill the Super Collider, there is probably some truth to this claim: it resonates well with the conclusions of the analysis in previous chapters as well as above on the doctrinal shift in science policy and what it has meant for science generally.

But it must also be remembered that the SSC most of all was overwhelmingly big, and that although its supporters clearly regarded it as a wise and worthy investment, it is highly debatable whether it truly was, perhaps especially if viewed against the backdrop of its time and what the project was made to symbolize. Then, the Superconducting Super Collider—or rather, its demise—can be seen as drawing a line in the sand, a boundary of sorts for how far old Big Science could be allowed to expand. As the history of the SSC clearly shows, and as Riordan et al. (2015) note in the introductory remarks to their volume on the SSC history, the project and the sequence of events that led to its cancellation raise fundamental and almost philosophical questions about how large a scientific project can really be and where the previously seemingly inexhaustible source of public support for science (and Big Science) actually becomes exhausted. It also raises sociological questions about the nature of the alliances between fundamental science and other stakeholders in politics, the economy, and society at large, alliances that necessarily become deepened and strained by undertakings like the SSC. Empirically speaking, it seems that one boundary, in the 1990s, lay somewhere between the SSC and the Large Hadron Collider (LHC) at CERN, and this was not only a matter of size (the SSC circumference was to be more than thrice that of the LHC) but also of politics, governance, management, and organization. Writes Riordan et al. (2015), the SSC project was conceived, planned, and executed as the rescue mission for US particle physics, which had slipped somewhat and gotten strong competition from Europe; thus, it was by design a prestige project. The LHC, almost squarely opposite in this regard, was an international collaboration that sought and organized the support and collaboration from almost every particle physics lab in the world, including the USA, and while it obviously had its own speckle of prestige, it was also a genuinely international effort that built on the long legacy of CERN as a collaborative peace project in Geneva. In this sense, also, the Superconducting Super Collider was a kind of ultimate move in the direction of "intensification" (Weisskopf 1967): Cut off from any reasonable societal relevance criteria and also from the idea of international collaboration in the field, it was reshaped into a pure prestige project that cohered with the Reagan administration's science policy doctrine of national pride and overarching policy objective of a peaceful US victory in the Cold War, almost at any cost. It can be argued that the LHC at CERN was just as much a move towards increased "intensification" in particle physics, but the difference is that it was allowed to become reality, which must be a sign that it was kept within some kind of reasonable political and institutional boundaries that the Super Collider broke.

It might be argued that the Superconducting Super Collider does not deserve as prominent a place in the historical account and analysis of the transformation of Big Science. But bearing in mind and drawing inspiration from the account by Riordan et al. (2015: 249ff) of the reactions among leading US physicists, lab directors and science policymakers to the demise of the Super Collider in 1993, it seems that the role of this failed project in transforming Big Science into its current state, and indeed transforming the science-society relationship as symbolized by Big Science, was quite important. After all, the quote from SSC director Roy Schwitters reproduced above is preceded in Riordan et al. (2015: 249) by a quote from the same person, concluding that from then on, "all science will have to be justified in stronger terms." As a summarizing prophecy of the road ahead, not only for Big Science in the United States but indeed Big Science and publicly funded science generally, on a global level, this quote is close to absolute.

Allowing a slight normative bent at the end of this discussion, it can perhaps be argued that the old Big Science was, as Price (1986/1963: 29) writes, "an uncomfortable brief interlude." It was the generation of Ernest Lawrence, Robert Oppenheimer, and Edward Teller that created the old Big Science, not only by their technical innovations and scientific achievements, but also because they occupied the chairs on the committees in charge of funding new accelerator projects, where funding decision-makers "based their decisions not only on the objective merits of the proposals but on their subjective judgments of the applicants' reputations and standing in their fields" (Hiltzik 2015: 435). Perhaps the old Big Science was most of all an adventure in wasteful spending on esoteric sciences and extremely large machines that employed 1000s of people and most certainly created a lot of knowledge and competence but that also elevated powerful men to positions where their hubris was allowed to flourish. The Super Collider might suggest so, but way before that, the Atomic Energy Commission had been dissolved in 1974, partly because of alleged mismanagement and lack of transparency (Chap. 2). While its replacement by ERDA and later the DOE has been interpreted as an intervention in lab affairs that created bureaucracy and made governance more difficult, it was perhaps also an understandable reaction on behalf of policymakers that saw particle physics continuing to thrive while the rest of the science system and large parts of society generally suffered from austerity. In the same vein, of course, economization, commodification, and the strategic turn in science policy may be viewed as reasonable reactions of the political establishment to a science that is seen as having run out of hand in its hubris-like esoteric search for the origins of the universe (cf. also the discussion on principal-agent relationships in a previous section). Contemporary science, oriented towards better materials, health, and sustainability, may be corrupted by a market-driven logic of relevance and measurable productivity, but it is more oriented to solving problems that are perceived as real and relevant, at least from the looks of them, and those looks are important in the prioritization games of national and international science policy.

The transformation of Big Science in a way epitomizes this shift by transforming the basic technologies of the old Big Science and adapting them for a broader range of applications less intensive and more extensive, less esoteric and more relevant. A discipline seeking "intensiveness" can probably continue developing in such a direction, farther and farther distanced from the rest of science and from society, as long as it does not lay claim to too-large sums of money that could be spent elsewhere. What counted as too-large in the 1990s is quite clearly demonstrated by the experience of the Super Collider. The transformed Big Science, essentially "extensive," can expand unobstructed across disciplinary boundaries and as it does, it can also expand its claim on monetary resources, because it has the key quality of serving the sciences instead of alienating itself from most of them—it is thus a Big Science that has returned to a kind of normalized state.

Yet also in a less normative meaning, the transformed Big Science is Big Science in a normalized state because the inherent task uncertainty of science makes it necessary to let its governance lie with individual scientists (Whitley 2000/1984: 14). In other words, the normal state is a science governed on the micro-level, and the old Big Science was a very notable exception from this.

The Concept of Big Science Revisited

Big money was never was part of the conceptualization in Big Science (Chap. 1) that set the framework for the analysis in this book. The reason lies in the unfair comparison between costs and output of different neutron scattering, synchrotron radiation, and free electron laser facilities, such as the comparison between the LCLS and the ESRF (Chap. 5), which yielded the result that science conducted at the free electron laser LCLS at SLAC is more than 25 times more expensive than science conducted at the European Synchrotron Radiation Facility (ESRF). Leaving aside the highly valid objections to measuring scientific output by counting journal publications, and focusing instead on the issue of costs, this result is essentially flawed: scientific productivity cannot and should not be measured this way. The operations costs of the LCLS, or the ESRF, or any other neutron scattering, synchrotron radiation, or free electron laser facility, are not linearly related to their scientific output because most of the scientists that do the experiments have their employment and most of the assets they require in their work elsewhere, normally in a university or research institute setting. Thus, the bigness, in monetary terms, lies elsewhere.

Furthermore, it may well be argued that transformed Big Science is not that big, neither in monetary terms nor physically or organizationally. While the machines are sometimes majestic, and as pieces of scientific instrumentation they are larger than most, the essential quality of transformed Big Science labs as facilitators of a myriad of small science projects makes them not very different from universities or institutes that are not Big Science (at least not in the definition used in this book) but that are undeniably big organizations that facilitate a wide breadth of activities in small or ordinary science, and that certainly have significant maintenance and operations costs. In Chap. 1, it was noted that the big organizations of transformed Big Science are really not that big if the number of employees are counted and compared to those of most universities and multinational companies.

All this, once again, testifies to the analytical unmanageability of the Big Science concept, and how discussions about the content, role and meaning of Big Science in science and society, if oversimplified and reduced to easily communicable points, can run into paradoxes and dead ends. In the study of complex phenomena, detail and nuance are virtues and not vices, and analyses with a dialectic flavor can be inventive and inspiring, not merely contradictory or inconclusive. Nearly all the available evidence, including that reviewed and cited in this book, indicates that the social and intellectual organization of contemporary science is highly complex and multifarious, that (transformed) Big Science is no exception from this but instead a phenomenon with intriguing qualities, and that furthermore, it is a growingly important phenomenon in contemporary science that is severely under-analyzed. Simultaneously, there is a steady output of reductionist and simplified (and thus misrepresentative) conceptualizations and conclusions regarding the current state of science and its organization, politics, and interfaces with society. Scholars studying science policy and organization carelessly use catchphrases like "mode 2" and "postnormal science"; academics and pundits with activist agendas make sweeping accusations of "neoliberalism" or "managerialism" in governance practices and call for "postmodern" epistemologies (and ontologies); policymakers and bureaucrats claim to be building "innovation systems" in order to contribute to the growth of a "dynamic globalized knowledge economy" while also relying on overgeneralized international ranking lists and publication/citation counts for their appraisal of what they call "excellence."

Meanwhile, the opportunities and incentives for interdisciplinary exchanges and collaborations to try new and boundary-crossing approaches to research problems and let go of oversimplified categories and definitions in favor of deepening and widening the knowledge about current phenomena are perhaps greater than ever, not least thanks to information technology and increased (international) mobility. But quite ironically, strong disciplinary compartmentalization and a widespread stubborn inability/disinterest in scholarly communities to make connections still exist between different empirical focus areas, different theoretical perspectives, and different methodological approaches.

Without doubt, a great many highly useful and illuminating studies have made truly contributory additions to the study of science policy and organization, the role of science in society, and more specifically, the phenomenon of (transformed) Big Science as understood here. Also, concepts like "mode 2," "managerialism," "innovation systems," and even "excellence" have their merits and are potentially useful if their limitations and capabilities are acknowledged, which by extension means that they have to be used in contrast both with each other and with additional concepts that can lend explanatory value to problems at hand, as part of a thoughtful analysis. This applies also to theory and method (as argued in Chap. 1), and to the choice of empirical material (as noticeable throughout the whole book). An analysis of a complex topic should be made with great care and an open and eclectic approach, in order to account for as many aspects of the topic as possible.

In his popular science book *The Evolution of Everything,* journalist Matt Ridley makes the case for an evolutionary approach to the chronicling and analysis of history, stating that there are "two ways to tell the story of the twentieth century"; either by describing "a series of wars, revolutions, crises, epidemics, financial calamities" or by pointing to the "gentle but inexorable rise in the quality of life of almost everybody on the planet" by "the swelling of income, the conquest of disease, the disappearance of parasites, the retreat of want, the increasing persistence of peace, the lengthening of life, the advances in technology" (Ridley 2015: 317). Not only the devil is in the detail; this idiom is generally believed to be derived from the expression "God is in the detail", which figuratively would imply that details are important for understanding. The history of science deserves to be viewed in such a way, to allow for its natural complexity and intriguing detail to shine through. A quest to give complexity and detail justice is furthermore helped by the careful use of conceptual frameworks and analytical tools from various theoretical strands that can be combined to achieve a higher explanatory power of the analyses and discussions that follow historical chronicling. Thus can the subject be properly represented, analyzed, and argued, and the reader be both informed and inspired. True, macro-level processes and events that give an oversimplified view of the subject are also useful not least in connecting to the general consciousness of the readership and making the history relevant in a broader sense, for the social sciences and a wider public. While it is possible to write the history of science and Big Science in the second half of the twentieth century by describing wars, disasters, economic crises, and failures of megaprojects—all of which undoubtedly have huge impacts on the history to be written and all of which deserve their places in historical accounts—it is also not only possible but indeed advisable to chronicle, analyze, and explain the history of science through complex micro-processes that seem evolutionary, or "gentle but inexorable." This is not to say that micro-level historical events and processes are ruled by social or technological determinism, but that their part in shaping major historical events and processes is significant. In no small part, the transformation of Big Science has played such a role in science, although it would appear (so far) as if the events and processes of this transformation, as chronicled and analyzed (in part) in this book, happened off the world stage.

Appendix 1

The Science and Technology of Neutron Scattering, Synchrotron Radiation, and Free Electron Lasers

All neutron scattering, synchrotron radiation, and free electron laser facilities are unique assemblages of technology and are operated by organizations with similarly unique varieties. But some fundamental characteristics are shared, in technology, in organization, and in scientific use.

What fundamentally unites all neutron scattering, synchrotron radiation, and free electron laser labs is that they all provide a crucial experimental resource—neutrons or x-rays and other radiation—to external research groups that visit the labs temporarily to do experimental work as part of their ordinary research projects. With very few exceptions, the users have applied in advance for experimental time, and their work in the labs takes place at the different experimental stations that are designed and built for specialized applications and that users also sometimes modify technically as part of their experimental work.

Particle physics, on its part, has a radically different technological and sociological structure. For contrast, Fig. A.1 shows schematically (and over-simplified, for reasons of clarity) the layout of a machine for

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particle collisions, normally not called an accelerator but a “collider.” Two beams of particles (normally electrons, positrons, or protons) are accelerated in opposite directions (arrows), either in separate tubes or in the same tube, and led to collide head-on in the interaction areas (white boxes) where detectors register the results of the collisions, which are traces of smaller constituent particles that can thus be identified. Two interaction areas are depicted for reasons of simplicity, but there can be many; at the Large Hadron Collider (LHC) at CERN, where the Higgs Boson was discovered in 2012, three detectors are in operation simultaneously, at three different spots around the very large ring. The operation of accelerators for this purpose is dedicated to keeping as high a current as possible (meaning the number of particles in the beams), keeping the particle beams focused (i.e. keeping the cross-section area of the beam as small as possible while maintaining the current), and thus increasing the “luminosity” or the “interaction level,” which is the probability of detecting interesting events in the collisions (Sessler and Wilson 2007: 80). These round-shaped accelerators (colliders) dominate in the history of particle physics, but also linear accelerators have been used; for example, the original machine at the SLAC National Accelerator Laboratory was a 3-km linac (that also gave the lab its original name, the Stanford Linear Accelerator Center) (Galison et al. 1992).

Missing in Fig. A.1 (as well as in Figs. A.2, A.3, and A.5, below) are the “pre-accelerators” or “injectors” where the electrons/protons are first accelerated to a desired speed/energy before being injected into the main accelerator ring. Also not showing in these figures or in Figs. A.3 and A.5 below (which also depict accelerator-based facilities of various sorts), are the magnet lattices of the accelerators and the “radiofrequency cavities” (circular rings) and “klystrons” (linacs) that charge the particles with energy, or in other words, accelerate them (Sessler and Wilson 2007: 36). Here, accelerators built for particle physics, and for producing neutrons, synchrotron radiation, or free electron laser are similar: their basic function is to accelerate elementary particles to high speed, although the purposes differ, and thus their basic technical setups are alike. Similarly, they have in common that particles tend to interact with everything around them and thus disappear, which means that the tube in which the particle bunches travel

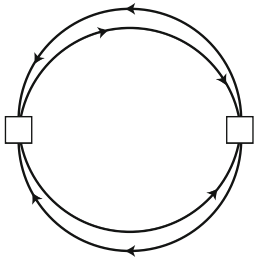

Fig. A.1 Schematic layout of a particle physics facility

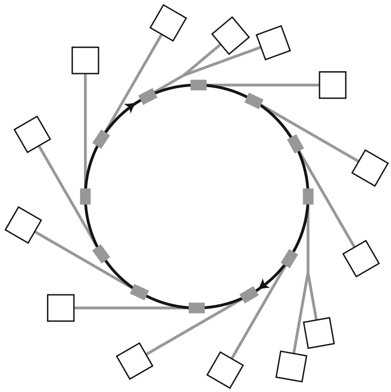

Fig. A.2 Schematic layout of a storage ring-based synchrotron radiation facility

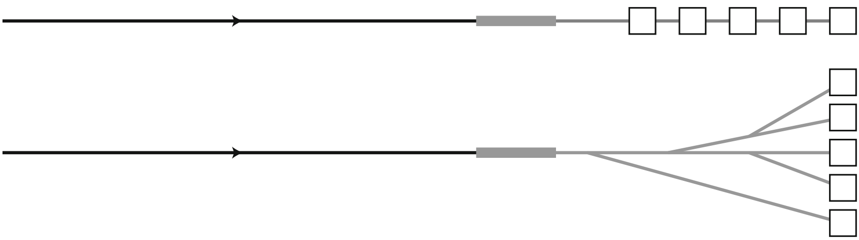

Fig. A.3 Schematic layouts of two types of free electron laser facilities must hold an ultrahigh vacuum—that is, very low air pressure—which is a challenge in itself to create and maintain (Margaritondo 2002: 59–60). A large number of magnets of different types also serve to keep particle bunches focused, positioned, and in cases when several bunches are stored or accelerated simultaneously, separated (Marks 1995: 325). But however sophisticated these magnets and the other components of the accelerator complex are, the current (the amount of electrons in a bunch) decreases naturally and must be refilled to maintain a high luminosity (particle physics, Fig. A.1) or a high intensity of radiation (synchrotron radiation and free electron laser, Figs. A.2 and A.3), or a high flux (accelerator-based neutron scattering, Fig. A.5).

Figure A.2 shows, schematically, the layout of a typical synchrotron radiation laboratory. The black circle is the accelerator (or, more accurately, the storage ring) where electrons are accelerated (arrows) and pass through bending magnets in corners or insertion devices (see below) in straight sections (grey small boxes), where they turn and produce the radiation that emerges through the tangential beamlines (grey lines) to the experimental stations (white boxes). The latter are all simultaneously served with radiation and thus run independently and in parallel. Some beamlines are shared between two experimental stations, and whether they can operate simultaneously, or whether the radiation is directed to only one of them at a time, depends on their technical setup and the type of experiments they support. The number of beamlines and experimental stations possible to accommodate depends fundamentally on the size of the ring as well as the design of the building that hosts it, but beamlines and experimental stations are also very expensive and only those with a clear demand in scientific communities are constructed and maintained, which means that the maximum physical capacity of a storage ring must not always be utilized. Technical operation of the accelerator is, similar to the case of particle physics, dedicated to maintaining the current and the focus of the beam, but in this case the purpose is to increase the brightness of the radiation that emerges out through the beamlines. Although the basic function and components are similar, on the level of specific technological solutions in accelerator design, construction, and operation, a storage ring for the production of synchrotron radiation is different from a storage ring for particle physics experiments.

Round-shaped accelerators are very seldom circular but rather polygonal, and the very first exploitation of synchrotron radiation used only radiation from those magnets that are placed in the corners of the polygon, where it naturally emerged. This is because of a fundamental law of nature: any elementary particle of high energy and velocity will lose energy if its trajectory is bent. As predicted by James Clerk Maxwell in the 1860s, the energy loss of the particle is emitted in the tangential direction of the bend, in the shape of electromagnetic radiation (Blewett 1998: 135).

The bending magnets of early synchrotrons and storage rings were only there to keep the particles in a predefined trajectory and produced synchrotron radiation by accident in a broad planar angle, which is not very inefficient since only a rather small fraction of the radiation angle could be utilized (Margaritondo 2002: 30). Thus, when "insertion device" technology matured in the late 1970s it enabled a significant jump in performance. Insertion devices are arrays of magnets located in the straight sections of the polygonal storage rings. There, they make the electron bunches turn several times and emit not only a much more intense beam of radiation, but also a beam with intensity peaks in specific wavelength regions (Margaritondo 2002: 10–18). Two main types of insertion devices exist: "wigglers," which consist of electromagnets and essentially behave like a series of bending magnets, thus mainly multiplying the intensity of the radiation, and "undulators," which are arrays of solid magnets that make the electron bunches oscillate in specific patterns and thus also enables them to cohere and produce sharp peaks of intensity in certain design-specific wavelength regions (Crease 2008a: 447). Insertion devices have enabled significant leaps in the performance of synchrotron radiation sources, and in a sense, the maturing of insertion device technology was what enabled the maturing of synchrotron radiation as a "mainstream" experimental technique in the natural sciences (Hallonsten and Heinze 2015: 844).

Several technical devices are also placed between the storage ring and the experimental stations, to focus and polarize the beam and separate out wavelengths of radiation. Different experiments require different wavelengths, and a typical storage ring for synchrotron radiation production emits radiation across the full spectrum from infrared and visible light to ultraviolet and x-rays—in other words, wavelengths from 100s of micrometers down to below one angstrom (one tenth of a nanometer). Insertion devices are designed to achieve peaks in specific wavelength regions, but "monochromators" are also needed to purify the radiation, with respect to wavelength, on its way to experimental stations. Mirrors, lenses, and gratings are also used to focus the radiation beam and to achieve polarization and increase the coherence of the radiation, which some experiments request.

Figure A.3 shows schematic sketches of two varieties of free electron laser labs. The long black lines are the linear accelerators (linacs) where electrons are accelerated and pass through very long undulators (grey boxes). There, they produce the laser that emerges through beamlines (grey lines) to the experimental stations (white boxes) that are placed after each other (so that the laser passes through stations that are not momentarily in use) or beside each other (which means that the laser beam needs to be directed with mirrors). In both cases, only one experimental station is served with the laser at each moment, and thus only one experiment can run at a time. Free electron laser is a refinement of synchrotron radiation that enables new types of experiments, for two main reasons. First, the x-rays are completely coherent—this is what defines laser radiation, in contrast to other radiation—which results in a significantly higher level of detail in almost all measurements. The time structure of the beam can also be managed, which means that x-rays in very short pulses can be obtained, to enable time-resolved studies (Feldhaus et al. 2005: 800). While in some parts, free electron laser and synchrotron radiation facilities are similar—they both use accelerators, undulators, and have several other things in common, like vacuum technologies and the basic layout of experimental stations—the technologies behind the production of the radiation differ slightly. Free electron lasers rely on the SASE effect ("self-amplified spontaneous emission"), whereby the radiation produced by the electrons in the very long undulator interacts with the same electrons to amplify the beam and increase its brightness and coherence (Emma et al. 2010)

Figure A.4 shows the layout of a reactor-based neutron scattering lab. The black circle in the middle is the reactor, where neutrons are produced by nuclear fission and spread though the beamlines (or neutron guides)

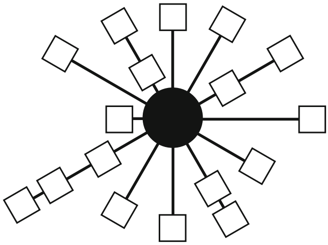

Fig. A.4 Schematic layout of a reactor-based neutron scattering lab

to the experimental stations (white boxes) placed around the reactor. All experimental stations receive neutrons at all times and can thus be run in parallel with no compromise to each station's level of performance. The experimental stations that share neutron guides use neutrons of different "wavelengths" (speed), and since the reactor produces neutrons of a large range of wavelengths, the experimental stations that share neutron guides are therefore complementary in their use of neutrons and do not compromise the performance of each other.

In Fig. A.5, the typical layout of an accelerator-based neutron scattering lab (spallation source) is shown. The long line is a linac where protons are accelerated and smashed into the target area (the smaller black circle), where neutrons are produced by spallation and spread through the neutron guides to the experimental stations (white boxes). Just like for the reactor-based type in Fig. A.4, all experimental stations receive neutrons at all times and can thus be run in parallel without compromising each station's level of performance. The major difference between a reactor-based neutron source and a spallation neutron source is that the latter produces pulsed beams of neutrons. In a reactor, the neutrons are produced by nuclear fission, which is a continuous process, but in a spallation source the protons are fired on to the target in pulses, meaning that the neutrons are also released in pulses. Thus, to a certain extent, the length and frequency of pulses can be manipulated.

"Spallation" originally means "fragmentation," and this is also essentially what a spallation source does: protons are accelerated to high energy and smashed into a target material with very high neutron density (mercury, tungsten, or a bismuth/lead alloy), where they knock out neutrons in large quantities that are led through guides to the experimental stations

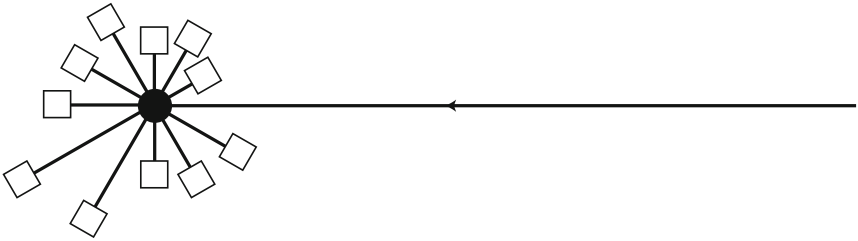

Fig. A.5 Schematic layout of an accelerator-based neutron scattering lab (spallation source)

(Berggren and Matic 2012: 31). A major technical difference between on one hand synchrotron radiation and free electron laser labs, and on the other hand neutron scattering labs (both reactor- and accelerator-based) is that synchrotron radiation and free electron laser can be focused and directed with the use of mirrors, gratings, and lenses, and the radiation beam produced can also be manipulated by influencing the behavior of the electrons in the storage ring or linac in different ways. This means that there are several technical parameters that can be tweaked in order to improve the quality of the radiation in various ways, from injection of electrons into the accelerator, all the way to the experimental stations. Neutrons, on their part, are very difficult to focus and steer since they are neutral, and thus their motion or direction cannot be manipulated by magnets, lenses, or anything like them. Therefore, neutron scattering labs (both reactor-based and accelerator-based) have to rely very much on the amount of neutrons they are capable of producing—in other words, the brute force or effect of the machine. This need to increase the effect is what once drove the development of the spallation source concept (besides the prospects of achieving pulses of neutrons) and remains the central technical challenge to this day, forcing spallation source development to attempt to achieve higher and higher effects of proton accelerators to achieve higher and higher neutron output (flux) from the target area.

Figures A.2, A.3, A.4 and A.5 above illustrate quite well how the basic physical layout of neutron scattering, synchrotron radiation, and free electron laser labs unite them: all use one technological centerpiece (an accelerator or reactor) to produce beams of radiation or neutrons that are transported to experimental stations where they are used. The experimental stations are used in parallel, for experiments of very different type and, importantly, in isolation from each other. As long as the accelerator or reactor performs as expected and delivers the neutrons or radiation to the stations, they are independent from each other, and any malfunctioning of equipment at one of them does not affect the others; likewise, the success of an experiment at one station is neither typically known nor of any direct significance for the users active at other experimental stations or the operators of the reactor or accelerator.

The contrast to particle physics is obvious and telling: there, whole accelerator complexes were designed and constructed with distinct areas of discovery (and even the discovery of specific particles) in mind. The often-used synonym of particle physics—"high energy physics"—is itself a testament to this, referring to the fact that the whole discipline is dependent on those high energies that can only be achieved with very large accelerators. The accelerators and reactors built to deliver neutrons, synchrotron radiation, and free electron laser have a generic and open-ended design. Their use is not known in advance, or even at their start of operation, other than in their very sketchy contours and with respect to the basic principles of how the neutrons and electromagnetic radiation are used as tools for experimentation.

Not only all experimental work with neutrons, but also significant shares of the applications of synchrotron radiation and free electron lasers can accurately be called "scattering". Beams of neutrons or electromagnetic radiation (x-rays, ultraviolet, or infrared light) are fired at a sample of the material that is to be studied, and thus scattered off the sample. Detectors (much like digital cameras) are placed around the sample to record the pattern of the scattered neutrons or radiation, and thus some of the sample's characteristics are mapped, such as molecular structure. For objects large enough to be visible to the human eye, the advantage of x-rays is their ability to reveal inner structures just like in medical settings or airport security checks. This is usually called "imaging" and has become a valuable tool in medicine, environmental studies, paleontology, archeology, and the history of arts; or in other words, all areas with a need to study objects using a very high level of detail. The only real drawback is that x-rays tend to be harmful in too-large doses, not only for living material but also for many of those materials and objects whose inner structures scientists want to study. Here, neutrons come in handy: neutral as they are, they are harmless and generally do not affect the sample studied at all. Moreover, neutrons can penetrate solid objects and large pieces of hard materials such as iron, steel, and ceramic, and hence can be used to provide "x-ray images" of things that x-rays would not reveal. The drawback of neutrons is that their neutrality makes them nearly impossible to steer, focus, and accelerate, but they can be slowed down comparably simply, which is useful for some experiments.

The real advantages of both neutrons and x-rays are on the level of what is not visible to the human eye, such as atomic and molecular structure, and here, neutron and x-ray scattering experiments have a significant role to play. The structural determination of bio-macromolecules with x-rays is a major application within the life sciences (see below), but neutron and x-ray scattering is used for a great range of other studies of the molecular and atomic structures of compounds. Neutrons are not only non-destructive but can be used to simultaneously reveal both the atomic structure of elements and compounds, and several other characteristics such as magnetism (Berggren and Matic 2012: 34). With pulsed neutron sources, time-resolved studies of large objects in motion can be done, such as "shooting movies" of the interiors of a running combustion engine and similar large mechanical structures. In some areas, therefore, neutrons are absolutely crucial for experiments. One disadvantage is that neutrons induce radioactivity in some materials, which can complicate the handling of samples after experimentation.

The high intensity of synchrotron radiation, and in particular the availability of high-intensity, so-called "hard x-rays", is the most important. Before the nature of x-rays was known, and wavelengths were not yet characterized, those x-rays with a higher penetrating power were designated "hard" x-rays, and the others "soft" (also, the name "x-rays" comes from this time period, with "x" denoting "unknown"). Today, the two categories of hard and soft x-rays correspond roughly to wavelength ranges from one nanometer to one angstrom (soft) and from one angstrom and down to a tenth of an angstrom (hard). Hard x-rays of high intensity are especially useful for the structural determination of molecules, which is typically done in measurements called diffraction or crystallography, essentially a variety of x-ray scattering that analyzes molecules and compounds that have been crystallized to achieve purity in the sample, to map the geometrical structure and constituent molecules and atoms. One of the most voluminous activities at synchrotron radiation laboratories today is the use of x-ray diffraction for structural determination of macromolecules like proteins, with huge relevance for the life sciences and not least also the pharmaceutical industry (Holmes 1998: 618). However, diffraction techniques are also regularly used for studies of materials and objects far larger than molecules, and for structural determination of compounds that are not biological.

Arguably, the most important impact of synchrotron radiation on the sciences, and the most important broadening of the synchrotron radiation user community, was the expansion of life sciences applications in the 1990s and beyond by the refinement of scattering/diffraction techniques and instrumentation. Figure A.6 shows an example of this, namely, the annual number of protein structures solved with the help of synchrotron radiation and deposited in the Protein Data Bank from the late 1970s and to today. The real growth began in the mid-1990s, and in 2014, 8888 structures resolved with synchrotron radiation were deposited in the Protein Data Bank. The development described by Fig. A.6 and by several other growth patterns in the use of synchrotron radiation by life science researchers has been due to the improvements in radiation handling and detector technology, as well as cooling, automation, computer software for data handling and interpretation, and special efforts to enhance the user support to accommodate biologists and biochemists in the lab environment.

Those experimental applications of synchrotron radiation and free electron laser that are not scattering are varieties of spectroscopy, whereby radiation is used to disturb the electronic structure of atoms by forcing electrons in the atoms to leave their positions. Different elements are receptive to radiation of different wavelength, and the moving electron also emits radiation with an element-specific wavelength that can itself be detected or allowed to trigger other events that reveal information about a compound on electronic, atomic, and molecular levels. Not only x-rays are used for such spectroscopic studies but also ultraviolet and infrared

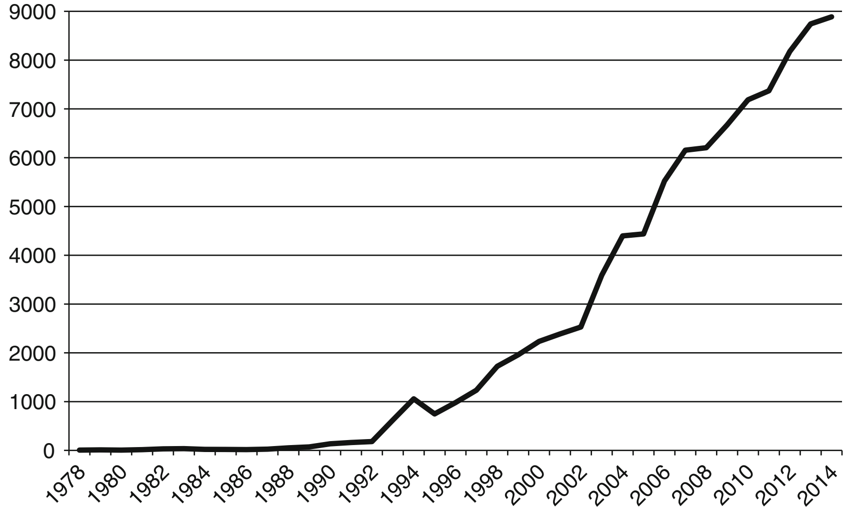

Fig. A.6 Annual number of protein structures solved with the help of synchrotron radiation and deposited in the Protein Data Bank, 1978–2014 (Source: Protein Data Bank 2016)

radiation, all of which can be produced with high intensity and manipulated to specific polarization and coherence with synchrotron radiation sources and free electron lasers. Various spectroscopic techniques are used to study the properties of materials, such as structure, strength, hardness, conductivity, magnetism, and resistance to external influences such as pressure and temperature. In addition to high brightness and the ability to polarize radiation and make it coherent, synchrotron radiation has the advantage that it can be tuned continuously over a wavelength range, which is especially useful because elements correspond to different wavelengths (Munro 1996). An important example from the very early days of synchrotron radiation, which shows the great leaps in performance that can be achieved with synchrotron radiation over other x-ray sources for spectroscopic techniques, are the EXAFS (Extended X-ray Absorption Fine Structure) experiments at the Stanford Synchrotron Radiation Project (SSRP) in 1974. In only three days of experimentation at Stanford, Farrell Lytle, one of the inventors of the EXAFS technique, managed to obtain as much data as he had gotten out of the previous ten years with the equipment in his home lab (Hallonsten 2015a: 239).

Table A.1 The 15 most common WoS-defined subject area categories among the 2014 journal publications reported by ESRF, ILL, and LCLS, in descending order

ESRF	ILL	LCLS
Multidisciplinary Materials Science	Physical Chemistry	Multidisciplinary Sciences
Biochemistry and Molecular Biology	Multidisciplinary Materials Science	Physical Chemistry
Physical Chemistry	Biochemistry and Molecular Biology	Multidisciplinary Materials Science
Applied Physics	Multidisciplinary Chemistry	Applied Physics
Multidisciplinary Chemistry	Biophysics	Optics
Condensed Matter Physics	Applied Physics	Condensed Matter Physics
Cell Biology	Multidisciplinary Physics	Biochemical Research Methods
Biophysics	Crystallography	Nanoscience and Nanotechnology
Nanoscience and Nanotechnology	Atomic, Molecular and Chemical Physics	Atomic, Molecular and Chemical Physics
Polymer Science	Condensed Matter Physics	Biochemistry and Molecular Biology
Metallurgy and Metallurgical Engineering	Polymer Science	Cell Biology
Multidisciplinary Physics	Inorganic and Nuclear Chemistry	Multidisciplinary Chemistry
Crystallography	Multidisciplinary Sciences	Crystallography
Biotechnology and Applied Microbiology	Nanoscience and Nanotechnology	Materials Science, Ceramics

Source: Web of Science database; ESRF, ILL joint publication database; LCLS online publications list. See Chap. 5 for details of the analysis.

Free electron lasers have often been called "fourth" or "next" generation light sources and thus seen as sequels to storage ring-based synchrotron radiation sources. But free electron laser should probably be viewed rather as a radical and very specific refinement of some of the extreme performance parameters of synchrotron radiation by the use of linear accelerators and very long insertion devices to produce a laser in the ultra-violet and x-ray ranges, to be used in some very specialized experiments. The extremely high brightness of the radiation, its coherence, and the ability to achieve pulses of lengths of only femtoseconds (one quadrillionth, or one millionth of one billionth, of a second), have opened new experimental opportunities that often defy simple and straightforward characterization and categorization. Although the sciences advertised as beneficiaries of the free electron laser produced at the Linac Coherent

Light Source (LCLS) at SLAC National Accelerator Laboratory in California are well-known disciplinary categories ("Atomic, molecular & optical", "Biology", "Chemistry", and so on), the description of instruments evade these categories and rather point out areas of use that are specific to the particular technical performance of the facility: "Coherent x-ray imaging", "Matter in extreme conditions", "Macromolecular femtosecond crystallography", and so on (see Chap. 3).

Table A.1 shows the ten most common subject area categories (defined and assigned to journals by the Web of Science database) represented among the journal publications emerging from the ESRF, the ILL, and LCLS, to demonstrate the disciplinary breadth of the uses of these labs. There are clear similarities but also differences between the three labs. The table should be read with some care, as the data has several caveats (see Chap. 5), but as a general illustration of the disciplinary breadth and variety of the sciences that make use of neutron scattering, synchrotron radiation, and the free electron laser, the table is quite apt.

Appendix 2 contains a list of neutron scattering, synchrotron radiation, and free electron laser labs in operation and under construction in Europe and the United States, as of late 2015. The list is not exhaustive, but contains major labs with well-developed user programs. Most of these, if not all, are organized according to some common principles, within which there is some variation.[1] As noted repeatedly throughout the book, neutron scattering, synchrotron radiation, and free electron laser labs are predominantly user facilities. This means that they build, operate, and maintain a set of experimental stations that scientists from universities and other public research organizations (like institutes), and also some users from the corporate world, make use of on temporary basis as visitors. Experimental time is provided free of charge to anyone who hands in an experimental proposal on a certain deadline (normally once a year) and passes the peer review of this proposal by a committee of experts convened

[1] Examples of user policies, where individual variation between labs can be noted and compared, can be found at the websites of e.g. the Spallation Neutron Source (SNS) (http://neutrons.ornl.gov/users), the European Synchrotron Radiation Facility (ESRF) (http://www.esrf.eu/UsersAndScience), the Paul Scherrer Institute that operates the neutron source SINQ and the synchrotron radiation source SLS (https://www.psi.ch/science/psi-user-labs), and most other labs listed in Appendix 2.

by the facility. Users are normally obliged to publish the results of their work openly, in academic journals, monographs, or conference proceedings (note that "openly" does not mean open access but just academic, non-proprietary, publications), and to submit to the facility the bibliographic details and/or prints of these publications. Should a user from an industrial firm not want to publish the results, she can short-circuit the whole process by simply purchasing experimental time. Several labs offer services like web-based remote-access interfaces for experiments that are comparably simple in their technological setup, and some even offer the possibility for scientists to send samples to the lab to be analyzed by skilled staff, who then return the data from the analysis.

For a comprehensive description and analysis of the organizations of neutron scattering, synchrotron radiation, and free electron laser labs, see Chap. 3. For a thorough analysis of bibliometric data and a wider discussion about user communities and productivity of these labs, see Chap. 5.

Appendix 2

List of Major Synchrotron Radiation, Free Electron Laser, and Neutron Spallation User Facilities in Operation and Under Construction in Europe and the United States, 2015

Reactor-Based Neutron Sources, Continuous Beam

- High-Flux Isotope Reactor (HFIR), Oak Ridge National Laboratory (US federal research lab), Oak Ridge, Tennessee; started operation in 1966
- NIST Center for Neutron Research (NCNR), National Institute of Standards and Technology (NIST) (US governmental agency), Gaitersburg, Maryland; started operation in 1969
- Institut Laue Langevin (ILL) (European intergovernmental collaboration, French private company), Grenoble, France; started operation in 1972
- Berlin Research Reactor 2 (BER-2), Helmholz-Zentrum Berlin (German federal research lab), Berlin, Germany; started operation in 1973
- Orphée, Laboratoire Léon Brillouin (French national research lab), Saclay, France; started operation in 1980
- Forschungsreaktor München II (FRM-II), Technische Universität München, Germany; started operation in 2002

O. Hallonsten, *Big Science Transformed*,
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Accelerator-Based (Spallation) Neutron Sources, Pulsed

- ISIS, Rutherford Appleton Lab (British national research lab), Oxfordshire, UK; started operation in 1985
- Los Alamos Neutron Science Center (LANSCE), Los Alamos National Lab (US federal research lab), Los Alamos, New Mexico; started operation in 1988
- Spallation Neutron Source (SNS), Oak Ridge National Laboratory (US federal research lab), Oak Ridge, Tennessee; started operation in 2006
- European Spallation Source (ESS) (European intergovernmental collaboration), Lund, Sweden; under construction, planned start of operation is 2020

Accelerator-Based (Spallation) Neutron Sources, Continuous Beam

- SINQ, Paul Scherrer Institute (PSI) (Swiss federal research lab), Villigen, Switzerland; started operation in 1996

Parasitic Synchrotron Radiation Facilities on Storage Rings ("First Generation")

- Cornell High Energy Synchrotron Source (CHESS), Cornell University, Ithaca, New York; started operation in 1980

Storage Ring-Based Synchrotron Radiation Sources Optimized in the UV/Soft X-Rays Regime, Using Bending Magnets ("Second Generation")

- Aladdin, University of Wisconsin, Madison, Wisconsin; started operation in 1981
- National Synchrotron Light Source (NSLS) (first ring), Brookhaven National Laboratory (US federal research lab), Upton, New York; started operation in 1982
- MAX I, Lund University, Lund, Sweden; started operation in 1987
- ASTRID (Aarhus STorage RIng in Denmark), ISA Center for storage ring facilities (Danish national research lab), Aarhus, Denmark; started operation in 1990

Storage Ring-Based Synchrotron Radiation Sources Optimized in the Hard X-Rays Regime Using Bending Magnets ("Second Generation")

- National Synchrotron Light Source (NSLS) (second ring), Brookhaven National Laboratory (US federal research lab), Upton, New York; started operation in 1984

Storage Ring-Based Synchrotron Radiation Sources Optimized in the UV/Soft X-Rays Regime, Purpose-Built for the Use of Insertion Devices ("Third Generation")

- Elettra Sincrotrone Trieste (Italian national research lab), Trieste, Italy; started operation in 1993
- Advanced Light Source (ALS), Lawrence Berkeley National Lab (US federal research lab), Berkeley, California; started operation in 1993
- Dortmund Electron Storage Ring Facility (DELTA), Technische Universität Dortmund, Dortmund, Germany; started operation in 1995
- MAX II, Lund University, Lund, Sweden; started operation in 1997
- BESSY II, Helmholz-Zentrum Berlin (German federal research lab), Berlin, Germany; started operation in 1998
- MAX III, Lund University, Lund, Sweden; started operation in 2007
- ASTRID 2 (Aarhus STorage RIng in Denmark 2), ISA Center for storage ring facilities (Danish national research lab), Aarhus, Denmark; started operation in 2012
- MAX IV (small ring), Lund University, Lund, Sweden; under construction, planned start of operation is 2016

Storage Ring-Based Synchrotron Radiation Sources Optimized in the Hard X-Rays Regime, Purpose-Built for the Use of Insertion Devices ("Third Generation")

- European Synchrotron Radiation Facility (ESRF), (European intergovernmental collaboration, French private company), Grenoble, France; started operation in 1994

- Advanced Photon Source (APS), Argonne National Laboratory (US federal research lab), Argonne, Illinois; started operation in 1996
- Swiss Light Source (SLS), Paul Scherrer Institute (PSI) (Swiss federal research lab), Villigen, Switzerland; started operation in 2001
- Angstromquelle Karslruhe (ANKA), Karlsruhe Institute of Technology (German federal research lab), Karlsruhe, Germany; started operation in 2003
- SPEAR3, SLAC National Accelerator Laboratory (US federal research lab), Menlo Park, California, started operation 1974; major upgrade finished in 2003
- Soleil (French national research lab), Saint-Aubin, France; started operation in 2006
- Diamond (British national research lab), Oxfordshire, UK; started operation in 2007
- PETRA III, Deutsches Elektronen Synkrotron (DESY) (German federal research lab), Hamburg, Germany; started operation in 2009
- ALBA (Spanish national research lab), Barcelona, Spain; started operation in 2010
- National Synchrotron Light Source II (NSLS-II), Brookhaven National Laboratory (US federal research lab), Upton, New York; under construction, planned start of operation in 2015
- MAX IV (large ring), Lund University, Lund, Sweden; under construction, planned start of operation in 2016
- Solaris (Polish national research lab), Krakow, Poland; under construction, planned start of operation in 2016

Linac-Based Free Electron Lasers

- FLASH (Free Electron Laser Hamburg), Deutsches Elektronen Synchrotron (DESY) (German federal research lab), Hamburg, Germany; started operation in 2005
- Linac Coherent Light Source (LCLS), SLAC National Accelerator Laboratory (US federal research lab), Menlo Park, California; started operation in 2009
- FERMI (Free Electron laser Radiation for Multidisciplinary Investigations) at the Elettra Sincrotrone Trieste (Italian national research lab), Trieste, Italy; started operation in 2010

- European XFEL (European intergovernmental collaboration, German private company), Hamburg, Germany; under construction, planned start of operation in 2016
- SwissFEL, Paul Scherrer Institute (PSI) (Swiss federal research lab), Villigen, Switzerland; planned start of operation in 2017

Sources: Updated tables from Rush (2015: 152) and Hallonsten and Heinze (2015: 843).

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Index

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