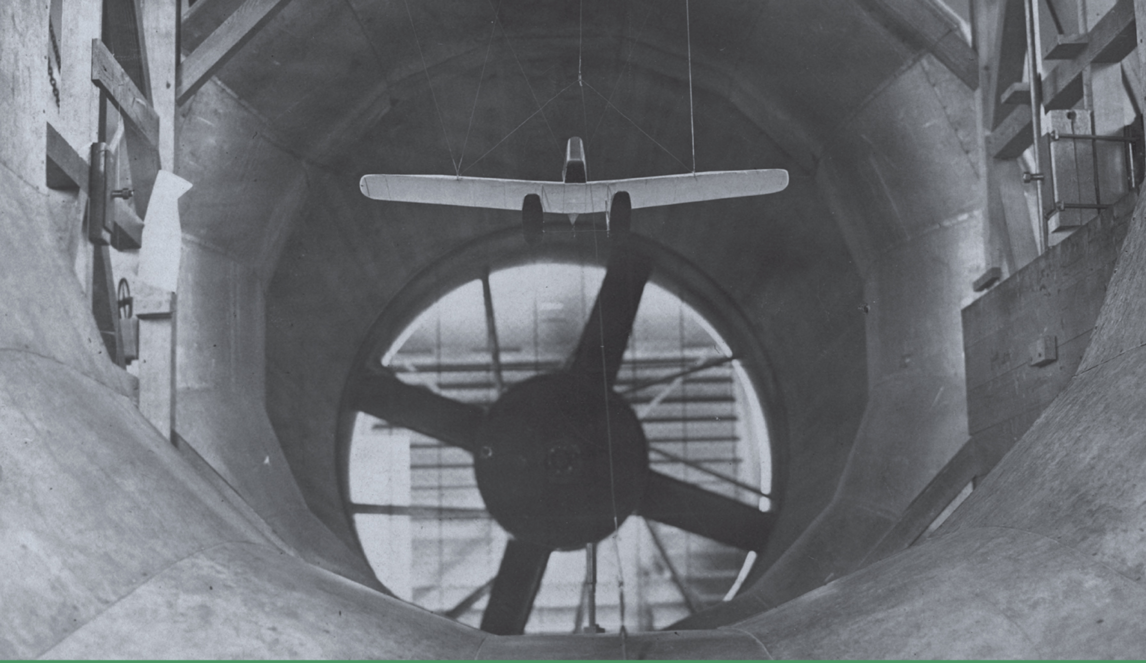
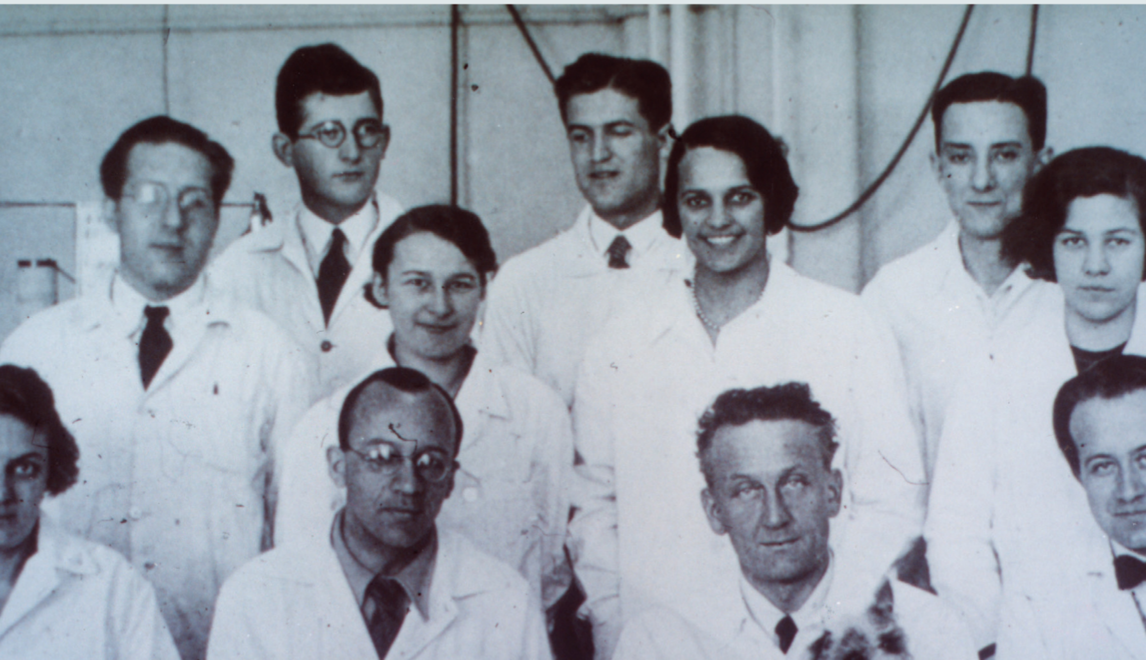


EUROPEAN CONCEPTUAL HISTORY

# BASIC AND APPLIED RESEARCH

## THE LANGUAGE OF SCIENCE POLICY IN THE TWENTIETH CENTURY

Edited by **David Kaldewey · Désirée Schauz**



# Basic and Applied Research

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# Basic and Applied Research

## The Language of Science Policy in the Twentieth Century

Edited by
David Kaldewey and Désirée Schauz

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# Preface

It is a truism that collaboration is the key to success. In the case of this volume, collaboration between scholars from different countries and disciplines was the key to success. Everything began with a chance encounter between a historian of science and a sociologist of science, who soon discovered their common interest in conceptual approaches to science policy. This set the scene for a fruitful interdisciplinary collaboration between the two of us. We both had studied historical science policy discourses in Germany and the United States. Analyzing these two countries suggested that the concepts of basic and applied research played a key role in shaping science policy. In order to explore the global synchronization of this distinction on the one hand, and the cultural variation of science policy discourses on the other hand, we decided to bring together scholars with expertise in the history of science policy in other countries who could widen our perspective.

We presented and discussed our project on several occasions and received invaluable feedback. We would like to thank Benoît Godin, Graeme Gooday, Lea Haller, Alexei Kojevnikov, John Krige, Dominique Pestre, Roger Pielke, Jr., Steven Shapin, and Rudolf Stichweh for discussing shifting notions of basic and applied research with us. Moreover, this volume benefited from the joint reflections within CASTI, an international network of scholars interested in "Conceptual Approaches to Science, Technology and Innovation" (www.casti.org). Together we have elaborated a common research program on the semantic field of science, technology, and innovation. We would thus like to extend our gratitude to all members of the network for being such a stimulating group of scholars and such a likeable bunch of friends.

Many others have contributed to the publication of this volume. We are indebted to Jason Chumtong for excellent editorial and technical assistance. At various stages, the project benefited from the support of Ivonne Weyers, Deepal Doshi, Silke Engels, and Lars Fritzsche. We are also grateful for the valuable comments and suggestions of the two anonymous referees. Our special thanks go out to Willibald Steinmetz and Michael Freeden, who showed great interest in our studies of science, technology, and innovation as an emerging

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field within conceptual history and agreed to include the book in the European Conceptual History series. Finally, we are much obliged to the authors contributing original research papers to our book in a time when evaluation regimes put pressure on publishing journal articles instead of books.

*David Kaldewey and Désirée Schauz, March 2018*

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# Abbreviations

| | |
|---|---|
| AAAS | American Association for the Advancement of Science |
| AfAF | Ausschuss für angewandte Forschung, Committee for Applied Research of the German Research Foundation |
| ARC | Agricultural Research Council (U.K.) |
| BBC | British Broadcasting Corporation |
| BMBF | Bundesministerium für Bildung und Forschung, Federal Ministery for Education and Research (F.R.G.) |
| CAS | Chinese Academy of Science |
| CDW | Colonial Development and Welfare (U.K.) |
| CMRI | Colonial Microbiological Research Institute (British Empire) |
| COS | Central Office of Statistics (Hungary) |
| CPRC | Colonial Products Research Council (U.K.) |
| CRC | Colonial Research Committee (U.K.) |
| CRS | Colonial Research Service (U.K.) |
| DARPA | Defense Advanced Research Projects Agency (U.S.) |
| DFA | Deutsche Forschungsanstalt für Psychiatrie, German Research Institute for Psychiatry of the KWG |
| DFG | Deutsche Forschungsgemeinschaft, German Research Foundation |
| DOE | Department of Education (U.S.) |
| DSIR | Department of Scientific and Industrial Research (U.K.) |
| EC | European Communities |
| EMBO | European Molecular Biology Organization |
| ERC | European Research Council |
| ERP | European Recovery Program |

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| | |
|---|---|
| ESF | European Science Foundation |
| EU | European Union |
| FCCSET | Federal Coordinating Council for Science, Engineering, and Technology (U.S.) |
| FhG | Fraunhofer Gesellschaft zur Förderung der angewandten Forschung, Fraunhofer Society for the Promotion of Applied Research (F.R.G.) |
| FP | Framework Programme for Research and Technological Development (EU) |
| F.R.G. | Federal Republic of Germany |
| G.D.R. | German Democratic Republic |
| GG | Grundgesetz, Basic Law (F.R.G.) |
| HAS | Hungarian Academy of Sciences |
| HCS | Hungarian Council of Sciences |
| HPCC | High Performance Computing & Communications (U.S.) |
| KWG | Kaiser-Wilhelm-Gesellschaft, Kaiser Wilhelm Society (Germany) |
| KWI | Kaiser-Wilhelm-Institut, Kaiser Wilhelm Institute (Germany) |
| MIT | Massachusetts Institute of Technology |
| MP | Member of Parliament (U.K.) |
| MPA | Archiv zur Geschichte der Max-Planck-Gesellschaft, Archive of the MPG (F.R.G.) |
| MPG | Max-Planck-Gesellschaft, Max Planck Society (F.R.G.) |
| MPI | Max-Planck-Institut, Max Planck Institute (F.R.G.) |
| MRC | Medical Research Council (U.K.) |
| MTA LT | Archives of the Hungarian Academy of Sciences |
| NA | National Archives (U.K.) |
| NAS | National Academy of Sciences (U.S.) |
| NASA | National Aeronautics and Space Administration (U.S.) |
| NIH | National Institutes of Health (U.S.) |
| NRC | National Research Council (U.S.) |
| NS | National Socialism (Germany) |
| NSF | National Science Foundation (U.S.) |

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| | |
|---|---|
| OECD | Organisation for Economic Co-operation and Development |
| OSTP | Office of Science and Technology Policy (U.S.) |
| P.R.C. | People's Republic of China |
| R&D | Research and Development |
| RFR | Reichsforschungsrat, Reich Research Council (Germany) |
| RRI | Responsible Research and Innovation |
| STI | Science, Technology, and Innovation |
| STS | Science and Technology Studies |
| U.K. | United Kingdom |
| U.S. | United States of America |
| USAID | U.S. Agency for International Development |
| ZFT | Zentralamt für Forschung und Technik, Central Office for Research and Technology (G.D.R.) |

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Introduction

# Why Do Concepts Matter in Science Policy?

*Désirée Schauz and David Kaldewey*

Fifty years ago, Daniel S. Greenberg (1967), responsible for the "News and Comments" section in the journal *Science,* published his well-known study, *The Politics of Pure Science.* With massive government support for research during World War II and the launch of a federal funding program after 1945, the United States had entered a new era of science policy. As a critical commentator of this transition, Greenberg exposed that the politics of science was not inherently different from any other form of politics, and included lobbying, rhetoric, and ideology. According to Greenberg, what lay at the heart of the politics of science was the scientists' claim of autonomy as a necessary precondition for the technological progress that might flow from their work. "The only difficulty," he remarked, "is that when the origins of technological developments are subjected to systematic, scholarly analysis, the question of the debt to pure research turns out to be riddled with far more paradoxes, puzzles, and uncertainties than the statesmen of pure science are generally willing to admit" (Greenberg 1967: 29).

The promise that science, if autonomous, would pay off in the long run was not easy to carry through in practice. To make the claim plausible, various communication strategies were necessary; one of them—of which Greenberg himself was not aware (Kaldewey and Schauz 2017)—was to replace the traditional term "pure science" with "basic research." Yet this was not the only problem the politics of science had to face in Cold War America: scientific expertocracy put both scientific credibility and democracy at risk; scientists and the public were confronted with the ethical dilemmas of the advanced scientific-technological world; and finally, in the ideologically tense atmo-

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sphere, political loyalty and secrecy policies became pivotal requirements for science. Science policy, therefore, was in need of narratives and rationales dealing with these tensions.

At the same time, the United States became the key player in a global system of science. Beyond federal support for research, the politics of pure science was also coupled with foreign policy. The concept of basic research was "the key node articulating American hegemony with the postwar reconstruction of science in Europe" (Krige 2006: 3). In the second half of the twentieth century, the United States not only became the most successful nation in scientific output, but also provided a conceptual language to make sense of the nexus between science and politics. This comes as a surprise because America had for a long time lagged behind Europe's science nations. Furthermore, in contrast to Europe, there was no strong tradition of a U.S. federal research policy before the twentieth century (Dupree [1957] 1986); and until the early twentieth century, the language of science and science policy was mainly British, German, and French (chapters 1, 2, and 3).

This volume is about concepts and, more generally, about language being an integral part of the politics of science. It deals, above all, with the distinction between "basic research" and "applied research." Both of these had become key concepts of twentieth-century science policy around the globe, and their distinction turned out to be a tremendously successful communication strategy for coping with conflicting demands toward science in modern societies. While the term "basic research" has often been associated with Vannevar Bush—an engineer, research manager, and science advisor—and his famous report delivered to the U.S. president in 1945, the genesis of the concept can be traced further back in history. Similarly, the general expectation that the advancement of scientific knowledge leads to technical progress and prosperity is not specific to the twentieth century; it has been present in the development of the sciences from their very beginning in early modern Europe. The history of global science policy is the history of concepts traveling through time and between cultural contexts, followed by a period of conceptual synchronization. Yet, while the concepts of basic research and applied research finally prevailed in most national science policies, their meaning and use still vary between different cultures and national settings.

Against this background, we employ a broad notion of "science policy." In a narrow sense, science policy points to a new policy field established in the course of the twentieth century, when governments became big sponsors of scientific research and thus built up special departments, agencies, and advisory boards that dealt exclusively with the increasing relevance of science to national welfare. In a broader sense, however, the term "science policy" refers to various processes of "political accommodation among science, society,

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and the state" (Jasanoff 1990: 250; see also Slotten 1993: 43). For example, long before nation-states became the most important sponsors of research, other stakeholders supported science by financing universities, museums, or other scientific institutes. With regard to the United States, where for a long time science had largely depended on private benefactors, Hunter Dupree ([1957] 1986)[1] identifies constitutional debates about the necessity of a national university and issues of patent policy in the late eighteenth century as an early sign of the federal acknowledgement of the importance of science, even though these initiatives were not successful at the beginning.

Dichotomies such as "pure versus applied science" or "basic versus applied research" played an important role within the development of modern science policy across national borders by building on and transforming values that had been previously conveyed in the ancient and early modern distinction between "theory" and "practice."[2] In contrast to the Western nomenclature, Marxist ideology even revived the distinction between theory and practice and turned these two terms into key concepts of Communist science policy in the decades after World War II. Such concepts, however, were never stable, and their meanings and discursive roles varied according to time and place. They are, in many respects, similar to what in intellectual history and political philosophy are known as "essentially contested concepts" (Gallie 1955/1956; Connolly 1993). In the United States, right in the heyday of the ideal of basic research in the mid-1960s, unfulfilled expectations stimulated critical debates about federal spending for basic research, particularly about whether it was appropriate to dissociate basic research from applied research.[3] In the following decades, the simplistic rationales associated with these terms lost their grip. Today, half a century later, prominent scholars perceive concepts such as "basic research" or "applied research" as old-fashioned, outdated, and ideological.[4]

The history of the basic/applied distinction is related to the history of the so-called "linear model of innovation"—a conceptualization of the relation between science and technology that asserts a causal chain from basic and applied research to technological innovation. The concept of the linear model is of relatively recent origin. It gained prominence in the 1950s and 1960s, first and foremost in economic research to grasp processes of technological change (Godin 2006). Its conceptual core, however—the assumption that pure science leads to useful knowledge, which in turn enables new technologies—is much older. For three decades now, various observers have rejected the linear model and argued that it has never been able to capture the complex linkage between science and technology (e.g., Rosenberg 1991; Stokes 1997; Pielke and Byerly 1998; Edgerton 2004; Fagerberg 2005; see also final chapter). Its decline parallels the decline of what Greenberg had called the politics

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of pure science. In the meantime, the debate on how science and technology are related to one another has resulted in a more general controversy in science and technology studies (STS) and in science, technology, and innovation (STI) policy studies. Many scholars state that we are now living in an era of "techno-science" and that the old hierarchy between science and technology has been turned upside down (e.g., Forman 2007). Given the vast array of critical commentators, it seems as if the very language used to describe the interactions between science, politics, and industry in the twentieth century has lost its meaning.

Today, neither scholars nor policy makers dispose of a generally approved conceptual framework to analyze and communicate the politics of science. The linear model has, notwithstanding the critiques, "not yet been successfully replaced by a new orthodoxy" (Wengenroth 2000: 28), and "a new political consensus has yet to emerge to replace 'basic research' as a central, organizing symbol" (Pielke 2012: 341). Against this background, it is no surprise that, over the past decades, new terms have been proposed again and again—"strategic research," "post-normal science," "mode 2," "triple helix," or more recently, "responsible research and innovation"—to understand how science and society interact in the twenty-first century (Flink and Kaldewey 2018). However, these terms are irrelevant outside of their respective epistemic communities. In the real world of science policymaking and in the identity work of normal scientists and engineers, most of these semantic variations are merely used as rhetorical embellishment. Furthermore, none of these recent semantic variations has proven successful in communicating science policy to a broader public.

The old-fashioned terms "basic" and "applied research," in contrast, are still frequently used by individual scientists as well as by universities and research institutions (Gulbrandsen and Kyvik 2010; Bentley, Gulbrandsen, and Kyvik 2015; Kaldewey and Schauz 2017). Not least, they have become embedded in everyday language, well beyond professional contexts.[5] With the turn of the millennium, we have witnessed the dawn of new basic research programs conducted by a number of agencies in various places around the globe. In 1997, the Chinese government initiated the National Program on Key Basic Research Project (973 Program),[6] as part of the Tenth Five-Year Plan. In 2011, the Korea Institute for Basic Science was established to support "groundbreaking discoveries in basic science research" and to "disseminate knowledge that will impact the development of society and benefit the wellbeing of humanity."[7] Last, but not least, the European Research Council (ERC), established in 2005, is the first EU institution dedicated to strictly science-driven basic research funding—and generally perceived as a huge success (final chapter). In short, the concept of basic research is still alive.

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The same goes for the old idea that scientific research ultimately triggers technological innovation and thus improves national competitiveness. This narrative was reiterated without batting an eyelash, for example, in President Barack Obama's "Strategy for American Innovation" (White House 2009), or in the "High Tech Strategy" issued by the federal government of Germany (BMBF 2010). Obviously, the linear model remains indispensable for communicating science policy in the twenty-first century. Again, all of this might come as a surprise as the distinction between basic and applied research, as well as the linear model of innovation, has been essentially contested and dismissed as ideological baggage for decades, particularly by STS and STI policy scholars.

Why is this the case? To answer this question, this volume analyzes the historical legacy incorporated in the semantics of basic and applied research and investigates the functions of these terms in communicating science policy under circumstances of divergent interests, political ideologies, and ethical dilemmas.

## A Symmetrical Approach to the Dichotomy

The question of why concepts such as "basic and applied research" are essential for communicating science policy has been neglected for decades by STS and STI policy studies. There are several reasons for this conceptual deficit. First, scholars traditionally concentrated on criticizing the analytical value of those categories and thereby failed to understand their historicity and discursive functions. Second, there is a general reluctance in STS with regard to the semantic aspects of science and science policy—the role of language has not been a popular topic in science studies for some decades.[8] This reluctance can partly be explained by path dependencies in the STS field: after the implementation of social constructivist approaches in the 1970s and after the "practice turn"[9] had gained momentum in the 1980s, discourses on questions of ethos, scientific ideas, and identity work were seen as acts of mere ideological purification intending to disguise the real motives and interests of scientists and engineers.

The third reason relates to how the relationship of basic and applied research is expounded in the more recent literature. In contemporary diagnoses, what seems to be at the core of attention is the increasing "pressure of practice" (Carrier, Howard, and Kourany 2008), "commercialization" (Siegel, Veugelers, and Wright 2007; Mirowski and Sent 2008), "commodification" (Jacob 2009; Radder 2010), "valorization" (De Jonge and Louwaars 2009; Bos et al. 2014), "academic capitalism" (Slaughter and Leslie 1997; Slaughter and Rhoades 2004), and "campus capitalism" (Greenberg 2007). STS scholars

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are traditionally engaged in criticizing the power of economic rationales in science policy. At the same time, they are deeply skeptical about the ideology of "pure science," which they suspect prevails in ivory tower academia. Therefore, hardly anyone in the field today would counter the economic colonization of research by calling for more autonomy of basic research or by claiming a science "for its own sake." It seems more opportune to propose or employ alternative notions of applied research, such as "transdisciplinarity," "translational research," "responsible research," or "science in the public interest." What is missing, however, is a reflection on whether and how these seemingly new categories reproduce the old distinction between basic and applied research. In other words, STS scholars want science to be applied, but applied in the right way—to serve, for example, the public instead of the military, civil society instead of the economy, and the patients instead of the pharmaceutical industry. This engagement—as justifiable as it may be—has made it difficult for the STS community to actually understand and explain the persistent relevance of the basic/applied distinction, as well as the way it still influences contemporary science policy. What is more, neither the realist assumption that there exists something like basic and applied research nor the opposite critical-constructivist assumption that these concepts conceal the actual nature of scientific performance is helpful for getting to the bottom of the question of how language is relevant to the politics of science.

In contrast to those STS debates, Peter Galison (2008) and Peter Dear (2005, 2012) accentuate that understanding the meanings of concepts such as pure science, as well as the relationship of contemplative and instrumental forms of knowledge, is one of the pressing problems to be addressed in the history and philosophy of science. Steven Shapin (2001, 2012), in line with this revived interest in historicizing modern notions of science, has broadened the scope to "metascience" discourses, which address the role of science and research more generally—in contrast to a focus on epistemic and disciplinary aspects of scientific discourses characterized by specific technical language. Furthermore, in his study on *The Scientific Life* in late modern America, Shapin (2008) includes the nonacademic sphere as well. He demonstrates how scientists both in academia and in industrial contexts have to position themselves and their work in a complex field that we may characterize using distinctions such as those between basic and applied research. For example, Shapin (2008: 210) presents a historically relatively new figure, the "scientific entrepreneur," and describes the tension inherent in this role: "They have one foot in the making of knowledge and the other in the making of artifacts, services, and, ultimately, money. They may or may not be aware of any 'conflict' between these aspects of their identity, but they embody drives and ac-

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tivities that, during the course of the twentieth century, were widely held to be in tension, and sometimes in opposition." In terms of methodology, Shapin's perspective can be characterized as a symmetrical approach that avoids any bias toward specific notions and valuations of either side of the distinction. In other words, "pure science" is not more ideological than "applied science," and vice versa. It is the tension between both concepts that is crucial. The task is thus to ask for the ideological underpinnings of the whole range of concepts relevant to the politics of science.

In a broader sense, this claim of symmetry applies, in fact, to the whole vocabulary that is significant for both scholarly and public debates. Particularly important are those concepts that Reinhart Koselleck (1972) has described as fundamental concepts (*Grundbegriffe* or *Leitbegriffe*) and those that Roy Harris (2005: viii) characterizes as "prestigious conceptual supercategories in terms of which the intellectual life of the modern world is organized." First of all, we have to think about terms that are often taken as given in the natural language, such as "science," "technology," "research," and "innovation." A closer look at this semantic field reveals a set of concepts that represents the essence or overall attributes of science and technology, such as "objectivity," "rationality," and "progress." Within this range, basic and applied research represent still another dimension of metascientific concepts, namely those terms of science policy that are mainly deployed for categorizing, organizing, and specifying the fields of science, research, and technology. Other examples of concepts fulfilling this function are "pure science," "excellence," "interdisciplinarity," "transdisciplinarity," "big science," and "strategic research." Not all of these terms, however, are used in the same dichotomous way as basic and applied research.[10]

In this volume, we want to take this methodological intuition one step further. To grasp the paradox that concepts such as basic and applied research are heatedly contested, while at the same time remain indispensable and of persistent relevance for communicating science policy, we analyze them not in analytical terms but as historical semantics. We propose to apply conceptual history (Koselleck 2002, 2004, 2006; Olsen 2012; Wimmer 2015; Müller and Schmieder 2016; Pernau and Sachsenmaier 2016) to the field of history of science and science policy. While there is a long tradition of conceptual approaches in political philosophy (Gallie 1955/1956; Skinner 1969; Connolly 1993), scholars have only recently begun to address the origins and shifting meanings of supercategories such as "science" (e.g., Harris 2005; Phillips 2015), "technology" (e.g., Schatzberg 2006, 2012), or "innovation" (e.g., Godin 2015a, b)—a trend that has hardly been noticed among the conceptual history community (Schauz 2014b).[11] For a long time, distinctions like "pure

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and applied science" or "basic and applied research" have been interpreted merely as the professional boundary work or funding rhetoric of scientists (Gieryn 1983, 1999; Meinel 1983; Slotten 1993; Kline 1995; Waterton 2005; Calvert 2006). More recent studies, which are synthesized and carried forward in this book, indicate that such distinctions have more complex functions and manifold meanings.[12]

Metascientific concepts emerge in public as well as in scholarly discourses across different disciplines. By focusing on the polysemy of the aforementioned concepts, we can examine how they serve as "collective symbols" (Link 1986), "boundary objects" (Star and Griesemer 1989), "travelling concepts" (Bal 2002), or "nomadic concepts" (Andler and Stengers 1987) that link different discursive groups with diverse, and often conflicting, interests and values. Conceptual history reminds us not only of semantic shifts, but also of the possibility that, at the same time, older meanings continue to exist and are revived every now and then, even though these historical references often imply an act of "invention of tradition" (Hobsbawm and Ranger 1983). Furthermore, conceptual approaches from linguistics and semiotics help us to understand that all concepts are part of broader semantic fields, including conceptual variations and related terms (Schmidt 1973; Eco 1976; Lyons 1977). Thus, it would be wrong to simply equate terms such as "pure science," "fundamental research," "basic research," or "blue sky research."[13] Rather, we have to examine whether and how such subtle semantic differences indicate historical shifts and transformations in the way we think about how science and society relate to each other.

Finally, following more recent discussions, scholars are increasingly building bridges between conceptual history on the one hand and discourse analysis or approaches to narratives and social imaginaries on the other.[14] The crucial insight from this strand of literature is that concepts are not free-floating entities but embedded in discourses, narratives, or imaginaries that in themselves are much more difficult entities to grasp. As a consequence, we aim to reconstruct how specific concepts such as basic and applied research are embedded and utilized in broader science policy discourses. The studies in this volume are therefore based on a great variety of sources: archival material from research organizations, governments, and administrative agencies; grey literature such as science policy and funding reports; journalistic reports published in newspapers and magazines; and, of course, scholarly books and articles published in scientific journals. Concerning the latter, it is important to note that it often does not make sense to distinguish between primary and secondary sources—metadiscourse about the language of science is itself rooted in a historical context. Therefore, in

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each chapter, the various sources are merged into only one bibliography that contains all printed sources. Archival and anonymous sources, in contrast, are specified in the notes.

## Historical Semantics and Historical Shifts in Science Policy

Debates on science policy have always been embedded in historical narratives and rationales. Whereas historical references are sometimes employed explicitly, most of the time the semantic legacy of science policy concepts goes unnoticed and operates subconsciously. There is, in other words, a tacit dimension in science policy discourses that is constitutive not only for political practice but also for scholarly discourses. This becomes evident in those contributions from STS and STI policy studies that aim to demonstrate that scientific research today is no longer what it used to be, and to explain why. The 1990s and 2000s produced a bulk of literature on changing modes of research, research organization, and science policy.[15] A common denominator of this literary genre is the assumption of a fundamental transition or an "epochal break" (Nordmann, Radder, and Schiemann 2011) that sharply dissociates the way research is conducted today from the way science used to be organized in the past. For example, many scholars refer to a traditional era of science that was characterized by discipline-oriented basic research at universities, which came to an end in the 1980s. This implies a historical image of modern science that presumes continuity from the nineteenth century era of pure science to the postwar period of basic research, which is evidently very far from actual historical developments in the nineteenth and early twentieth centuries.

In addition, in the field of STI policy studies, there is a wide strand of literature about the contexts and conditions of innovation, particularly on the historical origins and variations of national innovation systems.[16] These scholars hope to gain insights from history into how exactly scientific research contributes to innovation processes. They seek to find historical answers to the question of why and when national research policies succeeded in, or failed at, stimulating innovation. According to these "innovation studies," the early twentieth century appears as a first period in which the national research landscapes were transformed and the settings for the so-called national innovation systems were shaped.

These scholarly traditions, in all their variation and heterogeneity, share an interest in developing analytic categories and typologies to describe succeeding periods of science and historically changing science policy regimes. While we are interested in the problem of historical periodization as well,

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we pursue a different approach. We are interested in how actors themselves perceived and conceptualized the relationship between science, technology, and politics at different points in time. A similar change of viewpoint was proposed already in 1976 by Otto Mayr (1976: 670), who discussed the science-technology relationship as a "historiographic problem": "So far we have defined 'science' and 'technology' in our own terms and have then tried to analyze their relationship through the course of history from our own vantage point. Instead, we should recognize that the concepts of science and technology themselves are subject to historical change; that different epochs and cultures had different names for them, interpreted their relationship differently, and, as a result, took different practical actions." Mayr's idea, however, did not resonate in the history of science community until the 1990s (Kline 1995: 194–195). Furthermore, Mayr concentrated on a specific debate about the division of labor between history of science and history of technology. In contrast, our aim is to deal more generally with all those categories and metascientific discourses that make sense of how science relates to its societal environment, with a focus on the nexus between science and politics. We analyze how scientists, engineers, private stakeholders, and policymakers used concepts, first to negotiate goals and organizational settings of research, and second to cope with conflicting interests and problems emanating from science and its societal applications. The primary objective of this volume thus is to reconstruct the origins, multiple meanings, semantic transformations, and discursive functions of the semantic portfolio of science and science policy.

According to Reinhart Koselleck (2004: 223), language and concepts are "no mere epiphenomena of so-called real history," but rather frame both human experience and the way in which society perceives the world. Key concepts are cognitive strategies designed to deal with reality. At the same time, concepts convey expectations pointing to desirable or, alternatively, dreaded futures. In other words, concepts are relevant for making sense of both the past and the future. They are embedded in narratives, ideologies, worldviews, and in what Sheila Jasanoff calls "sociotechnical imaginaries" (Jasanoff and Kim 2015). It is a common understanding in conceptual history that concepts have their own time structure and therefore tend to be very persistent—an assumption that resonates with other theories on cultural change, such as William Ogburn's (1964) notion of "cultural lag." The challenge of conceptual history hence is to detect gradual shifts in meanings, especially since old and new semantic dimensions coexist. A shifting societal dictionary—be it the emergence of neologisms or changes in attributions—indicates historical upheaval. Following Koselleck (2004: 263–275), the real "driver" of history is the asymmetry of experience and expectation. Ideologies, in particular, are

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supposed to compensate semantically for a lack of convergence between expectations and experiences (Koselleck 2006: 85).

## Europe and Beyond: Science Policy in Transnational and Diachronic Perspective

This book is based on a comparative design that puts national science policies in a transnational context. At its core, it deals with a European semantic tradition that influences and finally gets itself transformed by interactions with the increasingly successful science policy discourse of the United States in the twentieth century. Furthermore, the book traces the genuinely European Marxist semantics of theory and practice—related to, of course, but not identical with the basic/applied distinction—beyond Europe by looking into how these concepts have been adapted in Mao's China. The first part comprises extensive chapters on three leading nations in science and technology: Britain, Germany, and the United States. These chapters pursue a long-term perspective by tracing the genesis and genealogy of categorizations of science back to the nineteenth century, which allow us to identify major shifts in the language of science policy. The second part consists of five case studies covering science policies in Britain and its colonies, West Germany, and, representing the Socialist part of the World, the German Democratic Republic (G.D.R.), Hungary, and the People's Republic of China (P.R.C.), all making visible how science policy discourses developed in different cultural settings after 1945. In the last chapter of the volume, the historical perspective is complemented by an overview of recent conceptual trends in the European Union's (EU) research and innovation policy, exemplifying how the basic versus applied distinction has been replaced by a pluralization of concepts that have yet to prove their structural potency.

In several classic studies, "pure science" and "basic research" have been considered concepts specifically linked to the self-image of Western democracies (Daniels 1967; Greenberg 1967; Tobey 1971; Mulkay 1976). A comparative perspective, in contrast, reveals that they have been equally applied in communist (China) and former communist (East Germany, Hungary) countries. Moreover, Germany, with its fascist past and its divided history after 1945, offers an intriguing case for analyzing how a common discursive tradition was adapted in various ideological regimes, and for putting the alleged link between democracy and the ideal of "basic research" to a test.

The contributions in this volume therewith reveal a much more variegated history of science policy in the twentieth century than the one we would expect following the ideological controversy between the East and the West—a controversy that is frequently exemplified, and at the same time simplified,

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by the debate between John Desmond Bernal and Michael Polanyi.[17] The literature has so far failed to problematize the ideological representation of the communist plea for "applied research" or the preference of "practice" over "theory" in the service of the technological advancement of Eastern societies and the parallel Western glorification of "basic research" as an essential part of democratic freedom.

The United States and the United Kingdom remain important cases given their global influence and pioneering roles in establishing science policy as a national endeavor in the course of the twentieth century. In fact, most of the mainstream literature on science policy focuses on the American context (e.g., Jasanoff 1990; Sarewitz 1996; Stokes 1997; Guston 2000), and most historical studies on the semantics of "pure versus applied science" and "basic versus applied research" deal either with the United States or with Great Britain.[18] While this strand of literature so far consists mostly of unconnected context-bound case studies, our comparative collection brings us closer to the goal of getting a full picture of how the basic versus applied distinction has developed in these two key countries.

The country case studies presented in this volume do not stand for themselves, but must be read as various strands of a transnational development. International exchanges and cooperations on both individual and institutional levels have characterized modern science just like personal competitions and national rivalry. This also applies, on a more general level, to modern STI policy: science nations have kept a close eye on each other. Although our collection of national case studies is not able to implement approaches of transnational history as consequently as its representatives have envisioned it (e.g., Mintz 1985; Conrad and Randeria 2002), the following synopsis intends to discuss the main findings not only in a comparative but also in a transnational perspective. The comparison of country case studies brings out the similarities as well as the national differences in science policy, which we will trace back to political settings, traditions, and the contingent history of events. The transnational perspective highlights the interdependency of nations and cultures and at least some of the processes of transfer, exchange, and mutual observations. In the Cold War period, for instance, it was the competition between East and West, on the one hand, and the asymmetrical relation between the leading nations and the other allies within the respective ideological camps, on the other, that stimulated the traveling of concepts, the adaption of role models, the production of catching-up narratives, and cultural or ideological boundary work. In this regard, transnational organizations such as the OECD acted as transmitters of conceptual transfer and as stabilizers of whole semantic fields (Godin 2005). This transnational scope thus opens up the discussion about what we call *conceptual synchronization* in science policy, in the

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course of which basic and applied research became the predominant concepts of the language of science policy around the globe.

Yet, in spite of conceptual synchronization and traveling concepts, there are limits of literal translation that point to persistent cultural differences. A well-known example that is pertinent in the context of the present volume is the difference between the German term "Wissenschaft," which denotes scholarly activities across all disciplines, and the English notion of "science," whose meaning was narrowed down to the natural sciences in the course of the nineteenth century (Phillips 2015). Another relevant example is the difference between "fundamental research" and "basic research," which most other languages cannot express simply because they lack the necessary linguistic nuance.[19] All in all, a comparative perspective is a big challenge for conceptual history because even small conceptual variations may indicate essential structural differences (Steinmetz 2016: 353–357). Given the Eurocentrism of the academic language, this applies especially to global comparisons. Finally, the limits of literal translation imply even more challenges for scholars as soon as the analytical language is not identical with the one of the research object—this is what Jörn Leonhard (2016: 153) calls a "translational circle." To avoid misunderstandings, all contributions to this volume refer not only to the English terms of key concepts, but as well to the original terms in the respective languages of the countries they study.

## Main Findings: The Diachronic Perspective

Using a diachronic perspective, the first three chapters trace the semantic field relevant to the modern politics of science back into the nineteenth century. For quite some time, "pure and applied science" seem to have been well-established concepts for communicating science and technology to the public and to the government. The entangled histories of Britain (chapter 1), Germany (chapter 2), and the United States (chapter 3) demonstrate, however, that the uses and the meanings of these terms differ over time. In Germany, where the natural scientists entered the universities comparatively early, the pure versus applied distinction represented hierarchical ways of knowing and a respective order of disciplines since the late eighteenth century. The natural scientists embraced the ideal of pure science as professional marker, which had an integrative function within academia—this is what we call *identity work*—and, at the same time, a distinguishing one with respect to the upcoming engineering or, rather, applied sciences—a process usually described as *boundary work*. In the United States, "pure science" became idealized in the late nineteenth century. Whereas the German devotion to pure science turned into a professional strategy aligned with a hierarchical system of higher ed-

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ucation, the American notion of pure science bore a quasi-religious meaning and was intimately connected with a critique of commercialization. Still different was the semantic trajectory in Britain, where scientists used terms such as "abstract" and "applied science" primarily to categorize different branches of the sciences. Although, in the 1850s and 1860s, the distinction between "pure" and "applied science" gained importance in the debates on higher education and therewith the positioning of the natural sciences in the universities, the term "applied science" had less pejorative connotations and thus worked as a useful promotion strategy for science by demonstrating its societal utility.

The early twentieth century witnesses the emergence of a totally new set of concepts, namely "fundamental," "industrial," "basic," and "applied research." It was the first time that the supercategory of "science" got challenged by various notions of "research." The shift corresponded with changing national research landscapes, particularly with the advent of industrial research laboratories and new types of institutions exclusively devoted to research. The new concepts of research were no epiphenomenon of this structural change; they were rather needed to renegotiate organizational arrangements and, above all, the means and ends of science in the twentieth century.

Against this background, this book challenges the common assumption that the concept of "basic research" is heir to the long tradition of "pure science" or to ideologies that glorify the scientific quest for truth. Rather, new terms such as "fundamental research" transmitted the message that science is useful, despite the fact that its outcome is not predictable. In the United States, the plea for fundamental research was driven by the critique that "pure science," conducted mainly at universities, was not able to meet the increasing demand for scientific knowledge (chapter 3). Against the background of these findings, it is not surprising that the German version of the concept of fundamental research gained importance in the Nazi period: *Grundlagenforschung* (basic research),[20] and the new term *Zweckforschung* (goal-oriented research) served as funding rationales that released research projects relevant to armament and economic autarky from the strict directives of the Nazi's four-year plan (chapter 2).

In the long run, and particularly in the United States and Great Britain, the semantic shift from "science" to "research" was accompanied by a structural shift from "science policy" to "research policy," in the course of which "research" became an expanding, multifaceted, and mundane umbrella term for heterogeneous research activities beyond the academic sphere. While science policy in the nineteenth century mainly dealt with higher education and university policy, research policy in the twentieth century widened its scope to include a much wider variety of institutions performing research. This was,

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however, not a linear development. In West Germany, for instance, science policy debates revolved very much around issues of higher education because *Wissenschaft,* and thereby the university, was intended to play an important role in the pursuit of democratization after 1945. Although U.S. science policy advisors put much effort into propagating programs for basic research in postwar West Germany, the relevance of this concept remained restricted to economic discourse until the 1960s. Moreover, since Germans clung to a cross-disciplinary notion of *Wissenschaft,* science policy (*Wissenschaftspolitik*) still covered a wide range of research activities, including the humanities, whereas postwar debates on research policy in the United States primarily focused on projects in the natural and technical sciences as well as in medicine (chapters 2 and 3).

In the mid-twentieth century, roughly in the decade after World War II, the term "basic research" became universally employed across borders. This semantic shift is analyzed in detail in several chapters of this book. The specific historic situation—the change from wartime to peacetime and the upcoming tensions of the Cold War—led to a revival of ideas traditionally related to the nineteenth-century ideal of pure science. These aspects are often ascribed to the ideas developed in Vannevar Bush's report, *Science, the Endless Frontier* (1945). A closer look, however, reveals how Bush's vision and the succeeding debates in U.S. science policy transformed older conceptions of science and thus considerably differ from the nineteenth-century ideal of pure science. Although U.S. experts also referred to the nineteenth-century humanistic notion of science, they aligned governmental research policy primarily with goals of national prosperity and security. Scientists, however, feared that an excessive demand for scientific applications could endanger the production of new scientific knowledge in the long run. In contrast to the concept of pure science, the advantage of basic research lay in the metaphorical use of "basic," which promised to lay the cornerstone for both the advancement of science and all kinds of societal benefits (chapter 3).

The case of U.S. science policy between the 1940s and 1960s, as analyzed by Greenberg (1967) and others, illustrates how the distinction between basic and applied research was deeply embedded in a broader debate on fundamental political values and the organization of the polity. The same goes for the situation in Europe. The exact nature of this political dimension, however, varies according to national contexts. In postwar West Germany, scientists and research organizations such as the Max Planck Society had to reestablish themselves in a radically transformed political environment (chapter 5). Here, a long tradition of basic research was invented to create an image of science that is independent and detached from worldly and political goals. This was possible notwithstanding the fact that basic research was also con-

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ducted in the Nazi period, and was actually very close to application: the institutes of the Kaiser Wilhelm Society—the predecessor of the Max Planck Society, with a focus on the natural sciences—were highly esteemed for their application-oriented research and collaboration with industry and the military. The point here is that the concept of basic research was fuzzy enough to secure the high scientific prestige of the research institutes of the Max Planck Society beyond the fascist era.

Still another political meaning is contained in the term "fundamental research" as used in British development policy in the 1940s (chapter 4). While this concept was not free from some connotations of "pure science," it primarily pointed to a specific spatial arrangement: "fundamental research" meant research into widespread issues as opposed to local research, which was about narrowly focused inquiry. When the British Colonial Office intended to modernize research institutions in the colonies, the challenge was to enhance their scientific reputation. The launch of a program for fundamental research in the colonies was thus based on the distinction between universal, internationally recognized knowledge, and more practical and technical forms of locally relevant knowledge. The objectives of this boundary work were, first, to attract renowned scientists from the United Kingdom, which should secure metropolitan supervision of colonial research, and, second, to make fundamental research a symbol for modernization of the British colonies.

The diachronic argument of this book is brought to the present time in an outlook that traces recent trends and new entries in the lexicon of science policy (final chapter). Concepts such as "frontier research" and "grand challenges" are interpreted as attempts to establish narratives and rationales for the politics of science in the twenty-first century. Despite the different metaphors and historical references, the protagonists of these semantic variations share the intention of avoiding old-fashioned dichotomies. Thus, these new concepts cannot be negated, as they seem to be purely positive and inclusive. However, it is not very likely that one of these new concepts will actually become a science policy master narrative comparable to what the linear model has been for the twentieth century. Rather, we observe a pluralization of science policy discourses, and it remains unclear whether in the long run this plurality will narrow again to one or two key concepts.

## Main Findings: The Transnational Perspective

In addition to the diachronic view, the transnational perspective of this book sheds light on exchanges and interdependencies in the global field of science policy. Our focus here are the decades after World War II. In this period, we observe not only *conceptual synchronization* but also *cultural variation* with

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regard to how the concepts were used and filled with meaning. This is particularly evident in the three case studies on science policy in communist countries—the German Democratic Republic (chapter 6), Hungary (chapter 7), and China (chapter 8). In the 1940s and 1950s, all of these countries reorganized their research infrastructure and brought science policy in line with communist ideology.

The transnational logic of competing science policy regimes is, however, not restricted to the Cold War era. In the course of the twentieth century, science and technology had become increasingly important indicators for where a nation stood in the global order, and those nations that felt left behind were trying to catch up with the others. These dynamic processes did not start with the Cold War, but can be observed already in the nineteenth century. When Britain had to forfeit its leading role in the chemical industry, the motive of catching up with Germany was crucial in the decision to install the new Department of Scientific and Industrial Research (chapter 1). In the United States, the narrative that America lags far behind Europe concerning the abstract and theoretical sciences was reiterated again and again between the 1840s and the 1920s. The image of the United States as a nation that preferred applied over pure science and was thus dependent on European knowledge production initiated many debates about the development of American science (chapter 3).

In other words, global competition was not invented in the Cold War, but it became more visible and dramatic. The arms race and ideological rivalry implicated a harsh competition for the superior system of science and technology. Milestones such as the launch of Sputnik, the ability to create an atomic bomb, or, more generally, the number of inventions and overall economic development became important stakes in this situation of mutual observation. In the People's Republic of China, it was the successful atomic bomb test in 1964 that marked a turning point in Chinese science policy (chapter 8). As a result, Chinese science advisors declared that the phase of imitation was over. To enter the next stage—that is, to aim at catching up and finally surpassing the Western nations—they could argue for the relevance of basic research; herein lay, at least for a small group of natural scientists, a chance for a circumscribed independence. Before, as is illustrated also by the case studies on the G.D.R. (chapter 6) and Hungary (chapter 7), the relation between "theory and practice" had to be strictly framed in accordance with Marxism and the Communist Party's politics.

All the same, we also find evidence for a reframing of science policy in terms of basic and applied research in the Eastern part of the world. The process of international conceptual synchronization, however, depended on political contingencies. Hungarian economists, for example, could produce

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neither basic nor applied research before the death of Stalin in 1953; before, they were instructed to rely exclusively on studies from the Soviet Union, and they were not allowed access to economic data, which were controlled by the party (chapter 7). In China, the radical politicization of science was tempered after the death of Mao in 1976 (chapter 8). Of course, Marxist scientists and policymakers were trying to develop their own conceptual frameworks. John D. Bernal and Gerhard Kosel, for instance, developed an alternative to the Western paradigm of the linear model, labeled as the "scientific-technological revolution." The G.D.R. clung to this model until the 1970s (chapter 6).

Overall, the vocabularies of science policy were framed by more general sociopolitical concepts both in the East and in the West: in communist countries, key terms were "revolution" or "planning," whereas leitmotifs in the West were "progress" and "modernization." Despite the ideological antagonism, there were common denominators in science policy discourses: first, the strategy of categorizing various research activities according to a dichotomous rationale, and second, the narrative that science is aligned with technological advances. Furthermore, the notion of planning was not exclusive to socialist governments, but became increasingly important in many Western countries in the 1970s (chapters 2 and 5).

A special case to examine conceptual synchronization of science policy with international discourses is the constellation of East and West Germany (chapters 2, 5, and 6). In the 1970s, the socialist government of the G.D.R. aimed to reform science policy after some big science projects could not meet the expectations and invited criticism both from industry and scientists. Party officials concluded that new knowledge, namely basic research, was needed. To adjust the alien nomenclature to socialist ideology, they started to interpret the basic/applied relation dialectically, invented a bureaucratic way of subdividing the process from basic research to development in nineteen stages, and created conceptual hybrids such as "targeted basic research"—which were, in fact, quite similar to the Western ones. The ironic result was that the G.D.R. introduced an extreme version of the linear model of innovation just at the time when it began to lose its steam in the West.

In the Federal Republic, the process of synchronization with the U.S. nomenclature started earlier and was connected to the efforts for a successful integration into the Western alliance. A full adaption, however, turned out to be difficult. Although "basic research" was already in use, it competed with other concepts, such as pure science or independent research, which were in turn related to discourses about the democratization of postfascist Germany. Moreover, the distinction between basic and applied research was hotly debated in the 1950s and 1960s, namely for two reasons: first, the German Research Foundation stressed its responsibility for all branches of *Wissen-*

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*schaft,* including the humanities and applied sciences. They avoided the concept of "basic research" because it seemed too strongly associated with the natural sciences. It was not until the 1970s that the concept of basic research became equally common in the humanities. Second, science policy experts complained that, compared to the United States, the Federal Republic lacked government institutions that could take on applied research. In the end, it was not until the 1970s that the distinction spread beyond the natural sciences and became the main funding rationale.

The competition between science policy regimes did not fade away with the end of the Cold War. An outlook into twenty-first-century science policy reveals that the United States is still a crucial reference point for STI polices around the world. The final chapter of this volume focuses on the European Union to get a picture of how transnational science policy is in search of a new language. In this context, it is conspicuous that European science policy is strongly aligned to the goal of establishing Europe as a key player in the global competition, while at the same time building not on its own tradition but on genuinely American narratives. This ambivalence of EU research policy is best illustrated by the European Research Council's (ERC) introduction of "frontier research" as a new term substituting outdated notions of "basic research." The frontier metaphor is deeply rooted in the history of the United States and thus is a good example of how concepts travel across time and across borders. The same goes for the concept of "grand challenges," the origins of which can be traced back to debates about supporting the computational sciences in the United States in the 1980s against the background of the "Japanese challenge." The European Commission adopted this concept and transformed it into the idea that the role of science is to "tackle societal challenges."

## From Epistemology to the Politics of Science

Finally, our book demonstrates that the distinction between basic and applied research is not primarily related to epistemic properties of different types of research. Rather, it deals with a variety of essentially political problems. Many dimensions of the politics of science have been touched on above: the funding and (re)organization of national research activities, the political symbolism of key concepts and metaphors such as "the frontier," issues of higher education, the long-lasting function of professional identity work of scientists and research institutions, global competition in technological developments, and national security issues. These were not abstract problems, but rather, especially in the Cold War period, issues with high relevance for individuals, scientific communities, and science in general. In the heyday of the McCarthy

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era, U.S. scientists were exposed to political attacks when they took a critical stance on the ideal of basic research. To some degree, they were thus forced to opportunistic politicking (chapter 3). The Hungarian and Chinese cases demonstrate that the question of how experts positioned themselves toward issues of science policy turned into a question of loyalty and sometimes even into a question of life or death (chapters 7 and 8).

Related to, but not identical with, the political problems are questions concerning ethical dilemmas and responsibilities, which arose from the sometimes destructive effects of science and technology. Especially in the aftermath of the two world wars, most urgently after the atomic bombs were dropped on Hiroshima and Nagasaki, the concepts of applied and pure science unfolded critical debates on the question of how to deal with failures and risks related to science and technology (chapters 1 and 2). With the beginning of the twenty-first century, there seems to be a shift in this longstanding debate. After a period of bureaucratic solutions, such as technology assessment and risk analysis, in the 1980s and 1990s, this aspect is now being framed as a question of democratization of science policy. The very recent concept of "responsible research and innovation" is a popular example, which tries to redefine such issues (Rip 2016; Ribeiro, Smith, and Millar 2017; Flink and Kaldewey 2018). However, even this bureaucratic construct conveys in its very name a semantic pair—"research" and "innovation"—that reminds us that the distinction between basic and applied research is still part of our conceptual vocabulary.

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of science and politics, particularly with the contemporary pluralization of science policy discourses and how they transform the identity work of scholars, scientists, and policy makers.

## Notes

1. It is telling that Dupree was digging for the roots of U.S. federal policy just at the time when science policy became highly significant for the U.S. government and the new policy field needed to be legitimized.
2. The early modern distinction between theory and practice, in turn, is related to several semantic traditions from antiquity to the Renaissance. Most important here is the influential tradition of debating the relative values of "vita activa" and "vita contemplativa" (Kaldewey 2013: 191–247).
3. This discussion is well documented in editorials and comments in *Science* (Abelson 1964, 1965, 1966a, 1967; Klopsteg 1965; Greenberg 1966; Reagan 1967). The debate heated up when the Department of Defense published the "Project Hindsight" study, questioning the utility of basic research (Abelson 1966b; Sherwin and Isenson 1967). This study was later countered by the TRACES study, which in contrast argued for the continuing relevance of basic research for technological development (Thompson 1969). The Project Hindsight vs. TRACES debate has often been referred to by science studies scholars (e.g., Kreilkamp 1971; Stokes 1997; Asner 2004; Hounshell 2004; Elzinga 2012).
4. See, for example, the critique as formulated in a variety of studies representing quite different factions in the field of science studies: Latour 1993; Gibbons et al. 1994; Oreskes 2003; and Douglas 2014.
5. For instance, a random analysis of more recent articles on science and technology in the *Süddeutsche Zeitung,* a widespread and influential German newspaper, supports the assumption that basic and applied research are still the dominant concepts in the public discourse: "Leid im Labor," 10 August 2016; "Saatgut für die Welt," 24 September 2016; "Der Staat finanziert, die Wirtschaft kassiert," 22 October 2016; "Revolution in der Zelle," 10 December 2016; "Künstlich intelligent," 1 January 2017; "Intelligenztest für Autos," 14 January 2017.
6. See the self-description on the website of the Ministry of Science and Technology of the People's Republic of China, retrieved 29 May 2016 from http://www.most.gov.cn/eng/programmes1/200610/t20061009_36223.htm.
7. See the self-description of the Institute for Basic Science, retrieved 27 July 2014 from https://www.ibs.re.kr/eng/sub01_01_01.do.
8. Exceptions are studies by linguists or scholars of communication studies situated at the margin of the STS community (e.g., Harris 2005; Perren and Jennings 2005; Ceccarelli 2013; Perren and Sapsed 2013).
9. The "practice turn" is a label introduced by Schatzki, Knorr Cetina, and Savigny (2001) to summarize recent developments in contemporary theory. In retrospect, several studies that have been influential in the field of science studies can be sub-

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sumed under this label (Latour and Woolgar 1979; Knorr Cetina 1981; Latour 1987; Pickering 1992).

10. For a more elaborated discussion of concepts that promise to yield important insights into the role of language in the politics of science and technology, see Schauz 2014b, Flink and Kaldewey 2018, and the website of the CASTI Research Network, http://www.casti.org/.
11. This introduction cannot give an overview that covers all the diverse approaches dealing with concepts, symbols, or metaphors. Moreover, it must be noted that scholars in history and philosophy of science and science studies have already developed a particular interest in metaphors, their epistemic role in knowledge production, and their symbolic functions for public debates on certain research fields, especially biology and genetic research (e.g., Blumenberg [1960] 2010; Keller 1995; Maasen, Mendelsohn, and Weingart 1995; Maasen and Weingart 2000; Brandt 2004; Vaccari 2008; Borck 2013; Surman, Straner, and Haslinger 2014). So far, however, this strand of literature has not included metascientific concepts.
12. See Lucier 2009, 2012; Clarke 2010; Bud 2012, 2013, 2014; Gooday 2012; Pielke 2012; Kaldewey 2013; Schauz 2014a, 2015; Lax 2015; Roll-Hansen 2017; Sapir 2017. Several of these authors have continued their work and are represented in this book (chapters 1, 2, 3, and 4) .
13. This kind of substitution is, for example, seen as unproblematic by Calvert (2004: 254), who traces different meanings of "basic research" in language and practice, but ignores the historical dimension of these meanings.
14. The discussion about how to reconcile conceptual history and discourse analysis, however, is restricted so far to German authors (Busse 1987, 2016; Steinmetz 1993: 30–44; Reichardt 2000; Bödeker 2002; Wiehl 2003; Konersmann 2005; Kaldewey 2013). For relevant approaches focusing on narratives and imaginaries, see Castoriadis (1975) 1997; Anderson (1983) 2006; Bal 2009; Koschorke 2012; and Jasanoff and Kim 2015.
15. See, to list just some of the most cited texts, Funtowicz and Ravetz 1993; Gibbons et al. 1994; Slaughter and Leslie 1997; Etzkowitz and Leydesdorff 2000; Fuller 2000; Ziman 2000; Nowotny, Scott, and Gibbons 2001; Slaughter and Rhoades 2004. Extensive reviews of this genre of literature have been provided by Godin 1998; Shinn 2002; Elzinga 2004; Hessels and van Lente 2008; and Schiemann 2011.
16. See Lundvall 1992; Nelson 1993; Rosenberg 1994; Freeman 1995, 1996; and Mokyr 2002. More recently, scholars aim to demonstrate that we deal here with an emerging scientific field (Fagerberg 2005; Fagerberg and Verspagen 2009; Fagerberg, Martin and Andersen 2013). For a critical perspective, see Godin 2009, 2012.
17. The most cited publications associated with this debate are Bernal 1939 and Polanyi 1940, 1962. See Douglas 2014 for a short report on this debate related to the problem of how to define "pure science." For the economic underpinnings of Polanyis's understanding of pure, independent science and the intellectual background of the British debates, see Nye 2011: 145.

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18. For the United States, see Daniels 1967; Hounshell 1980; Kline 1995; Lucier 2009, 2012; and Pielke 2012. For Britain, see Gieryn 1983, 1999; Bud and Roberts 1984; Donnelly 1986; Clarke 2010; Bud 2012, 2013, 2014; and Gooday 2012.
19. In the early twentieth century, "fundamental research" emerged in the context of industrial research and applied sciences, both in the United States and in Great Britain. Although the meanings of fundamental and basic research were overlapping in many respects at first, the two concepts grew apart after 1945, when basic research was transformed into a concept that primarily referred to academic research in the natural sciences (chapter 3).
20. While the English terms "fundamental research" and "basic research" often bear different meanings (chapters 3 and 4), the German term *Grundlagenforschung* does not allow for such differentiation.

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# Part I

# Genealogies of Science Policy Discourses

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Chapter 1

# Categorizing Science in Nineteenth and Early Twentieth-Century Britain

*Robert Bud*

Britain had no policy for science during the nineteenth and early twentieth centuries. Science was, however, called upon to address fears, problems, and incomplete narratives shared widely by government and people. Its study in schools and universities was a matter of perennial anxiety. Though the annual government grants given for research in the mid-nineteenth century were small, projects related to pressing government concerns received substantial support (Macleod 1971a, b). In the 1870s, the state funded the four-year round-the-world expedition by the naval vessel the *Challenger,* costing £100,000 ($500,000 or two million Reich Marks—hereafter RM). The mission to map and explore the deep sea and seabed was sustained both by the practical needs of laying an imperial telegraph network and by the scientific controversy over life and natural selection in such environments. The expedition resulted in fifty volumes of scientific reports, which set the stage for oceanography in the future (Burstyn 1972).

As the *Challenger* was finally returning to Britain in May 1876, London's South Kensington Museum was responding to a call for a boost to scientific culture in industry and education by opening an exhibition of "scientific apparatus" that had cost £15,000 ($75,000 or RM 300,000) to mount (Committee of Public Accounts 1878: ix; de Clerq 2003; Bud 2014b). Ranging from the Stephenson's pioneering Rocket locomotive to the Magdeburg hemispheres of von Guericke, this huge exhibition was visited by three hundred thousand people in just six months. The cost, hidden in the budget of the Science and

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Art Department (responsible for technical education), was equivalent to the entire recurrent budget of Berlin's famous Physikalisch-Technische Reichsanstalt, founded a decade later in Charlottenburg (Pfetsch 1970: 575). Such major investments drew upon both the enthusiasm of a few influential intellectuals and civil servants and the wider engagement of military men, journalists, businessmen, and politicians. We need, therefore, to take seriously their commitment to the projects themselves, to the categories that made them possible—including, for instance, the inclusion of both "pure science" and "applied science" in the South Kensington exhibition—and to the concerns that underpinned them. Often seen as either so natural, or, alternatively, so misguided, as not to deserve historical attention, the use of these categories will be the concern of this chapter.

The classification of science into the categories of "applied" and "pure" (and closely related terms, such as "fundamental") became conventional from the late nineteenth century, and remained so for a hundred years. In his classic study, *The Organisation of Science in England,* D. S. L. Cardwell (1957: 1) suggested that it was "applied science" for which policy was made in the period of his interest from the mid-nineteenth century to the beginning of World War I. When he was writing, now six decades ago, Cardwell's priority was to look at policies, and less attention was paid to "pure" and "applied" science as concepts in themselves. In part, that was because these were still widely treated as realities, needing only precise definition. Furthermore, the context in which they were invoked was often the disconcertingly vague, and at that time often hard to recover, discourses between scientists and the general public, and the political rhetoric of competition and crisis.

Today, categories are no longer taken for granted, and historians have been enabled to deal with the public sphere more substantially by the availability of digitized newspapers. These are particularly interesting because the politically-aligned newspaper of the nineteenth century was closely interdependent with the political platform; speeches were widely reproduced verbatim in the national press and copied regionally; the texts of books and their reviews and advertisements were similarly already intermingled by canny publishers. Effectively, therefore, the discourses of news, marketing, and politics came to be interfused on the newspaper page.

The history of such concepts as "pure" and "applied" science is not just the history of specific phrases in the public sphere, but also of terms whose multiple meanings were reconfigured by new means of mass communication and discussion and of political debate.[1] I shall argue, first, that talk about science and its categories did work that went beyond the conventional categories of epistemology. Second, in the public sphere, "applied science" came to be the key category, not dependent on "pure science." Third, it was in the

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contexts particularly of educational debate that these categories first became significant.

The chapter begins with a focus on the development of language and the work it was expected to do. It deals with the nineteenth-century emergence of the concept of applied science to describe a form of knowledge particularly with an educational significance, and with the cognate terms "pure science" and "technology," whose meanings were also shaped in educational contexts. It then treats two periods: first, the two decades spanning the end of the nineteenth century and the first years of the twentieth century, and, second, the years from World War I to the end of World War II. These periods differed not only in the issues that dominated their concerns but also in the technology of media spaces: the earlier period was limited to the printed word; in the later period, the wireless became important. They shared characteristics too—above all, perhaps, the exposure to overseas influence. During each of these periods, interaction with Germany, France, and the United States was profoundly important. The shared use of the English language meant a close interpenetration of British and American discourses. Many British scientists, and indeed other professionals, had studied in Germany, and knowledge of French was a requirement for the educated person. So, Britain acted as a key node in the international circulation of concepts about the structure of science.

## The Nineteenth Century: Emergence of Terminology and the Educational Context

The familiar terminology of pure and applied science cannot simply be read back in time, or even straightforwardly translated into an older classification system. We can see the tortured difficulties in producing a 1783 English edition of Torbern Bergman's *Essay on the Usefulness of Chemistry.* This was translated from Swedish into poor English ("lingua Franca") by the Austrian Franz Swediauer, and then rendered into good English by the renowned philosopher Jeremy Bentham. The result was that the German term "philosophische Chemie" came to be rendered as "pure chemistry" (rather than "philosophical chemistry," which might also have been acceptable). Perhaps surprisingly, the term "angewandte Chemie" became not "applied chemistry" but "(*chemia applicata*), mixt, particular or popular chemistry," which Bentham subsequently referred to simply as "popular chemistry" (Bergman 1779: 10, 12; 1783: 16, 20; Linder and Smeaton 1968). To Bentham therefore, writing in 1783, "applied chemistry" was not an option.[2]

Instead of simply inheriting old categories, the nineteenth century saw a reconstruction. The concept of "applied science" that emerged in the second

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half of the nineteenth century was a hybrid of three ancestors: "applied sciences" (quickly corrupted to applied science), "science applied to the arts," and "practical science." Let us look at each of these.

It was the Germanophile poet, polymath, and religious conservative Samuel Taylor Coleridge who introduced the term "applied sciences" to the English language at the beginning of the nineteenth century. In 1817, a time of social unrest and economic depression in the wake of the Napoleonic Wars, Coleridge sought to stabilize thought through an encyclopedia that would provide a rational structure for all knowledge in opposition to the alphabetically organized, and thus epistemologically haphazard, *Chambers Encyclopaedia* (Link 1948). In his proposal for the *Encyclopaedia Metropolitana,* Coleridge proposed a classification based on absolute truth value. This drew upon the differentiation of pure sciences and applied sciences developed in Germany and inspired by the work of Immanuel Kant. The first category, science based on the synthetic a priori, was necessarily and immutably "true" and therefore labeled as "pure" (see chapter 2). The second category, "mixed and applied sciences," more dependent also on empirical evidence, was necessarily contingent.[3] Here Coleridge drew upon the German term "angewandte Wissenschaft," which had been derived from Kant's "angewandte Vernunfterkenntnis" (Kant 1786: vi; Bud 2013a). As for Kant, the knowledge of this kind was less certain than its "pure" partner. Coleridge's structured encyclopedia, a form new to Britain, drew on a German tradition of Kantian compilers of knowledge, as exemplified for instance by Jena-based Carl Schmid, author of the 1810 *Allgemeine Encylopädie und Methodologie der Wissenschaften* ("Encyklopädie" 1895: 755; Snyder 1934: xxii). Although Coleridge argued with the publisher and eventually withdrew from the project, the encyclopedia nonetheless went ahead. It was completed over a period of twenty years and in twenty-eight volumes, thanks to a succession of three editors who shared both Coleridge's religious and philosophical position. A section of more than five thousand pages on "Mixed and Applied Sciences" subsumed subjects ranging from the fine and useful arts to medical sciences.

The articles in the encyclopedia were no-snippet length sound bites. Many were the length of entire books and were published separately. Charles Babbage's 1832 book *On the Economy of Machinery and Manufactures* was based on an entry he had contributed. This volume is itself credited by the Oxford English Dictionary as the first source of the phrase "applied science." The attribution of priority is incorrect, but it does highlight the influence of the work that Coleridge had instigated. The names of its structuring concepts were promoted through the advertisements for the work as a whole and the influence of its essays. A search of the numerous digitized newspapers of the nineteenth century makes it clear that the term "applied sciences" first

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appeared in the press during the 1820s (and until 1836 exclusively) within advertisements for the encyclopedia. And even in these advertisements, the plural form was sometimes corrupted to the singular “applied science.”

A complementary origin lay in the phrase “science applied to the arts,” which is an import from the French “science appliquée aux arts.” This term grew out of a formulation systematized by the Conservatoire National des Arts et Métiers, an institution in Paris that was redefined in 1819 as dedicated to teaching “l’application des sciences aux arts industriels.” In 1848, for example, the professors were charged with formulating a “système général pour l’enseignement des sciences appliquées aux arts industriels” (Marchand 2005). The author of this term seems to have been Charles Dupin, previously trained at the fabled Ecole Polytechnique. He had been influenced by the elite thinking during his youth, the practical achievements of French savants under the pressure of the revolutionary wars, and then by his explanation for the staggering British industrial achievements during the time the two countries had been at war (Dupin 1825; Alder 1997; Bret 2002; Christen and Vatin 2009; Bud 2014a).

Finally, there was also a long-established English term, “practical science,” that often connoted empirical science, related to—but not derivative from—so-called “abstract science.” In his studies on the history of brewing, James Sumner (2015) has highlighted the tensions between experts with a claim to “practical” expertise and those whose knowledge was more abstract or theoretical. The term had been used occasionally in the eighteenth century, but its use grew in the nineteenth, only giving way to “applied science” in the 1860s. Thus, in 1865, the University of Cambridge introduced a new course whose designation had changed from practical science in preliminary discussions to applied science in the published announcement.[4] These terms with rather different origins came to be used interchangeably, as they were hybridized to be useful in a new world.

Notwithstanding the efforts of Coleridge and his successors, the first occasion for the widespread journalistic use of the term “applied science” was coverage of the activities of the well-known and self-promoting chemist, J. F. W. Johnston and the campaign to cope with the elimination of tariffs on imported wheat. Applied science was proposed, for the first (but not the last) time as a substitute for “protection.” In the early 1840s, Johnston, professor at the new University of Durham, purveyor of soil analyses, and founder of the British Association, having originally espoused the term “science applied to the arts,” changed his linguistic allegiance. He argued that the model of “applied science” could be seen in the coherent body of techniques of navigation based on astronomy that had permitted sailors to sail the oceans. Across the country, the press, including the *Economist,* which led the campaign against

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tariff protection, endorsed Johnston's views that British agriculture could be saved through the careful assembly of techniques of soil analysis and "applied science" (Bud 2014a).

In the aftermath of the Great Exhibition of 1851, usages of "applied science" came to be even more common, but increasingly they were redirected to promoting the needs of educational innovation. While there were no clear definitions, there were illustrative stories that served as illuminating metaphors in making sense of the concept. The "romances" of the great inventors, James Watt and Robert Stephenson, both heroes of the nation as a whole and of specific regions, were incorporated within allegorical narratives about the nation's rise to greatness and the needs of its renewal. In 1867, the chemist George Gore was allowed two columns to lecture readers of the *Birmingham Post* on the importance of applied science to local industries, one quarter of which was devoted to the scientific heritage on which James Watt had drawn. In the region of North East England, centered on Newcastle-on-Tyne, Robert Stephenson, son of George and himself father of the railway system, was the scientific hero. The lives of both Watt and Stephenson could be read in terms of their arduous pursuit of a scientific education leading to brilliant achievements. These narratives communicated the truly local and national ancestry of applied science, belied any assertion it was an alien concept, and laid out a path for Britain's educational development in the future (Bud 2014a).

Just as Watt was destined, however unjustly, to be a hero of applied science, so Michael Faraday, perhaps equally unjustly, became the hero of that category's destined counterpart, "pure science." This, too, was a hybrid of native, imported, and pragmatic conceptions. There was a rich tradition of the practice of science for the sake of curiosity, sanctified by religion and validated by its interest and results. Often called "abstract science," it was, however, not sustained by a long domestic philosophical tradition. It is true that the term "pure science" is deeply rooted in the English language and appears in the writings of Samuel Johnson (1750) in the eighteenth century, but it was historically rarely used. It was to German sources that Coleridge had turned when he formulated his classification. Studying in Göttingen in the 1790s, Coleridge had been a pioneer in the English journey to German universities, which in the mid-nineteenth century became, for the British man of science, a rite of passage, and for the intending academic chemist, a professional necessity. From Germany, in particular, those students brought back cultures of pure science. According to Cantor's (1996: 174) biography *Faraday as a Discoverer*, the physicist John Tyndall—who had himself both studied in Germany and been inspired by another Germanophile, Thomas Carlyle—identified Michael Faraday as "the exemplary *pure scientist*" (italics in the original). Tyndall himself would become an influential and vocal spokesman for pure science.

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The new classification system of different kinds of science did prove useful in laying out priorities for educational development (Bud and Roberts 1984). In 1868, the German-born ironmaster, engineer, and member of Parliament Bernhard Samuelson gave notice that he would move for a parliamentary committee on the "provisions for giving instruction in theoretical and applied science to the industrial classes."[5] Samuelson had chosen his terms with care: as he explained to the House of Commons, he had deliberately eschewed the use of the term "technical education" for fear that he would be blamed for trying to replace the factory education of the engineer. Such practical training was proudly kept sacrosanct in Britain, and Samuelson knew that he should not be seen to threaten it.[6] Instead he emphasized that he was seeking complementary schooling. He had drawn upon a phrase that had become acceptable and indeed fashionable to make a very particular point about the need for practical scientific education preliminary to industrial experience. Following the parliamentary committee, an even more extensive "Royal Commission on Scientific Education" was held, and that again raised popular interest and awareness.

One early outcome of the Samuelson committee was the consolidation of a variety of scientific schools into a new government school of science in the West London site of South Kensington, close to the former home of the Great Exhibition. Because it was government-funded, its curriculum and role would be elaborately planned and carefully scrutinized. Increasingly it was seen to teach a core curriculum of "pure science." Thereafter, the well-prepared student could develop a career in teaching or in industry deploying "applied science." Meanwhile, a remarkable amount of private money was going into developing a new generation of independent colleges both in London and the provinces, where they would be the bases of the new civic universities.

The growth of these colleges and the challenge of overseas developments spurred discussion on the role of study. A liberal education, dedicated to the development of the mind, had traditionally been based on the study of the classical languages and on mathematics. Beyond these means, the end was a training of the mind for social and material benefit (Rothblatt 1976). Now, the sciences could be introduced into the curriculum either as parts of liberal education or as specialized applications of it. Pure science could most easily be seen in terms of liberal education, and indeed be defined by it. This was the argument elaborated in Alexander Williamson's inaugural lecture at University College London under the title "Plea for Pure Science" (1870). He himself was no "absent-minded professor" and was known for his development of a boiler for generating high-pressure steam (Harris and Brock 1974). Yet, speaking in the wake of the Samuelson committee, Williamson had been provoked by the apparently overwhelming public enthusiasm for "applied science," as was the American physicist Henry Rowland (1883) a decade later in

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his own “Plea for Pure Science” (Hounshell 1980). Williamson argued for the special role of the university as a preparation for life. Through subsequent enquiries, pure science, as the professional education of teachers and as the universal basis of education for all technical professions, was confirmed as the principal focus of the curriculum.

Hybrid versions of these arguments would be debated. Thus, in 1873, Tyndall was the first to popularize a quip by his friend Louis Pasteur published two years earlier in obscure publications: “There exists no category of science to which the name applied science could rightfully be given. We have science and the applications of science, which are united together as the tree and its fruit” (Pasteur 1871, quoted in Tyndall 1873: 221).[7] Tyndall would incorporate this quotation in lecture after lecture, for instance again at the Royal Institution in January 1879.[8] A few months later, his close friend Thomas Huxley, a professor in South Kensington of the apparently useless subject of biology, expressed similar sentiments at the opening of the Mason College Birmingham. His famous oft-recited regret of the very coinage of the term “applied science” would itself be influential, and the boundary between pure and applied science proved to be an enduring issue (Gooday 2012). The attack had been on the category of “applied science” because the implication of arguments such as those of Samuelson was that this was the fundamental unit, from which the idea of “pure science” was derived as a residuum.

The variety of work that the very concept “applied science” could be expected to perform has been explored through a study of approximately one thousand articles employing the term recovered from digitized newspapers (Bud 2014a). These show that during the nineteenth century, the term was used in three ways: first, it could connote both machines themselves and knowledge about them; second, it expressed belief in a historical relationship between carefully nurtured scientific knowledge and practices that had already characterized great industrial breakthroughs in Britain and elsewhere; and, third, it expressed the expectation of the future potential of science to assure national and international wealth and prosperity. The relationship between a familiar past and a future made strange, on account both of the novelty of its gadgets and of change in the commercial world, was frequently described in terms of the progress of “applied science.” Today, encountering the new social and political configurations brought about by social media, we can empathize with the need to make sense of the disorientation brought about by the telegraph, mass global travel, and other developments “in which the experience of the past is no longer a reliable guide” for the future (Kidd 1894: 6). The explanation given in this late nineteenth-century text would resonate in the next hundred years: “Since the beginning of the century, applied science has transformed the world” (Kidd 1894: 6).

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From the middle of the century, users of the term "applied science" sought to link a selective rendering of the nation's past to fears about a future perceived as full of risk. European and American competition, both commercial and military, became the foci for national anxiety. In 1868, the educationist Matthew Arnold reported on "schools and universities on the continent." His influential report popularized the complaint that adherence to the "rule of thumb," having once been a British strength, was now a weakness in British instruction compared to continental trust in "science and systematic knowledge" (Arnold 1868: 211–212). Again and again, the deployment of "applied science" was prescribed as the remedy. The sense of threatening decline was widely translated into a problem of education, as other countries invested in forms of technical training unknown in Britain. That scientists of the late Victorian period proclaimed a sense of crisis to win economic and cultural resources was not itself evidence of the quantity or quality of the crisis invoked (Turner 1980). The language of threat and imminent decline was real, but the use of rhetoric needs to be considered separately from commercial, scientific, or economic competition. Historians of science have recognized the importance and complexity of calls for applied science in the public sphere context—in such works as Ronald Kline's study of speeches by engineering leaders who addressed their professional constituencies in these terms (Kline 1995).

We of course need to be cognizant of the vast influence of German, French, and American usages on talk about science in Britain. In 1957, the same year as the publication of D. S. L. Cardwell's *The Organisation of Science in England,* the distinguished American historian George Haines (1957) published his *German Influence on English Education and Science, 1800–1866.*[9] Haines saw the impact of the impressive rise of Germany as one of English "acculturation." Nonetheless, rather than merely expressing German "influence," the rise of the concept of applied science can be seen as a process of actively responding to German educational patterns within British culture. Nor was the relationship one way. Following the examples of the Kantians, in German, the use of the term "angewandte Wissenschaft" tended to be related to the contingent products of man (particularly law) than to more typically Anglophone connotations. However, in 1898, the German mathematician Felix Klein led the formation of a center at the University of Göttingen dealing with applied physics and mathematics, known as the Vereinigung für angewandte Physik und Mathematik (Schubring 1989). At the tenth anniversary celebration, the university's pro-rector defended the foreign origins of the idea and extolled the special role of Göttingen, which according to long tradition had many Anglo-American connections and was in the front line of exchange with the Anglo-American cultural sphere (*englisch-amerikanische Kulturwelt*) (Cramer 1908: 23).

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The other English term that would be promoted together with new policies was "technology." This, too, was an expression of German ancestry (Sebestik 1983) that had been popularized through an early nineteenth-century encyclopedia. The British newspaper archive shows a huge jump in usage from the 1822 publication of Crabbe's *Universal Technological Dictionary.*[10] Again, in the 1850s, usage was eight times greater than in the previous decade. The predominant reason this time was the promotion of the term by the Scottish professor, George Wilson. Adopting the term "technology" from the work of the German chemical writer Knapp, he preferred it to "applied science" and used it to title his university chair at Edinburgh (Wilson 1855; Anderson 1992). This lapsed after his premature death in 1859, and the popularity of the term declined temporarily. It was revived, however, by a fundamentally new development early in the 1870s.

While the new colleges were teaching "applied science" to the middle classes at university level, from the time of the Samuelson committee, the long-established Society of Arts was discussing and then adopting vocational examinations for the working class. These were intended to build demand for an education equivalent to that of German trade schools. In 1873, seeking to emulate German technical education, the society launched a qualification system beginning with "cotton," "paper," "steel," "silk," and "carriages." Building on scientific examinations administered by the government's Science and Art Department, the new tests had two parts; the first evaluated technology, "that is to say the special application to each manufacture of the various branches of science that have to do with it" (Wilmot 1872: 9). A further part related to practical knowledge, and the candidate was expected to produce an attestation of skill from his employer. In 1880, the wealthy City of London Corporation and its long-established City Guilds took over responsibility for these examinations under the title "City and Guilds" and hired Rabbi Philip Magnus, who had previously studied in Berlin, to administer them. For the next fifty years, Magnus would be a doughty promoter of "technology" (Foden 1961, 1970). In 1883, twenty-five hundred students were in attendance, which rose by 1914 to fifty-five thousand students across the country (Foden 1961: xxii). Under the banners of "technical" or "technological," teaching was organized at the municipal level by local education boards with increasing ambition—and massively increasing local and government funding.

## The Saddle Time of Science Policy

At the heart of this chapter lie the transformational decades from 1899 to 1919, in which the agenda shifted from education to research. An "exem-

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plary anecdote" or key story is the loss of the dye industry, which was based on England's William Perkin's discovery of the "first synthetic dye" half a century earlier, to the better organized, research-based German companies.[11] This was told and regretted at a major semicentenary commemoration of the discovery held in 1906 (Travis 2006). The frequent invocation of this story indicated the increasing determination that it not be repeated. Inherited concepts from the nineteenth century were redeployed to address the challenges of the twentieth. This has been widely seen by such historians as Peter Alter (1987) as the key period of development of twentieth-century science policy. Adapting a term from Reinhart Koselleck, this period could also be described as *Sattelzeit* (saddle-time).[12]

In this period, the emphasis switched from training to research, and new associations were drawn with rationalization for and the development of new technology (MacLeod 2007). The term "applied science" expressed therefore the relationship with other states, such as Germany and America, whose military and commercial success correlated with a national research base. In addition, it came to be used to make sense of the relationships between, and responsibilities of, an increasingly large plethora of organizations devoted both to education and research within Britain (Bud 2014a). It was, of course, used occasionally by academics and administrators, but this term was to be found more widely in popular newspapers, cartoons, and novels making sense of modernity. The term was also given meaning within exhibitions and in press talks about exhibitions. Thus the Science Museum in London, whose new building opened in 1928, was described at the time as "housing the products of science as applied to industry" and as "a temple of applied science." The museum context highlights the interrelationship between the telling of heroic and familiar stories and the terms in which they were told (Bud 2013b).

The numerous failures that preceded ultimate British victory in the Boer War (1899–1902) prompted a national movement for enhanced efficiency (Searle 1971). This period was one of rapid change across many sectors. Thus, building on previous discussions, educational provision was institutionally transformed and grew hugely. Secondary education was reorganized and systematized by the Education Act of 1902. At university level, the new civic universities, based in the industrial towns (as opposed to Oxford and Cambridge), prospered and multiplied. In just fourteen years, between 1900 and 1914, the number of science graduates in the country more than tripled from two thousand to seven thousand (Macleod 1971a, b).

The optimism of a progressive thinker was sympathetically expressed by John Masefield, who would later be the nation's official poet (poet laureate) for over forty years, in his novel *The Street of Today.* The hero pronounces early in the book his opinion that once the fighting man had been top dog; then

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the spiritual man "made it hot for all who wouldn't or couldn't believe. Now there comes the scientific man who wants to get the power to make it hot for those who won't or can't conform to the plain dictates of science" (Masefield 1911: 20–21). The book itself begins with the hero explaining Mendelism at a dinner party. Significant as Masefield was, the most articulate, influential, and best-remembered voice of science-talk of the generation was the novelist and futurologist, H. G. Wells. His vision of a samurai class of scientific experts and his ridicule of war inspired a generation of men and women proud to call themselves "Wellsians" (Crossley 2011). In this atmosphere, applied science came to be associated with rational public policy, rational business practice ("scientific management"), and the source of all new things.[13] It was also increasingly interpreted as the application of pure science, which rooted it in traditional values and culture just as it reached out to an unknown and, to many, a disturbing future. For many, this emphasis on linking the new gadgets and enduring culture distinguished "applied science" from "technology."

British elite engineering teaching, unlike the German, was not generally separated from the universities. "Civic universities" taught vocational subjects to large numbers of students, often by absorbing local colleges and by adopting strategies validated in the 1870s. At Glasgow, Sheffield, and elsewhere, such colleges became departments of, or prepared for degrees in, "applied science." The newly regrouped Royal Technical College was associated with the University of Glasgow from 1913 and offered degrees in "applied science," so named because twenty years earlier the university had acquired the right to offer such degrees.[14] At Sheffield, a new university struggling to prove its academic status as a full and integrated scholarly center, the former municipal technical school was incorporated as the "Department of Applied Science." So at these institutions, engineering training exemplified applied science.

Technology was used as a designation by two rather different kinds of institutions in the early twentieth century. On the one hand, in the municipally run and City and Guilds technical colleges, which were seen to offer intense training in practice and little of its conceptual basis, engineering was identified with "technology" following the nineteenth-century tradition. On the other hand, a few of the most esteemed universities also promoted their studies of engineering as technology rather than as "applied science," citing the *Technische Hochschulen* as well as British tradition. Manchester was confident enough of itself and sufficiently impressed by German models to link up with the Municipal Technical College as its "School of Technology." On the question of degree titles, it compromised by preparing students for the university's degrees in "technical science," which evoked the qualification in *technische Wissenschaft* chosen by the *Technische Hochschulen* in Germany (Short 1974;

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Manegold 1989).[15] The university's industrial bent caused sufficient worry to the nation's Privy Council that in the 1904 charter of Manchester University, it was specified that the reorganized institution was entitled to offer degrees "provided that degrees representing proficiency in technical subjects shall not be conferred without proper security for testing the scientific or general knowledge underlying technical attainments."[16] Such a provision protected the university against the tendency "to degrade university teaching to technical teaching," in the words of Lord Balfour of Burleigh.[17] His contemporary, the German-educated Liberal Party politician R. B. Haldane, advocated the designation of applied science for the Imperial College he was promoting in London. He was, however, forced to give way. There, it was the local tradition of the City and Guilds, which Imperial College had incorporated, and the influence of its leader Philip Magnus that seem to have swung the decision to the federal name of "The Imperial College of Science and Technology."

As part of the modernization and efficiency agenda, the British government, challenged by the German formation of the Physikalisch-Technische Reichsanstalt, had established the (albeit less well-funded) National Physical Laboratory in 1899 (Moseley 1980). Such new institutions included committees of scientists now consulted on urgent government matters. In 1908, Haldane—inspired by the University of Göttingen, where he himself had studied and recently revisited—delegated Britain's development of aeronautics to a committee of distinguished physicists who were defined as "the experts," rather than to practical men who had actually flown (Gollin 1979). Most obviously during World War I, the government employed large numbers of scientists in research on military projects and to meet the needs of civilian industry. By the end of the war, the staff of the National Physical Laboratory numbered 532 (Fleming and Pearce 1922: 182). Alarmed by past German successes and anxious about the postwar world, the British government sought measures to promote "scientific education and industrial research."[18] Education, however, was beyond the remit of central government (Scotland had its own educational system), and what was founded in 1915 was the Department of Scientific and Industrial Research (DSIR). In the words of the first secretary, it had the remit "on the one hand, to encourage pure research, and on the other to organize applied research" (Heath 1919: 207). The former was largely delivered by support of graduate students (carefully not referred to as "education"); the latter, by subsidies for research in industry through matching funding conducted by industry associations. Although a wartime measure, this was planned as a preparation for competition in peacetime, when, as an MP pointed out in Parliament, "It will be more important for the child to know the date at which Oersted discovered the reaction between electricity and magnetism than even to know the date of the battle of Waterloo."[19]

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In retrospect, the formation of DSIR provided a key reference point in the history of government policies toward science. As in earlier years, however, this occurred within a much wider discourse of applied science, which provided a framework for governmental initiative.

As Edgerton (2006) has pointed out, the department was neither the largest nor the only funder of science during the early postwar period. Within the military, large research groups had built up at centers such as Farnborough (aeronautics) and Porton Down (poison gas). Additionally, industrial research, particularly in the electrical and chemical industries, was funded massively by companies. Imperial Chemical Industries, founded in 1926, had an annual research budget of a million pounds, several times that of all the research associations put together (Varcoe 1981). The research that came out of such novel laboratories was often, though not necessarily justifiably, praised as the basis of the new products that civilian and military life were soon enjoying.[20]

By the end of that period I have suggested we see as a *Sattelzeit,* 1899–1919, the terms "pure science," "applied science," "technology," and "industrial research" had settled down with associations that would develop but be broadly familiar for the next half century. It is important to emphasize that the new connotations of research—abstruse, benevolent, or malevolent in outcome—did not replace older meanings but were rather overlaid upon the pedagogical associations that had dominated earlier discourse.

## The Twentieth Century: From Education to Research

At the end of this transitional "saddle-time," the categories came to be useful in the new governmental adoption of policies for the support of scientific research. The first annual report of DSIR in September 1916 nodded awareness of the words of Huxley denouncing the differentiation between pure and applied science, but then emphasized its political reality: "The Council realise they have to deal with the practical business world, in whose eyes a real distinction seems to exist between pure and applied science."[21] The following year, a group of scientists responded to the new organization with their volume *Science and the Nation* (Seward 1917). As Gooday (2012) has shown, they could not agree on whether to advocate a vision of a pure science acting as the nerves animating a subsequently implemented applied science, or of science as a unitary category. It has been argued, however, that the scientists associated with the British Science Guild were emphasizing the prior importance of pure science to respond to the lay control of the DSIR (Hull 1999; Clarke 2010). This literature highlights the continuing ambivalence within the scientific community about the attraction of the distinction.

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To some scientists, the category of pure science assured independence and a certain distance from the violent uses of science so recently suffered. Clarke (2010) has shown, however, that the term "fundamental research" was attractive to the DSIR as a category mediating between pure and applied. At the same time, she says, even this neologism was unstable. Sometimes it could represent the more general mission-oriented work carried out in industrial laboratories; at other times it was used as a synonym for pure science. Such an ambiguity could serve as useful flexibility. In 1922, learning of the plans for a large Imperial Exhibition, the DSIR contracted the Royal Society to plan a central exhibit on "pure science" that would complement the "applied science" on display in the Halls of Industry and Engineering. It would demonstrate the advances in pure science that necessarily underpinned its applications. In practice, however, the distinction proved hard to implement, particularly because it was felt that demonstrations would be needed to interest the public. The organizing committee shifted its approach to "fundamental science," and ultimately the exhibition included a display on the development of the radio valve as well as on more obviously "pure science" (Royal Society, British Empire Exhibition Committee, 1925).

Even as they were criticized, the distinct terms "pure" and "applied" maintained their appeal. In July 1931, the Marxist physicist J. D. Bernal (1931) reported on the recent congress of the history of science held in London's Science Museum:

> Everyone agreed that pure and applied science were interdependent. The English emphasized the growing appreciation of the debt of industry and public services while the Russians pointed out the converse. To them, the development of pure science is dependent on that of economics and technics both for the problems they present it with and for the means provided for their experimental study.

This review was published not in an academic journal but in the widely circulated magazine of politics and general affairs, the *Spectator.* The relations between the subcategories of science were matters of general discourse and often, for all Bernal's perhaps ironically emollient words, sharply divided. Cecil Desch, the professor of metallurgy in Sheffield's Department of Applied Science, perceived threats to support of curiosity-driven science latent in the words of the Russians. In a November 1931 speech titled "Pure and Applied Science," he urged his audience at the University of Cornell to appreciate both the value of disinterested research, expressed by the recent exhibition about Faraday, and its multiple and surprising applications in industry (Desch 1931). In September that year, London's huge Albert Hall had hosted a display celebrating a century of induction discovered by Michael Faraday,

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with his purely scientific discoveries at the center of widening circles of recent developments representing the application of his work (James 2008). In an age of electricity, development of new uses became the narratives of applied science, just as stories of Faraday renewed their status as the illustrative anecdotes of pure science.

The alternative term "technology" was also widely used as a synonym when talking about gadgets, but it did not have the same connotation of dependence on "pure science" and elite culture. For example, H. G. Wells, who acclaimed the American "technocratic" movement, rarely used the term "applied science." The same goes for reports of the DSIR, specialists in individual subjects, and engineers. "Applied science" was a collective term not needed when a discipline or specialty were being discussed. Nor was it liked by engineers. The distinguished electrical engineer and industrial research leader Arthur Fleming avoided it both in his account of industrial research and in his history of engineering (Fleming and Brockelhurst 1925). As a modernizing electrical engineer, Fleming was anxious in his public utterances to address the interests and traditions of his professional peers.

Widely used elsewhere in the public sphere, the categories of "pure science" and "applied science" were nonetheless used to make sense of a range of important issues beyond science. The times were defined as an age of "applied science." The terms captured the cultural preeminence of gadgets and the science on which they were said to be dependent, both in terms of research and the educationally acquired skills of their inventors. Two politicians who had matured before the war, R. B. Haldane and Arthur Balfour, continued to be influential commentators on science after 1918. Both were philosophers with deep personal commitments to the rootedness of actions and inventions in ideas. Both were concerned that British people were not sufficiently committed to the linkage between thinking and doing. In 1927, Balfour (1927) reflected, "In the fundamental discoveries of pure science, this country, I believe, has taken its full share. About applied science I am not so sure." It was a contribution to British science policy discourse that was very widely reported at the time and would be taken up time and time again in the years to come. R. B. Haldane's nephew, the physiologist J. B. S. Haldane, was also adept at linking the mundane and the fundamental. He began a 1929 lecture to the socialist Fabian Society with the words "Western civilisation rests on applied science" (Haldane 1929). Writers, journalists, broadcasters, politicians, and museum curators popularized the term in the public sphere. The historian Peter Bowler (2009) has shown how popular journalists expressed a profound and widely held faith that fundamental problems could be overcome through applied science.

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The progress of modern medicine was frequently interpreted as evidence of the impact of applied science on the modern world. Its products in the interwar years, ranging from an understanding of vitamins A and D, to insulin, to the sulphonamide drugs, seemed to be offering a new kind of pharmacopeia and for the first time hope of rational chemotherapy. In London, the Wellcome Museum, funded by Sir Henry Wellcome, himself owner of a great pharmaceutical company, provided persuasive interpretations of the linkage of progress, science, health, and happiness (Arnold and Olsen 2003). Furthermore, the impact of applied science could be experienced in domestic life. The feminist writer Storm Jameson, writing in the *Daily Mail* in 1931, scorned those who romanticised the past and enjoined, "Make modern applied science lighten your work and enrich your leisure."[22]

Not just time was defined; so was space. In the currently peaceful world in which international travel and communication was increasingly common for private citizens, there was acute awareness of other countries. Was Britain falling behind Germany, France, or the United States? Talk about applied science provided ways of both bemoaning failure and celebrating success. International competition was exemplified by the weapons and progress of the recent war, and potentially the next war. Poison gas was the emblematic product of that interpretation, as gas warfare was still acutely recalled and its victims encountered daily. The influential *Daily Mirror* columnist Richard Jennings, reporting the 1922 British Association Meeting, asked, "Is scientific discovery destined to destroy mankind?" A few sentences later, he was talking about the applications of discovery because "it is applied science that matters to the average man."[23] To others, associated with the right wing of Oswald Mosley's "British Union of Fascists," the destructive quality of applied science was itself an attraction, offering a route to the development of yet more powerful weapons (Zander 2009). The *Daily Mail* newspaper, owned by Lord Rothermere, an admirer of Adolf Hitler (even though he feared Germany), was part of this movement but expressed its support in terms of "technology" rather than the more intellectualized "applied science." It was, however, the centralized new media of the age that were setting the pace in talk about science, providing privileged access with unprecedented reach.

Public broadcasting on the BBC had begun in 1922. By the late 1920s, the huge influence of the medium was being appreciated. The whole country had just one official channel to which to listen. There was a strict discipline in what could be broadcast under the Director-General Lord Reith, a Calvinist and himself under the eye of the government. Political controversy was excluded, as was trivia. Controversial talk about science, however, found a ready place, particularly from 1930, when the new head of talks, botanist and com-

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munist Mary Adams, took charge (Desmarais 2004; Jones 2012). Formerly an adult-education extramural lecturer in botany at Cambridge, Adams was keen to use the new medium to promote understanding of science within her BBC department of adult education. The number and range both of series and of voices, put together by Adams, were remarkable. Jones (2012: 930) has estimated at least forty broadcasts on science and society themes between 1930 and 1935.[24] Multipart series running at 7:30 in the evening included such topics as "Science and Religion" (twelve parts), "What Is Science?" (four parts), "Science and Civilisation" (six parts), "Scientific Research and Social Needs" (thirteen parts), and "Web of Thought and Action" (twelve parts). The points of view presented were diverse, but they all prioritized science and its connections to the world of today, in both its constructive and destructive aspects. Many of the talks were reproduced in the BBC's magazine the *Listener,* which was read by thirty-five thousand people by 1933, and were published in books, which attracted a further readership from enthusiasts. This represented by far the most intensive engagement with substantial issues of science in the public sphere in British experience.

The BBC expressed the diversity of meanings that science in general and applied science in particular had acquired over recent years. These meanings had been formed within diverse discourses about education and war, Britain's place in the world, and modernity, medicine, and agriculture. By the 1930s they were coming together. This convergence was promoted through such pioneering studies as Julian Huxley's *Scientific Research and Social Needs* (1934), first presented as a series on the wireless and then republished in the *Listener.* Godin (2006: 650) has credited to Huxley the formal "linear" taxonomy of research, formulated as a four-stage model from "background," to "basic," to "ad hoc research," to "development." We need to recall that this was, however, not framed within government; rather, the new discourse of science policy was expressed as part of a conversation in the public sphere. The book sold about two thousand copies but about thirty-five thousand people read the *Listener,* and radio audiences would have been in the hundreds of thousands. A review in the *Observer* newspaper pointed out how much more lively the broadcasts were than the printed page.[25] In his series of programs, Huxley reflected both on the general progression of ideas from pure to applied science, but also on the importance of influences flowing in the opposite direction. We need to recall here that his ideas were also discussed in the press. An 1936 article in the *Times* suggested Huxley's technical terms but concluded that for most people the distinction between "pure" and "applied science" would suffice: "However even if the distinction between pure and applied breaks down in theory, and should perhaps be replaced by some other terminology, such as basic and long-term research, on the one hand, as

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against ad hoc and development research on the other—it still remains convenient enough in most cases."[26]

The best known of 1930s reflections on policies for science today is J. D. Bernal's *Social Function of Science* (1939). This book is widely seen as the founding document of science studies. It was, however, also the culmination of a decade of talk about its topic. The assertion that science was indeed a "social function" had already concluded Huxley's book, which in turn rendered into print the sentiments of his 1933 radio series. Here the term clearly indicated the influence of Huxley's friend Bronislaw Malinowski. Pioneering structuralist anthropologist, coeditor with Huxley of the *Realist* magazine, and fellow broadcaster in the radio series on "Science and Civilisation," Malinowski was widely credited with promoting the concept of the social function among anthropologists of interwar Britain (Kuper 2014: 63).

Bernal's book expressed themes developed in the previous two decades not only among a small group of socialists but also among a greater circle of radio contributors on science. It also provided a pioneering and successful integration of polemical arguments for a new instauration of science with a rich panoply of quantitative data. Its own status as "science" and the ancestor of the "science of science" was expressed through its structure. The book was divided into two parts: "What science does" and "What science could do." Bernal called for a much more intensive commitment to investment in science even within a capitalist society, though he believed it was only in socialism that its full benefit could be exploited. He was known as a radical, but he used the traditional terms to frame his vision: applied science was referred to on nineteen occasions in the book, though he was concerned with pure or fundamental science. Bernal's contribution, forward-looking as it would appear, was also rooted in past British discourses of science as well as in Marxism.[27]

## Envoi

Socialism, if not communism, and radical change did come to Britain at the end of World War II. In October 1943, a committee of parliamentarians and scientists produced a report on scientific research and universities after the war. It called for a vast expansion of industrial research but also emphasized that "applied science cannot live on the fundamental discoveries of past generations" (Parliamentary and Scientific Committee 1943: 3). Early in 1944, two meetings of influential scientists, social scientists, and science journalists were held at Nuffield College, Oxford (Ritschel 1995). Many of the participants were people who would be important in thinking about science in the postwar world, including Solly Zuckerman (the future long-term science advisor),

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C. H. Waddington, C. P. Snow, Henry Tizard, John Cockroft, Harold Hartley, and two Hungarian economists who would become familiar as advisors to Harold Wilson in the 1960s, Tommy Balogh and Nicholas Kaldor, as well as such prewar leaders as Cecil Desch. In January and April, they conducted meetings leading to a "Statement of problems of scientific and industrial research," which was submitted to the chancellor of the exchequer (Nuffield College 1944). Four complementary perspectives were taken on science, from the points of view of industry's research needs, of the supply and needs of research workers, of society, and finally of education, where the conference was asked to reflect on the desirable relationship between pure and applied research.[28] A great expansion, including the formation of a further two dozen government-funded research associations, was called for. Despite the distinction of its authors and the wide-ranging scope of its concerns, this document, which was a manifesto for future science policies and whose authors included many people who would be influential for the next two decades, has been curiously overlooked. It also drew on twenty years of discussion about science expressed in the carefully summarized suggestions of recent years.[29] At its heart was a distinction between fundamental and applied research, the dependence of the latter upon the former, and an expression of their common needs:

> These considerations apply equally to what is called "fundamental" and to what is called "applied" research—by which we mean broadly speaking, on the one hand research carried on purely for the advancement of scientific knowledge, without direct reference to any practical outcome, and on the other research designed to achieve practical results either in industry or in any other field in which scientific discovery can be used to increase man's working mastery over the forces of nature. (Nuffield College 1944: 11)

It would be pleasant to end with the conclusion that "at last" science policy had been invented. Certainly the history of the field often takes as its founding moment the American "Bush report" of 1945, *Science, the Endless Frontier.* We know, nonetheless, that despite its fame, the National Science Foundation was not established for some years and had a more complex origin. Similarly, in Britain, while the Nuffield College paper highlights the widespread appeal of the call for more basic research to sustain applied research, most science expenditures were firmly under the control of major departments (particularly defense) for whom science was an instrumental tool. Even the appointment of a participant at the wartime Nuffield College meetings, the reforming conservative Quintin Hogg, as minister "for science and technology," did not happen until 1960, and his was more of a coordinative than a directive role. It could be said that many of the arguments of the early twentieth century did not achieve their goal until 2010, when scientific research, higher education,

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and industry policy were unified in one government department (the Department of Business).

We might conclude, nonetheless, that the categorizations of science, expressed as World War II was drawing to an end, were significant. They served as a passage point between a century and a half of wide-ranging discourse in the public sphere and the development of a postwar policy dialogue. Twentieth-century talk about research had drawn on contemporary concerns about war and rapid change and on nineteenth-century arguments about technical education, the privileging of practical training, and the arguments promoting applied science formulated in reaction. That discourse had been shaped by more than policy and politics. Broadcasting had proffered opportunities for coalescence of meanings from a variety of discourses. The challenge of disruptive technologies, which frightened as well as impressed, was managed by the language of applied science, which provided assurance that these awe-inspiring novelties were based on the enduring methods of science. When it might have seemed that culture and language would be torn apart, the enduring names of strong organizations provided closure of old debates. What was at stake in talk about "pure," "fundamental," and "applied science," and about "technology," was much more than either pure epistemology or the dynamics of innovation.

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## Notes

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1. For more detailed explanation of the methodological approach chosen here, see Bud 2013a.
2. Linder and Smeaton (1968: 272) compared the original Swiedauer's translation and Bentham's—the first is the original, the second Swiedauer's lingua franca, and the third Bentham's: (1) "Wenn sie aber zum besondern, oder allgemeinen, Nutzen angewandt wird . . . so verdient sie den Nahmen der angewandten (Che-

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mia applicata) eben wie die Mathematik in solchen Fällen diesen Zunahmen erhält." (2) "But when it is applied to a particular or general use . . . it merits the name of chemia applicata, just like mathematics obtains this name in such cases." (3) "But when it enters, more or less, into details, and applies itself to use . . . it may be termed [chemia applicata] mixt, particular, or popular chemistry, after the same manner that mathematics is denominated in the like cases."

3. The use of the word "mixed" was related to its use in the phrase "mixed mathematics," in which both axioms and material objects were invoked. See "mixed" in the *Oxford English Dictionary* (usage no. 5). *OED Online, June 2017, consulted November 13, 2017.*
4. University Library Cambridge, Guard Book, 28.5.1 ms ul, report revised by the Syndicate, 24 May 1865.
5. House of Commons Debate, vol. 191, "Motion for a Select Committee," 24 March 1868, cc160–86.
6. The issue of engineering education was a matter of heated dispute in the nineteenth century and of deep pride at the time, and has been widely written about since then (Buchanan 1989). For the related American disputes, see Calvert 1967.
7. The origins of this well-known quotation are complex. For instance, Hulin (1986) has highlighted the tension between Pasteur's Ecole Normale Superieur, dedicated to the training of future professors and teachers, and the Ecole Polytechnique, with its industrial bent, which Pasteur had addressed in a related way in 1868. Pasteur's challenge was also soon known in France. See, for instance, an essay by George Pouchet (son of Pasteur's old adversary Felix) (Pouchet 1872: 44–45; see also Fauque 2005).
8. "Tyndall on Illumination," New York Times, 16 February 1879.
9. The year 1957 is significant as the period of post-Sputnik introspecting in the West and the sense that where Germany had once outpaced Britain, now the Soviet Union was outpacing the United States.
10. In 1821, there are no uses in any newspaper; in 1822, only five; and in 1823, there are 142 uses. Data retrieved January 2016 from www.britishnewspaperarchive.co.uk.
11. William Perkin's discovery in 1856 of the coal-tar-based dye that he called "mauve" is generally regarded as the basis of the synthetic dye industry, which came to be dominated by German companies from the late 1860s.
12. In his description of the late eighteenth-century period he denotes as the *Sattelzeit,* Koselleck (2011: 10) explains, "While analogous developments can be discerned at the threshold of every epoch, a significant number of indicators do point to such an accelerated process of change in the political and social language of the period studied here. If so, then it is likely that the *modern world (Neuzeit)* was simultaneously experienced as a *new age (neue Zeit)*. Sudden changes permanently altered the once-familiar world, thus transforming the horizon of experience and with it the terminology (especially the central concepts) that had once been—reactively or proactively—tied to it." Similar phenomena could be observed in the period 1890–1919. The parallels are expressed in economic history

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by the term Patrick Geddes (1915: 59) used for this period, "Second Industrial Revolution," coined as early as 1915.

13. "Business has been conducted too much in the spirit of an art, too little in that of applied science. The modern tendency is to introduce the exacter methods of science" (Hobson 1913: 198).
14. The Glasgow and West of Scotland Technical College, "Relations with the University of Glasgow," November 1909, in "Special Committee on Relations with the University. Minutes &c," 61/27/1, Strathclyde University Archives.
15. Curiously, German practice was changing in the other direction at this time, as *Technische Hochschulen* also started to offer doctorates. At the opening of the *Technische Hochschule* in Danzig in 1904, the Kaiser himself praised the ways in which such institutions combined "theoretische und angewandte Wissenschaft" and acted as scientific universities (Mangoldt 1904: 13).
16. "The Victoria University: Manchester Charter," *Manchester Guardian,* 29 May 1903.
17. National Archives, U.K. (NA), PC 8/605 pt 2, ff 89731 p. 30, Question from Lord Balfour to R. B. Haldane, Transcript of Shorthand Notes of Proceedings before the Committee of Council, on 18 December 1902.
18. "Mr. Pease informed the deputation that the particular problems to which they had drawn attention had been present [*sic*] to the Board of Education for some time past, and that a scheme had been elaborated under which substantial additional assistance would be given by the Government to scientific education and to industrial research." See "Report of meeting with Royal Society and Chemical Society." NA, ED 24/79, 6 May 1915. See also Addison to Pease; NA, ED 24/1581, 20 March 1915, in which the plans of the time are described as "Advanced Scientific Education and Research."
19. House of Commons Debate, vol. 71, Arthur Lynch, 13 May 1915, cc1930.
20. The modern literature on interwar industrial research is of considerable interest in highlighting the status and public relations value of such work and its problematic, if occasionally enormous, direct knowledge value to the companies. See three valuable studies of Du Pont (Hounshell and Smith 1988); Philips (De Vries and Boersma 2005) and Metropolitan Vickers (Niblett 1980).
21. "Science and Industry," *Manchester Guardian,* 1 September 1916.
22. Storm Jameson, "Get the Best out of Life," *Daily Mail,* 4 April 1931.
23. "Destructive Science," *Daily Mirror,* 9 September 1922.
24. Jones (2010: 66–67) has suggested that 10 percent of BBC output was comprised of talks, and 10 percent of these were on science, so that, by time, science talks were getting 1 percent of the schedule. With total BBC audiences of thirty million by the end of the decade, a 1 percent share for science came to several hundred thousand.
25. "Listeners' Science," *Observer,* 29 July 1934.
26. "Science," *Times,* 1 January 1936.
27. On Bernal and both his scientific circle and his relationship to socialism, see Werskey 1978.

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28. Nuffield College, Wartime Research Committee and Social Reconstruction Survey, 12th Private Conference, "The Post-War Organisation of Scientific and Industrial Research: General Purpose of the Conference," 13 November 1943, MSS NCPC 7/2/2, Nuffield College Archives, Oxford University.
29. G.E.F, "Proposals for the Post-War Organisation of Scientific and Industrial Research," Nuffield College, Wartime Research Committee and Social Reconstruction Survey, 12th Private Conference 8-9 January 1944. Ff 7/2/27–7/2/32, Richard Gregory papers, SxMs 14/1, The Keep, Falmer.

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Chapter 2

# Professional Devotion, National Needs, Fascist Claims, and Democratic Virtues

## The Language of Science Policy in Germany

*Désirée Schauz and Gregor Lax*

In the second half of the twentieth century, "basic and applied research" became predominant and internationally acknowledged key concepts of science policy. Modern antonyms such as "pure and applied science" or "basic and applied research" spread across borders. However, the uses of these concepts, their discursive functions, and their ascriptions varied according to divergent national settings. This chapter is interested in the German particularities of the discourses revolving around these terms. What is special about the German concepts of *Grundlagenforschung* (basic research) and *angewandte Forschung* (applied research)? What did German scientists, politicians, and other experts mean when they used these terms? How did the concepts affect research policy and the overall understanding of science?

In the nineteenth century, Germany became a role model for leading science nations. The institutionalization of the natural sciences at German universities from the early nineteenth century onward was a success story. Experimental training and research units at German universities attracted more and more international students, particularly in chemistry and physics. One key to this successful professionalization lay in the fact that nineteenth-century natural scientists wholeheartedly embraced the academic ideal of

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pure science. After World War II, scientists in the Federal Republic of Germany,[1] who were striving to catch up with Western scientific standards, were dedicated to the idea of independent science and basic research to a similar extent. They even clung to the concept of basic research until the 1990s, when their colleagues in the United States—the leading science nation of the twentieth century—had long been converted to advocates of the applied side of research. What at first glance seems to point to a persistent German idealism of science—interrupted by the Nazi regime—on closer inspection turns out to be a history with several twists.

We argue that the shift from pure science to basic research was not a linear one. Moreover, the new concept of basic research cannot be seen as a simple conceptual variation of the older idea of pure science. Already in the first half of the twentieth century, the increasing industrial and governmental demand for research challenged the ideal of pure science. The support for science—that is, the institutional and financial underpinnings of a research landscape that became more and more differentiated—had to be renegotiated, and it was exactly in this period that neologisms such as *Grundlagenforschung, Zweckforschung* (goal-oriented research), and *Gemeinschaftsforschung* (collaborative research) emerged.

With regard to the specific national settings, the political history of Germany is distinctive for this case study. The Treaty of Versailles affected German science policy both directly, due to restrictions on German research by the allies, and indirectly, due to the following economic crisis and international isolation. After 1933, the academic elites were initially met with great hostility by the National Socialist Party. The racist personnel policy expelled many talented researchers from the German research landscape. It was only later that the Nazi regime pinned its hopes on research that was promising for armament improvements and conducive to its autarky policy. After the end of World War II, the allies regained control over German research activities. In contrast to 1918, however, they sought to integrate the Federal Republic into the Western world, and science soon became an integral part of the ensuing efforts at democratization and economic recovery. Yet, in addition to these political settings, Germany's specific academic culture and philosophical traditions dating back to the nineteenth century have to be taken into account to grasp the meanings and discursive role of German concepts of science policy.

Against this historical background, this chapter traces the emergence of the concepts of basic and applied research in Germany as part of a broader and historically grown semantic field. The chapter starts with a short history of the concepts of pure and applied science in the nineteenth and early twentieth centuries. The next section focuses on the Nazi period, when the term *Grundlagenforschung* gained importance for the first time as a political

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concept. In the following section, we discuss the semantic pluralism right after World War II, in which the transformation from fascism to democracy determined German science policy. Afterward, we show how "basic research" prevailed as an unofficial funding rationale that adopted the idea of pure and independent science and, at the same time, conveyed the promise of economic prosperity. Then we analyze the role of applied research and how it slowly gained importance in the shadow of basic research. The final section concludes by discussing the specifics of how German scientists and policy makers used "basic and applied research" and related concepts. We therefore contrast our main findings with the existing literature on science policies in other Western countries.

## From Academic Identity to National Needs

The historical development from the nineteenth-century academic ideal of pure science to "basic research" as a multifunctional symbol of science policy in the twentieth century is not a linear one. Quite the contrary, the notion of science and the societal expectations toward science were constantly shifting, which caused several twists in this history. When natural scientists started to enter the universities in the late eighteenth century and in the nineteenth century, they had to come to terms with the philosophical understanding of science, which focused on theoretical, pure knowledge. This even applied to new German institutions of higher education, such as the *Bergakademie,* which responded to the policy of mercantilism and cameralism and its demand for experts in mining, forestry, and engineering (Klein 2010: 441). Natural sciences such as chemistry, which were based on empirical and experimental work, thus had to prove that they could produce theorems meant to be imparted to students (Meinel 1981).

This academic definition of science was backed up by the German traditions of *Erkenntnistheorie* (epistemology), in which rationalist approaches were ranked higher than the empirical ones. Immanuel Kant defined the natural sciences as pure science only because they are based on "a priori" conceptualizations of nature, such as assumptions on cause and effect (Kant 1869: 46–84). Consequently, Kant turned the religiously coined notion of laws of nature into a secularized ideal of the human reason that applies rules to bring order into the external world (Hampe 2007: 76–79). In the early nineteenth century, this longing for the wholeness and absoluteness of ideas gave birth to a new generation of natural philosophers, who had a big impact on German natural scientists' notion of science (Phillips 2012). These philosophers clung to the epistemic hierarchy according to which "a posteriori" approaches played a subordinated role in contrast to metaphysical ways of

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knowing. Natural philosophers like Friedrich Schelling (1831: 10) argued that science had to venture into the unknown before any considerations of utility or application.

In sum, the new academic setting and the leading discourse in philosophy formed the identity of German natural scientists based on the ideal of pure science and its respective academic virtues. Due to the early establishment of the natural sciences at universities, German scientists adapted perfectly to the idea of *Wissenschaft* (science), which comprises all academic disciplines from what we would call the humanities today to the natural sciences and was later considered a peculiarity of German science (Meyer 2012; Phillips 2015).

This predominant epistemic ideal also had an impact on the way scholars and scientists depicted the relationship between science and technology at that time as implying a belief in deducing endless applications and specific knowledge from universal principles. This process of knowledge transfer was alleged to be one-directional. In nineteenth-century Germany, technology counted as one possible application of science; it was therefore commonly defined as *angewandte Naturwissenschaft* (applied natural sciences). Even economists assumed that only scientific discoveries and theories paved the way for inventions or technical improvements. In his handbook on politics and government, the economist Constantin Rössler (1857: 179) argued that "Technical science may stimulate pure science to a certain extent, but, on the whole, technology is much more at the receiving end. Pure science is always further ahead of applied science, and never the other way round. However, technology finally turns science into a common good."[2] In the course of the nineteenth century, this model of the science-technology nexus became particularly relevant for the advancement of technical training. Scientific training of engineers in early trade and polytechnic schools gained more and more importance in the curriculum. In the heyday of German industrialization, the appreciation for engineers by society was growing rapidly, and teachers in engineering schools started to strive for academic status (Lundgreen 1990: 57–58; König 1999). In the last third of the nineteenth century, most German states therefore established *Technische Hochschulen*[3] (Manegold 1989: 219–231). Having scarcely assumed their place as pure science within a university, natural scientists felt professionally challenged by the academic ambitions of the engineering sciences. Thus, this is the time when natural scientists started to use the hierarchical distinction between "pure" and "applied science" as a means of professional "boundary work" (Gieryn 1999: 51–62). Within the hierarchical order of disciplines, the attribute "applied" placed engineering sciences in a subordinate position.

However, scientists and engineers not only competed for academic status but also for societal support and acknowledgement. Scientists became aware

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that, in comparison to the visible impact of technological developments on national prosperity, the advancement of science and its effects were less visible and predictable (e.g., Liebig 1862: 33). Therefore, scientists praised the utility of science by arguing that only scientific discoveries of the laws of nature laid the ultimate cornerstone for technical progress (e.g., Virchow 1877: 8–9). Defining technology and engineering sciences as "applied science" thus worked as a promotional strategy with which the natural sciences claimed their share in material benefits. This public rhetoric, however, contrasted with the identity of scientists dedicated to "science for its own sake." Hence, it was right in the heyday of the pure science ideal that scientists began to narrow the image of scientific utility to the promise of technological progress.

Within the German community of engineers, the distinction between "pure" natural sciences and "applied" technical sciences was quite contested. While the label "applied sciences" ennobled modern technology and stressed the academic aspirations of engineers, their representatives at the new *Technische Hochschulen* were engaged in a debate about the right proportion of theoretical knowledge—mainly deriving it from the natural sciences—and practical teaching units (Heymann 2005: 58–82; König 2014). Moreover, these new professors gained in self-assurance and advanced the view that technology contributed to the progress of science just like science helped to improve technology (Riedler 1900: 12).

Actually, in the late German *Kaiserreich,* when industrial demand for scientific research was increasing significantly, the ideal of pure science began to lose its rhetorical power. Scientists, especially chemists, who had become used to interacting with the nonacademic, economic sphere, tried to flatten the hierarchy between pure and applied science by emphasizing the similarities of the scientific and technological endeavor. This was, essentially, the systematic approach to research and the desire to venture into the unknown. According to Wilhelm Ostwald, a Nobel laureate and a pioneer of physical chemistry, scientists differed from engineers only in their long-term goal of finding final explanations, whereas engineers abandoned the path of scientific questioning after having found new technological solutions (Ostwald 1905; 1908). This phenomenon of blurring the boundaries was restricted to research fields that were highly relevant to industry. In the academic hierarchy, universities representing "pure science" still ranked higher than *Technische Hochschulen* did.

In the early twentieth century, the German research landscape and national science policy began to change more generally: *Wissenschaftspolitik* (science policy), which used to focus on aspects of higher education, subsequently was complemented by *Forschungspolitik* (research policy) that aimed at organizing scientific knowledge production in favor of national and industrial needs (Szöllösi-Janze 2005; Stichweh 2013: 135–138). In 1911, a new

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research organization, the Kaiser-Wilhelm-Gesellschaft (KWG, Kaiser Wilhelm Society), was founded. Financed by industry and the German government, it responded directly to the rising industrial demand for research by establishing new institutes outside of universities focused entirely on research. Originally, its founding father wanted the KWG to be devoted exclusively to the natural sciences (Harnack [1909] 2001). The KWG institutes were detached from the established university system and run by academically renowned scientists (Vierhaus 1990; vom Brocke and Laitko 1996; Hachtmann 2007, vol. 1: 81–137). The KWG was expanding noticeably and established a total of twenty-three research institutes within two decades. Many of these institutes focused on material research (coal, iron, fiber, metal, leather, and silicates), which was of great interest for companies, especially for small ones unable to afford their own big laboratories.

The expansion of the Kaiser Wilhelm Society demonstrates that World War I and the demand for research in the name of national defense served as a catalyst for this new level of public engagement in science policy and research planning (Maier 2007: 9). Neither the pitfall of Fritz Haber's scientific contribution to poison gas and German warfare nor the cultural controversy over the means and ends of science and technology during the Weimar years hindered politicians and scientists from pursuing a research policy that took the interests of the private sector into account and expanded the financial aid for scientific research.[4] The financial and economic crises in postwar Germany spurred the initiative for a new funding organization for science, the Notgemeinschaft der Deutschen Wissenschaft (Emergency Association of German Science)—the predecessor of the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation), which became a cornerstone of German research policy. Although it was primarily financed by the government, its establishment in 1920 was the result of negotiations between leading natural scientists, ministry officials, and private stakeholders (Marsch 1994: 44–60, 84–95). In parallel, representatives of German industry launched another foundation, the Stifterverband für die Deutsche Wissenschaft (Donors' Association for the Promotion of Humanities and Sciences in Germany, thereafter Stifterverband), which acted as an additional private fundraiser collaborating with the Emergency Association of German Science (Schulze 1995: 59–83). Yet German research policy had to meet conflicting requirements after World War I.

On the one hand, German science policy presented science as a cultural asset. Because of the scientific engagement in warfare and the following diplomatic isolation, the German scientific community worried about losing its position as an important international hub for academic exchange. In fact, due to the military relevance of science, the Treaty of Versailles imposed restrictions

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upon German research. In the Weimar Republic, especially scholars in the humanities referred to the nineteenth-century Humboldtian idea of a university, which comprised the ideal of pure science and a universalistic understanding of education as a counterpoint to a specialized scientific-technologized world (Paletschek 2002: 191–195). Furthermore, the German attempts to catch up with international science, for instance, by collaborative scientific expeditions and research projects abroad turned research policy into cultural policy, which served as an instrument to rebuild foreign relations by other means.

On the other hand, German politicians and scientists hoped to overcome the postwar economic crises and the debts of reparations by research and innovation. In a period in which research policy was mainly aligned with national needs, the ideal of pure science with its academic claim of exclusiveness seemed to be less useful for communicating to the public the need for additional financial support of research. The increasing demand for project-oriented funding crossed institutional boundaries and stimulated a new language of national science policy.

In 1920, the Emergency Association launched a funding program to advance public health, the economy, and the greater public good (Schmidt-Ott 1921, [1925] 1968). At the beginning, this program focused on research in the natural sciences. From 1925 onward, it operated under the name *Gemeinschaftsarbeiten,* which became a new and powerful concept for German research policy. The joint efforts of researchers from different disciplines and institutional backgrounds—universities, industry, and the new institutes of the KWG—aimed at helping Germany to recover economically. Given the scarce financial resources during the Weimar Republic, collaborative work stood for the attempt to rationalize the production and exploitation of scientific knowledge.

According to the historian Jochen Kirchhoff (2007: 206–207), the organizers of the Emergency Association deliberately borrowed the concepts of "collaborative work" and "collaborative research" from U.S. research policy. In the context of technical sciences, the concept of collaborative work actually emerged already before World War I, but it became more common only after the Emergency Association had started to use it as a category of research policy.[5] Striving for national resurrection after the lost war, it was the symbolic meaning of "collaborative" that made the concept particularly attractive to German science policy advisers. Thereafter, they promoted science as a national project, which was, however, not exclusively technology-driven. The program also defined science as a cultural value. This was necessary to shake off the image of the German aggressor equipped with high-technology weapons and to rebuild international affairs based on scientific exchanges and international cooperation. Overall, the Emergency Association intended

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to restore Germany's national identity and its economic strength (Kirchhoff 2007; Flachowsky 2008: 46–109).

## Research Policy in the Nazi Period

After the National Socialist Party took over power in 1933, the ideal of pure science definitely faded. At the very beginning of their reign, the Nazi Party did not show much interest in research policy at all. On the contrary, representatives of the Nationalsozialistischer Deutscher Dozentenbund (National Socialist German University Lecturers' League), which represented the younger generation of lecturers attempting to bring German universities into line with Nazi ideology (Grüttner 2002; Nagel 2008), attacked the established scientific elite. They denounced the nineteenth-century humanistic notion of academia as a selfish project pursued by the liberal bourgeoisie that had estranged science and scholarship from the German people (Henkel 1933; Krieck 1933). However, the attitude of National Socialists toward science changed when the government introduced the four-year plan in 1936, which aimed at preparing German society for war by striving for economic autarky and boosting the armaments industry. In 1937, the government installed the Reichsforschungsrat (RFR, Reich Research Council)[6] that operated as a central body for research organizations.

The RFR could easily revive the Weimar funding program of collaborative research as it fitted in with the *Volksgemeinschaft* ideology and with the council's aim to concentrate all research forces for the war plans. Surprisingly, the establishment of the RFR marked exactly the period when the concept of *Grundlagenforschung* (basic research) became a relevant category for the first time in Germany.[7] Together with the new concept of *Zweckforschung* (goal-oriented research), German policy makers introduced a new conceptual pair that followed the funding rationale of the four-year plan to which the RFR had to adjust. Its first president, a military general and a professor of army technology, Karl Becker, defined basic research as science that could not be "commanded and accelerated." He guaranteed, that "as far as researchers and facilities in the institutions in question offer even some guarantee of success," he would abstain from exerting any control over these institutions (K. Becker 1937: 26). The "institutions in question" were institutes of the KWG, which were relevant to warfare or characterized by their close relationships to industry. In return, goal-oriented research, which was meant to build on basic research, had to fit into the schedule of the four-year plan. The term denoted primarily industrial research leading to the development of advanced technology. According to the four-year plan, industry had to be completely transparent about its research activities (K. Becker 1937: 25, 27).

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A good example for the bifurcated research rationale after 1936 is the field of aerodynamics and fluid dynamics. Whereas the Aerodynamische Versuchsanstalt (Aerodynamic Experimental Station) in Göttingen was placed under the direction of the Ministry of Aviation, the Kaiser-Wilhelm-Institut für Strömungsforschung (Kaiser Wilhelm Institute for Fluid Dynamics) was entitled to perform "basic research" (Eckert 2017: 213). Both institutes were run by the same person: professor of mechanics Ludwig Prandtl. Furthermore, the KWG adopted the new nomenclature of the Nazis' research policy quite early (KWG 1939: 322; Telschow 1940: 753).

The new language deployed by Nazi officials proved to be important for communicating their research policy to both the scientific community and the public. The distinction between "basic" and "goal-oriented research" indicates that policy makers accepted, to a certain extent, the autonomy of scientific research: science as a whole could not be conducted according to a strict plan. This autonomy ended, of course, when it came to staffing policy (Schüring 2006; Orth 2016). Moreover, it required German scientists willing to serve the goals of autarky and warfare. In press campaigns promoting German research efforts, officials intended to demonstrate how German scientists, whose work was less visible, contributed—through basic research—to the public good during wartime.[8]

Besides addressing German scientists, science policy experts used the new conceptual pair to reflect on how increasing industrial demands for research affected the institutional setting. Even before 1933, under the paradigm of rationalization, experts deliberated upon the proper way of organizing research in order to quickly achieve societal and technological progress without duplicating efforts in both academic and industrial research. The fact that big companies conducted more and more research in their laboratories raised the problem that good salaries in industry attracted increasing numbers of talented researchers. The future role of universities as training and research institutions and the division of labor between academic and industrial research thus became vital questions of research policy. Furthermore, the changing research practices also led to a discussion about the advantages of individual or team research. The terms "basic research" and "goal-oriented research" were part of these ongoing negotiations (Drescher-Kaden 1941: 10, 16–17; Verein Deutscher Chemiker 1943; Stadlinger 1944: 227, 229; see also Orlans 1969: 114–115; Strupp 2001: 2).

In the end, however, scientists needed some time to adopt the new nomenclature of Nazi research policy. When "basic research" became more widespread after 1939, two things were striking. First, the term primarily appeared in fields of the natural sciences that were close to technical application or at least promising for military, economic, and political aims. Second, most

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scientists mentioned *Grundlagenforschung* and *Zweckforschung* in the same breath to stress the necessity of both "basic" and "goal-oriented research." Whenever it came to explicitly defining terminology, they depicted basic research as the study of nature, devoid of any concrete notion of how it might be applied. Yet having just drawn such a distinction, scientists immediately strove to emphasize that basic research could not be narrowed down to certain research practices and institutional settings, whether in the industry, in universities, or in other research institutions (Bauermeister 1938; Ostwald 1942: 130–131; Niemeier 1944: 106–107). Furthermore, scientists often criticized the distinction between basic and goal-oriented research as misleading because it suggested that basic research was far removed from any notion of useful application (Endell 1942: 113; Ostwald 1942: 130–131; Zenneck 1944: 10). Only a few scientists actually recognized the semantic shift from pure science to basic research and criticized the new term for constraining science to technological ends (Richter 1943: 207).

The way scientists defined "basic research" must be seen as a vestige of scientists' former understanding of science as contemplating nature and its difference from technology. The interdisciplinary research field of forestry is a good example, as it represented a utility-oriented notion of science first and foremost. Forestry research promised more profitable cultivation and effective technical treatment of wood as a raw material. Germany's rise as a colonial power in the late nineteenth century had already transformed forestry into a politically and economically significant discipline, fostered by the German state. In the four-year plan and during the war, the issue of raw materials, and with it the supply of wood, gained even greater importance (Steinsiek 2008). In forestry, basic research and goal-oriented research represented two equivalent subareas of the discipline: one studied the nature of the substance wood—for example, the physiology of trees—and the other analyzed trees' material properties and the effects of technical treatment. The overall goal of both research fields was to acquire knowledge about the optimal use of wood as a resource for the economy (Runkel 1942: 305–306).

The fact that scientists highlighted the utility of research in general can be understood as a strategy to secure financial support for their respective research fields. However, one question remains unanswered: if the classification of research types appeared to have little consequence for the scientific community, why did scientists deploy the new terminology often in a way that appeared reminiscent of the nineteenth-century boundary discourses? Given the uncertain status of scientists at the beginning of the Nazis' reign and the introduction of the RFR as the central body responsible for managing all research efforts, scientists obviously felt the need to renegotiate the conditions under which research was conducted under National Socialist rule.

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They therefore adopted the new concepts in a way that enabled them not only to communicate their commitment to the regime, but also to align the science policy of their time to their specific professional interests and goals. Back in the Weimar years, the individual freedom of professors allowed them to meet individual research interests and socioeconomics needs. Given the academic success story of the natural sciences in Germany, the professional ideal of pure science became almost dispensable. Established professors guaranteed the integrity of science by their personal standing and reputation. Facing the Nazi regime and its war preparations, however, the way scientists inscribed nineteenth-century definitions of science in the new concept of basic research suggests that they tried to protect themselves against unrealizable, exorbitant expectations of Nazi policy makers.

In the late 1930s and early 1940s, German scientists insistently demanded more attention for basic research. In doing so, however, they did not aim at returning to the ivory tower of pure science—that is, they did not fight for absolute professional independence that ignored any political expectations toward research. The majority did not oppose the overall political scheme of the Nazi regime. For example, their plea for basic research explicitly referred to research fields that were highly relevant to industry and war plans (KWG 1939: 322; Telschow 1940: 753; Hoffmann and Suhr 1944: 550). Yet they claimed a right balance of basic and goal-oriented research by arguing that research needs time and that the future utility of scientific outcomes is not foreseeable as readily as future societal needs (Thiessen 1938: 730; Ramsauer 1943; Schultze 1943: 201). Recent studies reevaluating German science during the Nazi period demonstrate that, apart from the racist staff policy, researchers were able to perform normal science according to their output (Ash 2006: 34–35).[9]

Most scientists in this fascist period continuously believed in the national benefits of science and its contribution to technological progress, but the concept of basic research also conveyed the experience that the relationship between science and technology was complex and its outcomes hard to predict. With regard to the scientific community, the discursive function of the concept of basic research lay in the possibility of communicating the uncertainty of scientific progress while promising scientific utility in the long run. What was specific to the wartime period was that scientists were seriously concerned that scientific knowledge might run short if researchers aligned knowledge production exclusively with immediate technological needs (Ziegelmayer 1936: 253; Stock 1938: 150–151; Brüche 1944: 113). The key rationale, therefore, can be characterized as an argument of knowledge sustainability, which appeared in a quite similar way in the U.S. scientific discourse during World

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War II (Barton and Burnham 1943: 176; Simons 1943: 391; Taylor 1944: 250; see also chapter 3).

## Dealing with the Fascist Past and the Democratic Future

Immediately after World War II, the Allies intended to suppress all German research activities that might have been relevant for the development of armaments. The Allied Control Council Acts and the ensuing executive regulations specified by each of the Western occupation zones prohibited any basic or applied scientific research with military relevance (Frowein 1949).[10] It is important to note that the crucial criterion for prohibition was the military potential of research projects, rather than differences between basic and applied research. Moreover, in the immediate postwar period, the nomenclature of German research policy was not yet settled, and therefore different labels categorizing science and research coexisted. From 1950 onward, however, the use of *Grundlagenforschung* increased, while that of alternative terms, such as *reine Wissenschaft* or *freie Forschung,* was stagnating.[11] Organisation for Economic Co-operation and Development (OECD) statistics on research and developments finally led to an international synchronization of key concepts of research policy (Godin 2005). *Grundlagenforschung* and *angewandte Forschung* thus became the predominant synonyms for the English terms "basic and applied research." In the 1960s, however, the concept of *reine Wissenschaft* could still work as a synonym for basic research, but it became less relevant. The semantic pluralism in the aftermath of the war responded to various requirements that concepts of German research policy had to fulfill in the era of so-called Westernization, in which the Federal Republic recovered economically with the financial support of the Western allies and ran through a transformation from fascism to democracy by adopting sociopolitical and economic models of the Anglo-American World (Doering-Manteuffel 2011).

The concepts categorizing scientific research after 1945 can be roughly divided into two groups. The first includes terms like *Grundlagenforschung, reine Wissenschaft, reine Forschung, freie Forschung,* and *freie Wissenschaft*. These concepts were described as theory-laden and more oriented toward acquiring new knowledge in and for science rather than toward external concerns. The second group encompasses concepts characterized by practical orientation, such as *angewandte Forschung, Anwendungsforschung, Industrieforschung,* and *Zweckforschung.* The meanings of these concepts were partly overlapping, and attributes from older concepts (such as pure science) reverberated in the new terms (such as basic research); even the distinction between the two groups of concepts was fuzzy or contested. As a result, intermediate terms such as

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"application-oriented basic research" emerged (Stifterverband für die Deutsche Wissenschaft 1967).

Although actors involved in research policy were aware that it was difficult to draw a line between basic and applied research in practice, a normative hierarchy was nevertheless established in the 1950s. Accordingly, research activities labeled as "pure," "free," or "basic" research were given more prominence in public discourse compared to activities defined as "applied" or "goal-oriented" (Trischler 2006: 237). The hierarchy was visible as well in debates about the societal and political role of science, the legal status of science, the organization of research, and the funding policies of government-financed or private foundations. Against the background of the experience with the former fascist regime, the Federal Republic's *Grundgesetz* (GG, Basic Law), that is, the new constitution enacted in 1949, guarantees that "science," specified as "research and teaching," is "free" (GG article 5, paragraph 3). Against this legal backdrop, the debates on independent research mainly revolved around universities and their academic personnel—an exclusiveness that only a few years later was challenged by other institutions of higher education (Dirks 1958: 22–23; Fiege 1959: 87; Bettermann 1963: 69).

The notion of freedom of science held different semantic facets (Wilholt 2012). Legal experts in the late 1950s were pondering whether this article could be interpreted as a government-sanctioned guarantee for the existence of universities—a sort of fundamental law of German universities—as these were seen as the central arena where independent research could be conducted (Köttgen 1959). These experts, as well as other scholars, traced the idea of academic freedom back to the origins of the German university and its independent corporate status in premodern feudal society or, as a much older reference, to Greek philosophy (Wilpert 1953: 268; Köttgen 1959: 16–17; J. Ritter 1965: 17). This invention of a tradition of academic freedom, which mostly neglected the different historical rationales of premodern corporative and modern institutional rights of the universities, made the Nazis' efforts of bringing universities into line with their ideology look like a short and exceptional interlude.

Referring to epistemic reasons, both the scientific community and research-policy experts pled for institutional independence of universities and their teaching staff funded by the German states. In view of the insecure future after the war, researchers wanted to make universities the core of unrestricted, sustainable knowledge production and scientific training. The industrial chemist Walter Reppe, for example, argued that for the sake of progress—he meant both scientific and technological progress—science had to be freed from any practical, political or economic purposes. He warned, "The intellectual foundations of science are in danger! . . . Independent research serves the

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truth, the advancement of our knowledge, and our cognition" (Reppe 1950: 5; see also Rein 1950: 6; Clausen 1964: 35–43).

The idea of freedom of research and teaching was also framed by a pedagogical discourse of higher education, especially with regard to the training of schoolteachers. Providing the grounding for higher education, *Wissenschaft* was meant to be detached from all practical considerations or material goals in order to focus on the cultivation of human understanding (Stein 1947; Müller 1958: 557; Behnke 1959; Baumgarten 1963). In contrast to the epistemic and pedagogical legitimization, the interpretation of freedom of science as a specific variation of the personal freedom of opinion was less common and became only more important in the period of the German student movement in the late 1960s.

Contemporaries in the 1950s considered *freie Forschung* to be an indispensable characteristic of pure science—an attribute that was, of course, also transferred to the concept of basic research in the following years. After the National Socialists had discredited the old ideal of pure science, its revival in the 1950s functioned as a marker for the break with the fascist past. It referred to the heyday of the German university in the nineteenth century. At the same time, the ennobling idea of scientific research for its own sake was now exploited as a democratic symbol by presenting pure science as part of a humanistic worldview and the common good of humankind. Educated elites (*Bildungseliten*) evoked the German neohumanist-idealistic tradition of science, primarily associated with plans for the University of Berlin conceptualized by Wilhelm von Humboldt, who was the Prussian minister of cultural affairs and education in the early nineteenth century. Yet, industrial representatives also supported this ideal of higher education (Kost 1956). The idealization of Humboldt's plans—the so-called "Humboldt myth," which can be traced back to the early twentieth century (Ash 1999; Paletschek 2002)—turned the university into the center of pure, independent science encompassing freedom in research and teaching. Moreover, the Humboldt model included the claim for unity of research and teaching, the goal of general education (*Allgemeinbildung*), and the ideal of the unity of science—that is, the unity of the humanities and the natural sciences (Schwarz 1957). Addressing the future of the German university in 1945, philosopher Karl Jaspers (1946: 18–21, 104–105)[12] retrospectively identified the increasing dominance of a positivist and utility-oriented understanding of science as an academic pitfall.

The literature has already put this notion of the German university in a historically critical perspective—with regard not only to Humboldt's impact on German university tradition but also to the actual implementation of the Humboldt model in postwar Germany. The drawback of the reference to the nineteenth-century university ideal was a restoration of the power held by full

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professors, and this was known in Germany as *Ordinariensystem,* which actually hindered the democratization of German scientific institutions and the early integration of young academics (Clausen 1964: 6; Weisbrod 2002: 20; P. Wagner 2010). Nonetheless, the "Humboldtian" notion of science gained importance in postwar Germany[13] because, first, it denoted science as a cultural asset and, second, it again tied science closely together with education in order to transform higher education into a school of democracy and a bulwark against fascism (Baumgarten 1963; Holzapfel 1963). The idea of *Allgemeinbildung,* implemented with formats such as *Studium Generale* (Wolbring 2014: 309–321), also applied to some extend to *Technische Hochschulen,* which presented a culturally defined understanding of technology as their new leitmotif (Schmidt 2014: 82–84).

In a similar vein, scientists and scholars claimed that pure scientific research always had to be based on higher moral principles and philosophical deliberations (Jaspers 1949: 11). Hence, scientist often used the concept of pure science when they were reflecting ethical aspects of research and the philosophical problem of objectivity and truth (Hennemann 1947: 10; Schwarz 1957: 26).[14] German natural scientists complained that ever since materialism gained importance in the second half of the nineteenth century, "pure" empirical sciences has been constantly drifting apart from the humanities (Hartmann 1955: esp. 35–36). According to the biologist Max Hartmann (1953: 614), former director of the Kaiser Wilhlem Institute (KWI) for Biology during the Nazi period, only a combination of "ardent eros of knowledge" and "cold, clear logos," together with a "moral sense of responsibility," could restore the freedom of science. Although most scientists and scholars avoided referring explicitly to examples of scientific contributions to Nazis' warfare and racist policies, this debate must be seen against the background of the fascist past.[15] An exception was the physician and representative of early psychosomatic medicine Viktor von Weizsäcker. Weizsäcker, whose role during the Nazi period is still being discussed controversially (Roth 1986; Jürgen 1996), dealt explicitly with Nazi eugenics in medical research. He pointed to the destructive effects of scientific applications in the service of war and the Holocaust and warned that pure science and its belief in objectivity had become detached from humanitarianism (Weizsäcker 1948: 14–15).

This discourse on the problems of pure science took a slightly different turn in the beginning of the Cold War, when the threat of a nuclear war became a new negative symbol for what science can do. Whereas natural scientists and philosophers pointed to the fallacy of pure science, sociologists perceived pure and independent science to be at risk due to increasing industrial and military demand for research. Social scientists identified the modern political and economic powers as a challenge that corrupted the core

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rationales of science and its productivity (e.g., F. Wagner 1964: 86–91). Confronted with the consequences of atomic research, physicists such as Carl Friedrich von Weizsäcker and Werner Heisenberg, leading German scientists who had been part of the uranium project during the war, drew another conclusion from the ambivalence of modern science. They stuck to the difference between science and technology—that is, pure science and its applications: "The effects of science on our world are largely effects of technology, which have been brought about by science" (Heimendahl and Weizsäcker 1965: 10). However, Weizsäcker did not use this argument as a relief strategy for scientists. Given the inevitable ambivalence of modern science, physicists called for political action; specifically, scientists should assume responsibility, get involved in politics, and use their scientific expertise to inform the public about risks of modern technology (Howe 1959; Heimendahl and Heisenberg 1965: 52–53).[16]

At the same time, not all scientists discussed the ideal of pure science in this differentiated way. Under the auspices of denazification, it became important for German scientists to distance themselves from the fascist past. At this point, the purity metaphor and the distinction between pure science and its applications turned into a professional strategy to set aside any political or moral responsibility. For the special case of mathematics, Herbert Mehrtens pointed to the semantic flexibility of "pure," which functioned both as a strategy of scientific autonomy and as a label for moral and political innocence of mathematicians (Mehrtens 1994). In most of the documents and self-testimonies whitewashing German scientists after 1945 (so-called *Persilscheine*), semantic variations of the argument that scientists followed their pure devotion to the advancement of scientific knowledge played a crucial role (Proctor 2000: 10–11; Heim 2002; Sachse 2002).

Moreover, the notions of pure or true science were still important for the professional identity of natural scientists. They were quite common in proceedings and addresses on the occasion of academic festivities or inaugurations of new institutes. Most of these speeches entailed retrospectives on German science before the world wars, portrayed as a time when pure devotion for the advancement of scientific knowledge was still possible (F. Becker 1950: 529). When Lise Meitner (1964), for example, remembered the beginning of the KWI for Chemistry before World War I, she was missing "the happy time of pure science, when scientists had not to think of the dangerous applications of their scientific outcome." Especially the reestablishment in 1950 of the Gesellschaft Deutscher Naturforscher und Ärzte (Society for German Naturalists and Medical Doctors)—the most famous and influential German science association in the nineteenth century—can be seen as a symbolic act of evoking the ethos of pure science, the beginning of scientific

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professionalization, and the unity of natural scientists (Degen 1954, 1956; Pfannenstiel 1958: 1–2). In obituaries and laudations, the attribute "pure" kept on being an essential marker for scientific accomplishments and the individuals' contributions to scientific progress (Laue 1951: 515). Accordingly, a "born scientist" was characterized by his or her "pure quest for knowledge" (Henke 1955: 193).

All in all, the attributes "pure" and "independent" were entwined in different ways. Yet in order to work as a democratic symbol, the resulting concept of science had to be backed up by the commitment to a humanistic worldview. Hence, against the historical background of the Nazi regime, the image of science being independent from political and other external influences was generally important for all nonprofit research institutes—not only for universities. With the beginning of the Cold War, however, the democratic underpinning of science also became vital for the political boundary work of the Federal Republic as well. Because of the competition between the two political systems, West German policy makers and experts started to put the G.D.R.'s science policy under scrutiny (Lange 1955). Heinrich Kost (1956: 26–27), a leading representative of industry, business, and trade associations, thus turned the ideal of freedom of science into an ideological argument for the necessity of protecting Western culture from Communism:

> There is no doubt: striving exclusively for short-term material benefits inadvertently undermines our Western model of civilization. There is only one force that will be pleased by such a development: bolshevism, which puts an end to the freedom of science as much as it abolishes the freedom of economic activity. . . . Against this background, the voluntary promotion of scientific research, teaching and training appears to be the ideal instrument for protecting the freedom and independence of science, which we know to be no less vital for our civilization than the protection of private property, economic or personal freedom.

U.S. science policy experts visiting Germany after the war enforced this Western perspective on Eastern research policy by contradicting the communist argument that "pure science" was a "myth of the bourgeoisie, a legend of capitalism" (Conant 1953: 7). The U.S. democratic understanding of science was also backed by revived nineteenth-century scientific ideals of freedom and universal humanism (Bender 1997: 4–5). Whereas in the United States, the writings of Robert K. Merton (1942) and Michael Polanyi (1962) were prominent references in this ideological debate, in West Germany, Karl Popper's *The Open Society and Its Enemies* became an influential scholarly reference. The book of the British scholar, who had emigrated from Austria in 1937, was published in 1944 and translated into German in 1956/57. According to Pop-

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per, no form of "closed societies" could ever breed a critical, and therefore true, knowledge-aspiring science (Popper 1958: 275–319; see also Hailsham 1961).

## Planning for a Prosperous Western Democracy

Notwithstanding the purity talk, after 1950 it was the concept of "basic research" that became more and more popular. In contrast to the Nazi period, in which it was mainly used as a counter concept to pure science, after 1950, "basic research" absorbed the attributes of, and norms and ideals associated with, pure or independent science. This also applied to the ideological dimension according to which a "free society" must promote "basic research" because otherwise it would risk becoming "primitive" and prone to materialist-communist tendencies (Strugger 1956: 5). At the beginning, however, scientists seemed to be puzzled by this semantic transfer. Some preferred conceptual hybrids such as "free and pure so-called basic research" in order to make sure that exactly these attributes were included (Reppe 1950: 7; similarly, e.g., Holdermann 1949: 161; Bianca 1950: 2).

Only few scientists in the immediate postwar era refused to follow this semantic shift. August Thienemann (1949), a zoologist and member of the Academy Leopoldina, strictly rejected the concept of basic research:

> It is supposed to lay the basis—for what? . . . It is contrary to the German spirit to degrade science in its entirety to the role of maidservant of practice. That is why the term "basic research" has to disappear, and the general public needs to be reminded over and over again of the value of theoretical, pure science for its own sake. This is particularly important in times of hardship, when it seems so obvious to put material considerations above intellectual concerns.

It was, however, the metaphorical use of basic research that actually did the trick for communicating to the public the need for government support of science. The advantage of talking about "basic research" was that one could promise the advancement of scientific knowledge as well as economic and societal progress. In line with the argumentation that is known from U.S. science policy discourse following the so-called Bush Report, "basic research" was supposed to lay the ultimate ground for technological innovations and national welfare. The idea, of course, was not new. The assumption rested upon the nineteenth-century idea that the discovery of only a few laws of nature would provide plenty of applications in the future. A slogan widely used in the 1950s expressed this expectation: "die Forschung von heute ist der Fortschritt von morgen"—today's research will be tomorrow's progress (Gross 1955: 34; Gerlach 1956: 31; Hess 1962: 31). Even natural scientists

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who particularly clung to the ideal that science should be performed only for its own sake, such as the chemist Otto Hahn (1949, 1954), felt the pressure to promote academic research by promising technological or economic progress. The reference to the idea of laws of nature in this context is surprising since this strict understanding of causality had already lost its epistemic relevance in the wake of quantum theory (Engler 2010). Even more, the rhetorical power of the revived idea of scientific laws stresses once again how older attributes of science were adapted to contemporary expectations toward academic research after World War II.

The popularity of basic research in the Federal Republic of Germany was partly due to the promotion tour by prominent U.S. scientists and policy advisers. For example, the former president of Harvard University, James B. Conant, came to Germany in 1953 as a high commissioner and argued for the necessity of fostering basic research and, therewith, for the support of autonomous research as an essential contribution to industrial progress (Conant 1953: esp. 7). The Stifterverband, which remained an important fundraiser for scientific research until the 1960s and an intermediary between science and the economy,[17] published translations of such speeches and texts written by American scientists. Next to Conant, the most prominent author was presidential science advisor Vannevar Bush, who famously had been among the first to call for federal funding of basic research. His slogan "The research of today and the world of tomorrow"—translated as "Die Forschung von heute und die Welt von morgen" (Bush 1954)—was echoed many times in the Federal Republic. This plea meant more than lip service; the United States actually financed a lot of research in Western Europe. John Krige argues that this basic-research mission played a key role in reconstructing European science under "American hegemony" for two reasons. First, the concept was important for communicating the U.S. financial support for the former wartime enemies to the American public without raising concerns. Second, the United States promoted basic research as unclassified research in the allied countries to increase its stock of scientific knowledge and thus to secure American technological leadership (Krige 2006).

The call for more basic research was mainly driven by worries that research that cannot promise immediate applicable results would be neglected. This argument was partly taken over from the U.S. debate and partly derived from the German discourse on catching up with the leading nations of the West (Arnold 1956). Overall, the promotion of basic research aimed primarily at research in the natural sciences executed at universities. According to the research experts, "basic research" should have been reserved largely for universities and *Hochschulen* and considered as "a sort of stockpiling" for economic prosperity and national welfare (Reppe 1950: 6; see also Klar 1959: 77).

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In the 1950s, experts were actually concerned about the sufficient funding of German universities and, as a consequence, their autonomous status (Heppe 1956). In the end, it became an international consensus of the OECD nations that universities are centers of basic research (King 1964: 4).

In the Federal Republic, the central funding organization for academic and independent research was the Deutsche Forschungsgemeinschaft (DFG). It was reestablished in 1949 and operated until 1951 under the name of its predecessor, the Notgemeinschaft der Deutschen Wissenschaft, and was mainly financed by both the federal government and the German *Länder*, with additional support of the Stifterverband. Although public debates on research at universities were closely linked with the ideal of basic research, the DFG mostly avoided speaking of basic and applied research in the 1950s and 1960s. Moreover, the DFG clarified that it supported not only both the natural sciences and the humanities but also "pure and applied research" (DFG 1955: 10).[18] The statutes defined the purpose of the association as a service for "science in all its branches" (DFG 1961b: 214). In the 1950s, the DFG obviously felt compelled to defend its funding policy and argued that the American plea for more basic research had to been seen against the backdrop that the United States spent 90 percent of its governmental research budget for "applied research" and thus needed to increase the stock of knowledge in the natural sciences (DFG 1955: 10–11). While the United States had its governmental research institutes, according to the DFG, Germany had none. However, the DFG adopted the metaphor of "basic," at least, in some way: "It [the DFG] supports all branches of research and thus lays the scientific fundaments for required public provisions serving the common welfare." Here, the freedom of science, guaranteed by the Basic Law, was legitimized by the service for the common good, which is another facet of the meanings of academic freedom (DFG 1961a: 12, 13).

The DFG failed to explicitly define its understanding of "applied research." The overviews of the spending in the 1950s placed the agricultural and technical sciences (*Technik*)—that is, fields that could be defined as applied sciences in the traditional sense—as additional categories beside the natural sciences, the humanities, and medicine. With regard to agricultural sciences, a report of 1951 explicated that the Notgemeinschaft der Deutschen Wissenschaft (1951: 27) had so far funded only basic research, whereas applied research projects were meant to be funded by the Ministry for Agriculture. In general, the natural sciences got the largest share of the total DFG funding budget—48 percent in 1955, whereas technical research fields got only 12 percent in the same year (DFG 1957: 35). In 1953/54, the DFG installed a Committee for Applied Research to improve support for technical and agricultural research. In this context, the DFG explained that it had fo-

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cused on basic research so far because applied research had been expected to attract more financial support from industries. The justification of the new committee reveals, in other words, that academic research in the natural sciences was conceived as synonymous with basic research (DFG 1957: 58–59).

After all, there are two reasons why the DFG did not feature basic research as a pivotal funding rationale in the first two decades after the war. First, the funding organization was responsible for supporting research at both the universities and the *Technische Hochschulen*. Second, the distinction between basic and applied research had not been relevant to the humanities, the social sciences, economics, or medicine and was only rarely used in these areas by then (DFG 1957: 58). In self-descriptions of the DFG, the concept of basic research became more common only in the 1970s. According to the report of 1974, the funding rationale focused on "basic research and research close to application [*anwendungsnahe Forschung*] carried out at institutes of higher education [*Hochschulen*]" (DFG 1975: 9). Since the economic crisis of the early 1970s had resulted in budget cuts, it became harder to justify spending on academic research. In contrast to former reports that praised the principles of independent open science, the aspect of research planning according to economic criteria gained importance. It seems that this was the time when the idea of basic research as the first stage in a linear process of innovation started to take center stage in the DFG's efforts to communicate research policy to the public (DFG 1972: 17).

Along with the universities, the Max-Planck-Gesellschaft (MPG, Max Planck Society) was considered a privileged venue for basic research. In its statutes from 1949, the MPG described itself as an association of independent research institutes (*freie Forschungsinstitute*) and emphasized its autonomy from the state and industry. As a self-governing body, financed mainly by public funds, its autonomous status was guaranteed by the Basic Law and meant institutional autonomy as well as the autonomy of the individual scientist. Although the ideal of basic research was not recorded in the statutes, it turned into a pivotal marker for the scientific identity of the MPG.[19] In the 1950s, scientists of the MPG, such as Nobel laureates Otto Hahn (1953) and Erich Regener,[20] stressed their focus on autonomous basic research whenever they wanted to emphasize their independence from politics and the economy while at the same time promising future economic benefits. It was exactly this semantic shift after 1945 that made it possible to invent a continuous tradition of independent, pure "basic research" from the Kaiser Wilhelm Society to its reformation as the Max Planck Society (Biermann 1961). Due to this rhetorical capacity, the label of basic research fit also those MPG institutes that were engaged in research fields close to technical applications, such as the Max-Planck Institute for Metal Research. In return, it worked equally

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when the institutes of the MPG applied for funding from the European Recovery Program (ERP), which explicitly aimed at projects promising short- or medium-term applications. The Max Planck Institute for Chemistry, for instance, ensured that its work would offer a broad range of opportunities for application in the future, in particular with respect to "biomedical, agricultural, and heavy-industrial chemical areas of research."[21] It received a generous sum from the ERP,[22] even though its scientists never pushed its research toward these fields or other more application-oriented ends (Reinhardt 2012).

## The Emancipation of Applied Research and Engineering Sciences

When German scientists and policy makers were talking about basic and applied research after 1945, it became a habit to emphasize that it was hard to draw a clear line between the two of them. There was, however, also more serious critiques with regard to the implicated superiority of basic research over applied research. To begin with, the purity discourse on science had moral implications. As it has already been observed for the United States, the label "pure" conveyed evaluative elements that suggested that applied or goal-oriented research was in some way "impure" (Kline 1995: 217; Kaldewey 2013: 360). In the same line, some German researchers pointed to the "emotional tone the term 'pure' implicates." In their view, applied science was neither comparable with contract research nor less scientific than basic research (Wenke 1957: 44, see also 43).

Especially the engineering sciences felt offended by the increasing requests for a privileged support of basic research. For example, in 1950, Richard Vieweg, who served as president of the Physikalisch-Technische Bundesanstalt (Federal Physical-Technical Institute) and chairperson of the Deutscher Verband Technisch-Wissenschaftlicher Vereine (German Union of Technical-Scientific Associations), problematized the insecure position of applied or engineering sciences within academia. In particular, he worried about their chances of getting public grants while competing with the basic sciences—that is, the natural sciences (Vieweg 1950: 731–732; Sörensen 1952: 158). Vieweg criticized the partitioning of basic and applied research that was pushed by several parties as not only being motivated by the desire to classify the two but also implying favor of the former over the latter (Vieweg 1955). With reference to the unity of science, symbolized by the tree of knowledge, critics of the ideal of basic research claimed that basic and applied research benefited from each other. They argued that technology and applied sciences opened new scientific horizons far beyond the knowledge required for solving the original technical problem. In this sense, applied research could stimulate basic research (Houdremont 1953: 39–40).

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Another challenge came from the industrial researchers, who did not accept the common assumption that basic research was restricted to the academic sphere. Instead, they pointed to the efforts industry had made so far in basic research and explained how basic and applied research intertwined within industrial laboratories (Houdremont 1953; Steimel 1963). Besides, these experts knew that investment in research could increase the prestige and reputation of companies; as one industrial representative put it, "Research is the golden framing of a company's business card" (Steimel 1963: 9). After all, the public funding of science at universities was not against the interests of industry, since it secured the training of future industrial researchers and spared them from doing risky research with unpredictable outcomes.

The criticism of the basic/applied distinction finally left its mark on the nomenclature of research policy. With the creation of compounds such as *angewandte Grundlagenforschung,* engineers tried to prevent the neglect of applied sciences (Heiss 1950, 121; Wever 1952: 1053). Later, the Stifterverband mentioned the semantic variation *anwendungsorientierte Grundlagenforschung,* defined as basic research that is inspired by its practical relevance, in its list of established concepts of research policy (Stifterverband für die Deutsche Wissenschaft 1967). In the end, such intermediate categories did not become as common as the antonymic terms "basic and applied research." Notwithstanding the success of the basic research narrative, the criticism at least activated a debate on how to improve the support for applied research. Already in 1949, the Fraunhofer Gesellschaft zur Förderung der angewandten Gesellschaft (FhG, Fraunhofer Society for the Promotion of Applied Research) was founded. However, the goal of this new organization and, as a consequence, its legitimation remained unclear until the mid-1950s (Trischler and vom Bruch 1999: 38–39). It thus took a while until the FhG, focusing on contract research for industry and public administration, advanced to being the biggest German organization for applied research.

Another attempt to increase the promotion of applied research was made by the DFG. In 1953/54, a permanent Ausschuss für angewandte Forschung (AfaF, Committee for Applied Research) was established and run by representatives from the *Technische Hochschulen* and industry, as well as ministry officials (Lax 2015: 163–79). The AfaF played an important role until well into the 1960s. At the beginning, the main goal of the AfaF was to take stock of the different fields of applied research and to assess which areas were in need of support. The report pointed to specific fields, such as shipping and aviation, and highlighted the MPG as an example of excellent cooperation between industry and research institutions (DFG 1956: 9). In 1957, the AfaF became an advisor for the newly established Wissenschaftsrat (German Research Council). Overall, the AfaF was meant to coordinate industrial requests

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for research and agendas of academic research. The first AfaF memorandum defined "applied research" as a combination of basic research seeking to understand the laws of nature (*Naturerkenntnis*) and technical research leading up to product development (DFG 1956: 7). In preparation for the AfaF, Kurt Zierold, general secretary of the DFG, pointed to a U.S. funding classification according to which "pure research" and "fundamental applied research" were meant to be funded by the government, while "applied research" and "development" should be financed by both industry and government.[23] By the late 1970s, the committee no longer met regularly and lost its relevance. It seems that the status of applied research was no longer controversial and its funding was secured.[24]

Overall, the ideal of pure, independent research in the immediate aftermath of World War II was challenged by the necessity of economic recovery within the Western alliance. Even proponents of the ideal knew that compromises had to be made between granting the greatest possible autonomy in the allocation of resources and the individual choice of research topics on the one hand, and the response to societal or economic needs on the other hand (Raiser 1950: 3; Tellenbach 1954: 11). As a result, German science faced a process of reorganization that reached its peak at the end of the 1960s. The awareness that even autonomous institutions were in need of planning led the DFG to launch new funding formats, such as interdisciplinary, collaborative research programs (*Sonderforschungsbereiche*)[25] (Orth 2011: 96–238). In parallel, the West German government and industry started with big science projects, as other states had done before (G. Ritter 1992; Mutert 2000). In this phase of growing research planning, the imagined relationship between basic and applied research changed to some extent. Against the background of the economic crisis in the 1970s, scholars from the Max Planck Institute in Starnberg suggested that advanced research fields of basic research should turn to more application-oriented research to speed up the knowledge transfer from basic to applied research—an idea that came to be known as the "finalization thesis" (Böhme, van den Daele, and Krohn 1973). Yet, in the end, such suggestions indicated the persistent belief in basic research providing the ultimate ground for all kinds of technological innovations. Moreover, the concepts of basic and applied research definitely took hold because they proved to be relevant for the communication of research planning.

## Conclusion

What is special about the way science was categorized during the changing German science policy regimes? At first sight, the German scientific community in the second half of the twentieth century embraced the basic research

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ideal as enthusiastically as it embraced the pure science ideal in the nineteenth century. Still, while the ideal of pure science gained prominence quite early in the nineteenth century, the concept of basic research received ambivalent responses in the 1950s and 1960s and prevailed only later as the official funding rationale. Both phenomena point to the specificities of German academia.

To begin with, when natural scientists entered German universities in the first half of the nineteenth century, they adopted the predominant philosophical ideal of pure science and turned it into a successful strategy of professionalization and boundary work facing the upcoming engineering sciences. In Britain as well as in the United States, in comparison, the idealization of pure science gained importance not before the 1870s. Although all idealistic notions of pure science had in common that their promoters turned technical inventions retrospectively into success stories of science (Gooday 2012), it was the epistemic underpinning that characterized the German distinction between pure and applied research. In contrast to the United States, the German understanding of pure science was less morally charged (Lucier 2012). Yet it still implied a stricter hierarchy between pure and applied science than in England (Bud 2014).

In the early twentieth century, the ideal of performing science for its own sake was challenged by the rising societal expectations in view of industrial applications of scientific results. Governments realized that science policy was more than a higher education policy and thus developed an interest in a research policy aligned with national economic interests. Hence we witness a period of semantic transition, in which new concepts emerged. In the United Kingdom, ministry officials used the neologism "fundamental research" to convince scientists to cooperate with industry, and, in turn, to persuade companies to build up their own research laboratories (Clarke 2010). In the United States, the new term "basic research" came up in the context of agricultural research and debates about its public funding (Pielke 2012: 340). In Germany, both government and industry gave money to found new independent research institutes run by the KWG. Here, the cooperation between academic science and industry worked successfully without compromising the scientific reputation of its leading scientists. Furthermore, facing the financial crisis after the World War I, Germany established a national research foundation. Although contemporaries still considered science a cultural value in itself in the 1920s, the concept of pure science did not suffice to communicate the coordination of different institutions performing research. Under the conceptual umbrella of "collaborative research," German policy makers managed to bring together scientific, economic, and political interests to overcome the national crisis while respecting the academic ideals of the unity of science and the individual autonomy of professors at the same time.

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It was not until the late 1930s that the new concept of basic research became more popular in German research policy. The National Socialist Party, which had discredited the ideal of pure science as a selfish project of the bourgeoisie that had estranged academia from German society, used the term "basic research" to describe research projects that were most promising for furthering their war plans and their idea of economic autarky. The concept enabled policy makers and scientists to communicate both the promise that science would lay the cornerstone for technological progress, and the inherent uncertainty of the scientific venture into the unknown.

In the immediate aftermath of World War II, the public debate on science showed a good deal of semantic pluralism. The older concept of pure science was revived and primarily used in a moral sense, either with a critical stance when scientists were deliberating the ethical or epistemic hubris of pure science or in a self-defending way for distancing themselves from the fascist past. Next to pure science, the attribute of independency became even more important because it turned science into a symbol for the democratic transformation of West Germany. Universities and the MPG emphasized their independence from any political or economic interests. With the invention of neohumanistic traditions, freedom of science became a powerful emblem especially for German universities. Under the auspices of democratization in the 1950s, science became more tightly coupled to higher education again. Against the background of the fascist past, the coupling of science policy and education may have been stronger than in other Western nations.

In the 1950s, the concept of basic research became more and more popular because it had taken a semantic twist. While in the Nazi period the term worked as a counter concept to pure science, after 1945 it actually absorbed the ascriptions of pure, independent science. In the Federal Republic, scientists, politicians, and industry shared the belief that it is important to support basic research at universities. They followed the U.S. promotion of the ideal of basic research among the Western allies. The backing of basic research in the natural sciences promised economic benefits and the advancement of scientific knowledge at the same time. The unrestricted production of new knowledge was said to be a kind of economic savings account. Herein lay the rhetorical power of the concept. In contrast to the United States, the concept did not determine the official nomenclature of the reestablished German Research Foundation. In the United States, where the launch of the National Science Foundation in 1950 was a novelty, the concept of basic research functioned as a political symbol capable of integrating different political camps and interest groups (Pielke 2012). In Germany, in contrast, government funding of science had a long tradition. Furthermore, contrary to the NSF, the DFG was in charge of supporting all branches of science, including the hu-

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manities, medicine, law, and economics, as well as the applied sciences, right from the start. In the context of the DFG, “basic research” first and foremost denoted academic research in the natural sciences. Yet since the bigger part of the DFG’s spending went to the natural sciences, basic research can be seen at least as an unofficial funding rationale. Besides, applied research in the engineering sciences proved to be less dependent on public funding. Only from the late 1960s onward, when the international conceptual synchronization within OECD nations took place (Godin 2005) and the Federal Republic’s government began to do research planning on a broader scale, the distinction between basic and applied research finally prevailed as the official funding rationale.

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## Notes

1. For the different developments of research policy and the key concepts after 1945 in the Communist German Democratic Republic, see chapter 6.
2. All original German quotes have been translated by the authors.
3. Since there is no translation that really matches the meaning of *Technische Hochschulen,* we use the German term instead of less proper translations such as technical colleges or technical universities.

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4. For a more differentiated analysis of the intellectual debates on science and technology during the Weimar Republic, see Panoutsopoulos 2014.
5. The concept of collaborative work emerged at the turn of the century in socioeconomic and pedagogical debates. First mentions with clear references to research defined "collaborative work" as international cooperations in the technical sciences with regard to the testing of material and standardizations of measurements: Exner 1910. The concept seemed to keep its specific relevance for the technical sciences in the following years. According to Karl-Heinz Ludwig (1979), engineers elevated it to a professional ideology during the Nazi period.
6. The RFR was responsible for funding research. During the war, it answered directly to the Army Ordnance Office (Flachowsky 2008: 232–462).
7. The German version of fundamental or basic research had already emerged in the early twentieth century. Before 1937, the use of the term, however, was mainly restricted to the discipline of mathematics and debates on epistemology. Here, the term primarily referred to research on fundamental problems in mathematics (Schauz 2014: 286).
8. *Illustrierte Zeitung,* Leipzig, No. 4956, 22 August 1940. Established in 1843, the *Leipziger Illustrierte* was one of the oldest magazines in Germany and well known for its high-quality prints. In comparison to the very popular *Berliner Illustrierte,* the magazine from Leipzig addressed primarily readers of the middle class and ones with a higher level of education. During the Nazi period, the *Leipziger Illustrierte Zeitung* turned into an unofficial propaganda medium of the National Socialist Party, like many other magazines in this period. For the role of magazines in the Nazi period, see P. Rössler 2010.
9. The history of both the KWG and the German Research Foundation was the subject of two major research projects. See the wealth of research published in the academic series *Beiträge zur Geschichte der deutschen Forschungsgemeinschaft* (Stuttgart: Franz Steiner Verlag) and *Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus* (Göttingen: Wallstein).
10. Kontrollratsgesetz no. 25 from 29 April 1946; Militärregierungsgesetz no. 23 from 12 September 1949. The exact German terms deployed in these two acts were *grundlegende wissenschaftliche Forschung* (fundamental scientific research) and *angewandte wissenschaftliche Forschung* (applied scientific research). The regulations defined military relevance on the basis of several classified research fields. For more details of how the allies controlled research, see Heinemann 2001.
11. According to the database of Google books and its statistical tool, Google Ngram, the use of basic research nearly doubled between the 1950s and 1960s. Alternative concepts, first and foremost pure science—taking all possible inflections of *reine Wissenschaft* into account—were stagnating (period 1945–1960, smoothing 3, German corpus, inflection search). This comparison of how the different concepts diffused does not include concepts such as pure chemistry, which worked together with the distinctive attribute of applied as a label for different subdisciplines.

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12. It is most revealing that Jaspers' speech was a new version of an older one he had given in 1923; fifteen years later, he presented an updated version of the same speech (Jaspers 1923; Jaspers and Rossmann 1961).
13. The reference to Humboldt also applied to the G.D.R. Following the Soviet policy by shifting research from universities to the academy of science, however, the G.D.R. abandoned the goal of tying together research and teaching (Paletschek 2002: 200–201).
14. Within the epistemic discourse on theory of science, the concepts of *reine Wissenschaft* and *reine Naturwissenschaften* were and still are primarily used as a reference to Kant.
15. Despite the growing awareness of the dilemmas of modern science, in most cases the Nazi period was defined as an exception, a period of *Befehlsforschung* (commanded research) in which science had been abused (Wenke 1957: 44).
16. For more detailed studies on the political and ethical statements of German nuclear physicists, see Kraus 2001 and Carson 2010.
17. See, for example, the list of donations in Stifterverband für die Deutsche Wissenschaft 1955: 152–153.
18. In the second edition, it was "basic and applied research" (DFG 1957: 8).
19. For a more detailed analysis of the policy of basic research within the MPG, see chapter 5.
20. Archive of the History of the Max Planck Society (MPA, Archiv zur Geschichte der Max-Planck-Gesellschaft), Sect. I, Rep. L 15/-SV, 1949–1968, Map Vademecum: Regener to the MPG, 23 August 1952 and Regener to Telschow, 29 April 1952.
21. MPA, Sect. II, Rep. 1A ID9, folder 2, Marshall-Plan, 1950–1953, General: Declaration of the Max-Planck-Institutes for Chemistry in Mainz, 23 May 1951.
22. MPA, Sect. II, Rep. 1A ID9, folder 2, Marshall-Plan 1950–1953, general and second tranche: List of ERP-funds for institutes of the MPG.
23. Archive of the DFG Office, DFG Committee for Applied Research, 6210 (founding documents), meeting on 23 January 1954.
24. Walter Pietrusziak (Archive of the DFG office) to Gregor Lax, e-mail, 5 July 2013. The AfaF files documenting the late 1970s are not yet accessible.
25. This funding format still applies to all disciplines; it is not restricted to the natural or engineering sciences. The most important collaborative actors were and still are the universities. Cooperation with other research organizations like the MPG or the FhG is also possible. The debates on interdisciplinary research and teaching formats started already in the late 1950s (Schregel 2016).

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Chapter 3

# Transforming Pure Science into Basic Research

## The Language of Science Policy in the United States

*David Kaldewey and Désirée Schauz*

Between the seventeenth and early nineteenth century, various European scholars introduced a distinction between a pure and a practical or applied part of their respective disciplines (Kaldewey 2013: 328–330). Differentiations between "pure" and "mixed" mathematics, for example, were elaborated by the Flemish mathematician Adriaan van Roomen in 1602, by the English philosopher Francis Bacon in 1605, and, later, by the German philosopher Christian Wolff in 1716. While those distinctions had their roots in the ancient concepts of theory and practice, in 1751 the Swedish chemist Johan Gottschalk Wallerius developed a modern division of pure and applied chemistry that avoided the connotations of the older distinction, because both pure and applied chemistry included theoretical as well as practical work (Meinel 1983). In the late eighteenth century, Immanuel Kant granted the natural sciences the status of "pure science" in general because they were based on "a priori" conceptualizations of nature such as assumptions on causes and effects (see also chapter 2), and in 1817 the British polymath Samuel Taylor Coleridge, inspired by Kant's transcendental philosophy, introduced the terms "pure sciences" and "applied sciences" to the English language (Link 1948; Yeo 1991; see also chapter 1). Given these various categorizations and their heterogeneous cultural backgrounds, it does not make sense to assign priority to any one term's origin. It is evident nevertheless that debates about such terminologies, as well as the associated strategic identity work, took place in those European nations forming the core of the developing modern science

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system. Against this background, the question we address in this chapter is whether and how American scientists in the nineteenth and twentieth century built on this European tradition. The question is important not least because it is well known that, after World War II, it was scientists and policy makers from the United States who were most influential in establishing the distinction between basic and applied research in science policy regimes around the globe. It appears, therefore, that the global language of science policy was first developed in Europe, but then, in the course of the twentieth century, became coupled to the rise of the United States as the leading science nation.

It is, however, too simple to assume that in the beginning U.S. scientists and engineers merely adopted European semantics. While at first glance there are no original American concepts of science or research before World War I, a closer look reveals that a "pure science ideal"—or, in a more critical reading, a "pure science ideology"—emerged between the 1850s and 1880s (albeit slowly) that was not identical with European notions of pure science. In the late nineteenth century, this ideal was so strong that conceptions of "technology" and "engineering" were commonly construed as "applied science," thus leaning on the positive connotations of "pure science"—that is, the scientific spirit and rigorous methodological standards (Kline 1995; Forman 2007). Around 1900, the United States thus firmly had established a distinction between pure and applied science (Lucier 2012). The pervasiveness of this distinction in public and scientific communication at that time, however, disguises the subtle differences between European and American categorizations, as well as the fact that the meanings and values associated with the terms "pure" and "applied" were still far from being stable.

The American pure science ideal in its most famous articulations—the mainstream reference is physicist Henry Rowland's "A Plea for Pure Science" from 1883—was formulated against utilitarian research ideals and professional scientists who were more interested in business than scientific explorations (Daniels 1967; Hounshell 1980). The idea of pure science, however, was never truly devoid of utilitarian references. Rather, pure science had from the very start been characterized as the origin for future practical applications—an argument in place as early as the 1830s in Britain and which may be summarized as "postdated utilitarianism" (Shapin 2008: 43). Meanwhile, roughly between 1900 and 1940, several new research ideals and practices emerged that gradually transformed the semantic field for categorizing scientific endeavors: new categories such as "industrial research" or "fundamental research" emerged, transcending the boundaries between pure and applied science. It thus became more common to talk about "research" instead of "science" (Godin and Schauz 2016; Kaldewey and Schauz 2017). Finally, in the 1930s and 1940s, these various semantic developments began to merge

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into "basic research," a new concept that became predominant after 1945 (Pielke 2012; Schauz 2014).

In short, the role and reputation of American science in relation to Europe changed radically between 1850 and 1960. The historiography of American science has extensively debated and reflected upon this transformation. The starting point for such debates was the notorious verdict of Alexis de Tocqueville ([1840] 2010: 775–788), who stated that "Americans are more attached to the application of the sciences than to the theory." The narrative that American scientists in the nineteenth century and beyond draw on the scientific output from the Old World was reiterated until the mid-twentieth century (e.g., Shryock 1948), presuming "that there was no American science worth speaking about before 1945, let alone as far back as 1880" (Kevles, Sturchio, and Carroll 1980: 27). This picture became more nuanced in the 1970s, after several historians pointed to nineteenth-century America's genuine contributions to diverse fields in the natural sciences (Reingold 1964, 1972, 1976, 1979; Daniels 1972; Cohen 1976; Kevles [1977] 1995). Moreover, the history of industrial research in the first half of the twentieth century reveals a specific American style of organized research (Reich 1985; Wise 1985; Dennis 1987; Hounshell and Smith 1988; Hounshell 1996; Shapin 2008). While this reassessment of the United States' history of science concerns the actual progress of scientific knowledge production, in this chapter we ask how scientists and other stakeholders in the nineteenth and early twentieth centuries communicated the means and ends of scientific research in the United States. The fact that similar terms (e.g., "abstract science," "pure science," and "applied science") were deployed in the old and the new world does not mean that their related meanings and rhetoric were synonymous.

After 1945, the United States became not only the key player in the global repositioning of science policy, but also the source of semantic innovations: since then, Europeans have been copying key concepts and ideas from American discourse on science policy. The most successful "export" product is likely the category of basic research, which gained a specific meaning in the Bush Report and was a tremendous influence on science policies around the world. Yet even after the heyday of the ideal of basic research in the 1960s, the United States has remained a prime exporter of science policy concepts, as can be seen in the more recent discourse about "grand challenges" or in the metaphorical notion of "frontier research" (see final chapter). An entire body of literature discusses the new "social contract for science," as well as the establishment of U.S. federal science policy in the second half of the twentieth century (e.g., Guston and Keniston 1994; Sarewitz 1996; Elzinga 2012). Yet such literature focuses far less on the semantic shift leading to the new key concepts of basic and applied research and their multiple meanings and discursive functions.

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We reconstruct the history of changing science and research categorizations in the United States from the 1840s to the 1960s in four steps. In the first section, we look at how U.S. scientists built upon semantic traditions from Europe, most commonly upon the distinction between "abstract" and "practical" sciences. We then trace the ideal of "pure science" and its transformations during the second half of the nineteenth century. The third section concentrates on the new categories of industrial, fundamental, and basic research that appeared at the dawn of World War I. The fourth and final part of this chapter analyses how "basic research" prevailed over former concepts and functioned as a contested but successful boundary concept that could deal with conflicting demands upon science in Cold War America.

## Semantic Heritage: Abstract and Practical Sciences

In 1815, the well-rounded man David Humphreys—a colonel during the Revolutionary War, an American ambassador to Portugal and Spain, confidant to George Washington, entrepreneur, and, finally, a poet and author—published a drama playing upon the cultural differences between Americans and Englishmen. Humphreys argued that there was not as great a difference as might be expected between "educated men in Europe and America" because they studied the same classical authors. On the other hand, he held that Americans were more prone to practical than contemplative activities: "But Americans in general, and more especially in the Eastern States, from the smallness of their fortunes, the necessity of applying themselves to some profession, and the consequent want of leisure, have fewer opportunities than Englishmen, for indulging themselves in the pursuit of abstract science, or the acquisition of ornamental accomplishments" (Humphreys 1815: 11). Although not addressing science or scholarship in this drama, Humphreys anticipated an argument that became a common diagnosis in nineteenth-century American science. This diagnosis built on the distinction between "abstract" and "practical" sciences, a semantic pair that originated in eighteenth-century Britain (see chapter 1). The meanings of both these terms, however, were ambiguous. Daniel Kevles ([1977] 1995: 6) stated that for nineteenth-century American professionals, "abstract science" meant the "study of nature for the sake of understanding its substance, its working, its laws"; whereas "practical science" implied "the exploitation of nature and nature's laws for the sake of material development." Kevles' definition, however, projects a twentieth-century meaning of the basic/applied distinction onto the past.[1]

It might be more true to say that, in the first half of the nineteenth century, there was no genuine U.S. terminology categorizing different forms of research. Rather, there was a belief among the enlightenment-influenced po-

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litical founding fathers that science is necessarily both revealing the truth of nature and practically relevant (Cohen 1976: 370). Scientists did not feel the need to "draw the sharp line of cleavage between 'pure' and 'applied' science" (Cohen 1948: 62–63; see also Reingold 1970: 174). There was, in other words, an appreciated social practice called "science," but no requirement to distinguish variations within that category.

Furthermore, early uses of the term "abstract science" indicate a certain uneasiness. In 1830, for instance, *The North American Review* published a report about the New England Asylum for the Blind, the first school for the blind in the United States. The anonymous author discussed the possibility that abstract science might be a useful occupation for blind people:

> The blind, from the cheerful ways of men cut off, are necessarily excluded from the busy theatre of human action. Their infirmity, however, which consigns them to darkness, and often to solitude, would seem favorable to *contemplative habits,* and to the pursuits of *abstract science* and *pure speculation.* Undisturbed by external objects, the mind necessarily turns within and concentrates its ideas on any point of investigation with greater intensity and perseverance. It is no uncommon thing, therefore, to find persons setting apart the silent hours of the evening for the purpose of composition, or other *purely intellectual exercise.*[2]

The notions associated with abstract science—"contemplative habits," "pure speculation," and "purely intellectual exercise"—evidence the influence of a European, particularly humanist, tradition, and reveal a tremendous cultural distance: abstract science is depicted here not as an occupation for noblemen and the higher strata of society but rather for the disabled, who may in turn, however, ennoble their life. In other words, we see here not hostility toward abstract, theoretical, or intellectual activities, but rather a feeling of strangeness.

The *locus classicus* of the argument that Americans felt more inclined toward practice than theory is Alexis de Tocqueville's *Democracy in America,* an extensive work published originally in French in two volumes in 1835 and 1840. Again, it is important to keep in mind that this book was written from a European perspective and thus imposed European categorizations of science on American tradition. The work contains a famous chapter titled "Why the Americans Are More Attached to the Application of the Sciences Than to the Theory" (Tocqueville [1840] 2010: 775–787). In his notes on the chapter,[3] Tocqueville divides the sciences into three parts: (a) "purely theoretical" or "abstract," (b) "theoretical but close to application," and (c) "absolutely applied" (Tocqueville [1840] 2010: 775). "The Americans," he continues, "excel in the last two and neglect the first one." Thus while the American mind is "clear, free, original and fruitful," there is also "hardly anyone in the United

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States who devotes himself to the essentially theoretical or abstract portion of human knowledge" (Tocqueville [1840] 2010: 778).

Tocqueville ([1840] 2010: 781) assumes that there are different "motives that can push men toward science": "material interest," "desire for glory," and a "passion to discover the truth." Perhaps, he notes, "the greatest scientists are due uniquely to this last passion." The American people, he argues, lack this quality, and one finds mostly a "selfish, mercenary and industrial taste for the discoveries of the mind." But why is this the case? Tocqueville's answer was that the valuation of applied science is specific for all democratic peoples. While he did not rule out the possibility of some speculative genius in a democratic environment, he was convinced that the "pure desire to know" had in general other preconditions (Tocqueville [1840] 2010: 781), namely, an aristocratic structure that detached science from everyday needs: "All that I want to say is this: permanent inequality of conditions leads men to withdraw into proud and sterile research for abstract truths; while the democratic social state and democratic institutions dispose them to ask of the sciences only their immediate and useful applications. This tendency is natural and inevitable" (Tocqueville [1840] 2010: 785).

For the present argument, the point here is not to assess the plausibility of this hypothesis but rather to understand the changing categorizations of science that Tocqueville's thesis implies. Until today, many authors have cited Tocqueville to frame different narratives of how science and education developed in nineteenth-century America. By following the debates on this issue through the nineteenth and twentieth century, we learn a lot about identity work of scientists and science policy makers in the United States. For example, some four decades after Tocqueville's journey to America, a group of "frustrated American scientists," led by Joseph Henry and Edward Livingston Youmans, invited one of the most famous British lecturers on science, John Tyndall, to promote science and scientific education in America (Cohen 1976: 377–378).

Tyndall closed a series of lectures on light, delivered between 1872 and 1873 in various locations of the United States, with some general remarks about the position of science in American society. The message was simple, and at that time, resonated with the emerging U.S. ideal of pure science: "This, ladies and gentlemen, is the core of the whole matter as regards science. It must be cultivated for its own sake, for the pure love of truth, rather than for the applause or profit that it brings" (Tyndall 1873: 212). Tyndall (1873: 214) then turned to the sensitive issue: "I know what De Tocqueville says of you." Having said so, he did not try to refute the pessimistic assessment by Tocqueville about the problematic relation between democracy and pure science. He expressed, however, the hope that things might have changed in the

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meantime. One indicator, according to Tyndall, was that practical men were attending his lecture, not because they expected a monetary award but because they were interested in science as an "intellectual good." Furthermore, Tyndall explained that the reductionist view of science as delivering useful material results was not only an American problem, but rather quite salient in England, insofar that both countries "have reason to bear those things in mind" and were in danger to "forget the small spiritual beginnings of such results in the mind of the scientific discoverer" (Tyndall 1873: 216–217). Tyndall ended his lecture by proposing that his American hosts support every student and researcher willing to devote his life to pure science as much as possible. According to him, there was in America "a willingness on the part of individuals to devote their fortunes in the matter of education to the service of the commonwealth," with the chance now given "to wipe away the reproach of De Tocqueville" (Tyndall 1873: 225).

At a farewell banquet for Tyndall, one of the hosts, physicist and amateur astronomer Henry Draper, adopted this argument. He reiterated Tocqueville's thesis by stressing what U.S. scientists owed to their aristocratic British colleagues, believing it was possible to overcome the predicament in the future:

> Together we must try to refute what De Tocqueville has said about us: that communities such as ours can never have a love of pure science. But, whatever may be the glory of our future intellectual life, let us both never forget what we owe to England. Hers is the language that we speak; hers are all our ideas of liberty and law. To her literature, as to a fountain of light, we repair. The torch of science that is shining here was kindled at her midnight lamp. (Draper 1873: 736)

In sum, there was at least a small group of scientists in the 1870s eager to learn how to love "pure science." One might conclude that we can observe a transfer of the European, idealistic tradition to the United States. We should not, however, conclude in haste that this conception of "pure science" was of purely European origin.

## Semantic Substitution: The Rise of the Pure Science Ideal after 1850

Given the debates that evolved around Tocqueville's critique of American science, one might conclude that there was no genuine American understanding of pure science until the 1870s.[4] Indeed, most scholars assume that the idea of pure science was firmly established in the 1880s, an assumption backed mostly by Henry Rowland's famous speech "A Plea for Pure Science," delivered in

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1883 and published in several prestigious American and British journals.[5] A century later, science studies turned Rowland into the representative of "the *ne plus ultra* of pure science rhetoric in the nineteenth century" (Hounshell 1980: 612).[6] Along with figures such as Robert Merton, Vannevar Bush, and Michael Polanyi, science studies scholars often associate Rowland with elitist notions of unfettered, independent, and autonomous scientific research (e.g., Oreskes 2003: 727).[7] Furthermore, suspicion has been raised that Rowland's ideal of pure science has, "at least implicitly, guided the historiography of science" (Johnson 2008: 611).[8] The actual historical developments, however, are more complicated—for two reasons. First, it is problematic to conceive of historical actors as the "inventors" of new concepts. Contested concepts develop over centuries, travel through various cultures and languages, and only momentarily become stable. In this regard, "pure science" was no novelty in the 1880s: there were various lines of tradition reaching back to both European and American discourses. Second, even if concepts are introduced successfully, they are not stable, but in constant flux.

As indicated above, the terms "pure" and "applied" science were not or only occasionally used before the 1860s; it was more common to distinguish between "abstract" and "practical" sciences (Kevles [1977] 1995: 6; Lucier 2012: 528–529). There are, however, earlier debates dealing with tensions resulting from commercially-oriented research. Around 1850, Alexander Dallas Bache, a physicist that had gained high reputation as superintendent of the U.S. Coast Survey, gathered some scientists and formed a group that he self-mockingly named the "Lazzaroni."[9] Originally, the "Lazzaroni" were lower class beggars in Naples, Italy—scientists in the United States at the time seemed to have felt like beggars asking for money from their fellow citizens in order to indulge in their idiosyncratic lifestyle of scientific research (Jansen 2011: 250). The members of this group were involved in the formation of the American Association for the Advancement of Science (AAAS), the National Academy of Sciences (NAS), and the Smithsonian Institution.[10] While historians have discussed how influential this group has been (Beach 1972), for our argument it is more important to examine what kind of language they employed to communicate the intricate relationship between science and its social environment in a society where no traditional or stable scientific institutions existed.

Joseph Henry—a physicist, member of Bache's Lazzaroni, first secretary of the Smithsonian Institution, and second president of the AAAS—expressed his concerns about how economic influences could corrupt science: "The man of science [pursues] abstract researches [that] pertain not immediately to the wants of life" (Henry 1850, quoted in Lucier 2009: 719). Paul Lucier has shown that Henry carefully distinguished the role of "scientists"

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from commercially-oriented "professionals." For example, while Henry himself was consulted on various occasions by telegraph companies, he "usually gave his advice for free" and actually rejected money offered to him as compensation for his advice "as a bribe" (Lucier 2009: 719). Even more explicit was Bache, who followed Henry as president of the AAAS in 1850, and said, "Applied science is profitable in a pecuniary sense; but abstract science, on which the other hangs, is not remunerating. Yet how many applications flow from one principle! The world would gain, in a very high ration, by bestowing its rewards for principles, instead of for applications" (Bache 1851, quoted in Lucier 2009: 719). David Hounshell (1980: 616) interpreted Bache's address as "in large part a plea for pure science," anticipating much of Rowland's famous speech thirty years later. Nathan Reingold (1970: 172–177), in contrast, discussed these issues ten years earlier, a time when contemporaries hotly debated American "politics of pure science" (Greenberg 1967). Reingold warned scholars against reducing Bache to a "pure science" role model. According to him, Bache's main achievement was to organize a social milieu in which scientists, engineers, and other stakeholders could come in contact: Bache and his people "did not see any chasm separating theory and practice" (Reingold 1970: 174). In a study of Bache's role in the U.S. Coast Survey, Hugh Richard Slotten (1993: 47) clarifies this argument: to communicate expensive "big science" projects both to the government and public, "the distinction between what we would today call basic and applied research was left usefully obscure."

Today, such interpretations from historians have become historical themselves, projecting specific notions of "pure science" and "basic research" onto the past. In our view, scholars can reflexively control such historiographical problems by focusing more accurately on the role of language in the politics of science. In doing so, we may see that neither Henry nor Bache used the pure/applied distinction; rather, they employed the common terminology of their time, the distinction between "abstract" and "practical" sciences. On the other hand, they are also examples for how prominent scientists began to build new institutions and therefore delivered ideas and arguments for why society should support their scientific work. These arguments, in turn, may have paved the way for the new term "pure science," which gradually took shape in the decades to come.

A valuable source for a first impression of the slow emergence of the pure science concept are the *Proceedings of the American Association for the Advancement of Science*. Between 1848 and 1872, these works contain hardly any distinct references to "pure science,"[11] while in 1873 the chemist Lawrence Smith emphatically used the term in his presidential address. We find in Smith's talk the usual postdated utilitarianism argument, which Bache had already em-

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ployed two decades earlier for the same occasion. This time, Smith complemented "abstract scientific ideas" by referring to the "man of pure science":

> Let us ever bear in mind that it is abstract scientific ideas which underlie, in these modern days, all discoveries conducive to man's progress, from the making of a pen to the construction of a telescope; or, as Herbert Spencer well expresses it, "each machine is a theory before it becomes a concrete fact." The man of pure science paves the way, erects the mile-stones, and puts up the guide-post for the practical man. (Smith 1873: 5)

Three years later, the *North American Review* published an article titled "Abstract Science in America, 1776–1876" (Newcomb 1876). The author, astronomer Simon Newcomb, compared the development of American science with its European counterpart. Yet, although part of the title, the term "abstract science" appears only once in the lengthy manuscript. Instead, Newcomb employs systematically the up-and-coming terms "pure science" (101, 102, 106, 110) and "applied science" (101, 106, 119, 123). Furthermore, he uses the adjective "pure" several times in connection with "love." Each time, the phrase "pure love" (93, 99, 101) is implemented to foreground the value of science as independent from utilitarian considerations: "The first condition of really successful and important scientific investigation is, that men shall be found willing to devote much labor and careful thought to that subject from pure love of it, without having in view any practical benefit to be derived from it as an important consideration" (Newcomb 1876, 93). This conjunction of "love" and "science" was nothing new—we find it not least in the writing of Tocqueville ([1840] 2010: 775, 781, 782, 785), who was building on a European tradition. The idea of an *amor sciendi* is a recurring motive attributed to those joining the first universities in the twelfth and thirteenth centuries (Grundmann 1960). We also find the term in Rowland's (1883a: 243) speech: "There will be those in the future who will study nature from pure love, and for them higher prizes than any yet obtained are waiting." The question remains, however, on whether the notion of pure science, rooted in a love of truth, was simply a European heritage that found its way into American semantics of science, or whether there was something more unique within the American ideal.

David J. Kevles remarks that the substitution of "abstract science" for "pure science" by Rowland and others added the "connotation of high virtue" to the formerly descriptive phrase (Kevles [1977] 1995: 45).[12] Although true, it does not explain the particular success of pure science as an American ideal in the late nineteenth and early twentieth centuries. There is another, more important, religious connotation in the adjective "pure": while in the late nineteenth-century Europe, this bond had become loose and scientists

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started distancing themselves from religion, religious puritanism in the United States was a much more relevant cultural influence at the time.

Rebecca M. Herzig examines the relevance of religious puritanism for nineteenth-century American scientists (2005). Herzig deals, among other things, with pure science, making the intriguing point that this ideal was coupled with religious notions of suffering. “Purists” demonstrated their emancipation from material concerns through “embodied displays of deprivation”; they deliberately embraced toil and poverty, and thus enacted a “sacrificial self” (Herzig 2005: 48). Herzig (2005: 49) supports this assumption by examining the biographies of scientists such as psychologist G. Stanley Hall, chemist Ira Remsen, and physicist Henry Rowland, all of whom “imbibed while still in childhood this brew of suffering, moral uplift, and worldly labor.” Sociostructural statistics of U.S. scientists between 1861 and 1876 demonstrate that over 27 percent had fathers employed in the ministry. Furthermore, several “pure science” supporters studied for the ministry, originally aiming for a spiritual career before dedicating their lives primarily to science. This trajectory proved relevant for the way they later conceived of scientific practice: “While they moved away from religious service, they maintained Protestant ideals of purification through worldly labor” (Herzig 2005: 49). As researchers, however, they did not perceive this as problematic if religious and scientific values met in the search for truth. Ira Remsen, for example, a renowned colleague of Rowland at Johns Hopkins University, saw a common purpose in religion and science: “The *ultimate* of both science and religion are *infinites* . . . something that gives meaning to all that passes, and yet eludes apprehension; something whose possession is the final good, and yet beyond all reach, something which is the ultimate ideal, and the hopeless quest” (Remsen, quoted in Herzig 2005: 61, emphasis original). Lawrence Smith’s address to the AAAS, which has already been quoted above, contains a similar argument: “Science and religion are like two mighty rivers flowing toward the same ocean, and before reaching it they will meet and mingle their pure streams, and flow together into that vast ocean of truth which encircles the throne of the great Author of all truth, whether pertaining to science or religion” (Smith 1873: 24–25).

This picture of scientists transposing Puritan ideals to the sphere of science is quite different from, but not contradictory to, the more common interpretation of the pure science ideal stressing the demarcation between science and commerce. We must, in fact, assume that the religious aspects were often tacit while scientists more explicitly addressed the relationship between science, technology, and industry. Historians such as Hounshell (1980), Dennis (1987), and Lucier (2009) stress that Rowland’s pure science ideal basically rooted itself in concerns about the appropriation of science by commercial

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interests. It would be problematic, however, to reduce the pure science ideal to this kind of boundary work. Similarly relevant for Rowland and others was a more positive definition of what science is about, a positive identity only understandable when seen against the background of religious influences and the corresponding notion of truth—the demarcation against commercialized applied science, as well as the positive, moral and religious values of science nested within each other:

> We are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science . . . . We wish for something higher and nobler in this country of mediocrity . . . . Nature calls to us to study her, and our better feelings urge us in the same direction. . . . Young men, looking forward into the world for something to do, see before them this high and noble life, and they see that there is something more honorable than the accumulation of wealth. (Rowland 1883a: 243, 244)

Furthermore, Rowland uses a biblical metaphor when describing how the pioneers of pure science "strike into unknown forests, and climb the hitherto inaccessible mountains which lead to and command a view of the *promised land*" (Rowland 1883a: 248, emphasis added). In the decades to come, Rowland's speech created a tremendous echo: Countless scientists adopted his rhetoric, reflecting the condition and perspectives of American scientists. They rarely, however, explicated the religious dimension of the pure science ideal, a dimension that in time became further diluted.

Thus, while "pure science" was firmly established as a value, the rationales associated with it (and brought forth to defend it) gradually shifted once more toward utilitarian arguments. At the turn of the twentieth century, many associated "pure science" as a necessary precondition (rather than a counter-concept) for commercialized, applied science. For example, in an article on "Utilitarian Science," the zoologist and first president of California University (Stanford) David Starr Jordan defined what would later be called the linear model of innovation: "Applied science can not be separated from pure science, for pure science may develop at any quarter the greatest and most unexpected economic values, while, on the other hand, the applications of knowledge must await the acquisition of knowledge, before any high achievement in any quarter can be reached" (Jordan 1904: 76). Lucier (2012: 582) summarizes the paradox of the late nineteenth century ideal of pure science by pointing out two contradictory rhetorical strategies: the disassociation of pure science from applied science as opposed to the causal link between them. In other words, science was either (a) purified as a distinct activity separate from technology and commerce, yet somehow related to religion, or (b) understood more broadly as encompassing applied science and technological

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developments as well—that is, as a complex system in which pure science was a crucial, but not the only, dimension.

## Semantic Transition: Adjusting to the Pluralization of Research after 1914

In the early twentieth century we witness a semantic transition correlated with a fundamental structural change to the U.S. research landscape. The increasing industrial demand for scientific research led big companies such as General Electrics or DuPont to establish their own laboratories. Furthermore, the new level of "scientific warfare" that emerged in World War I highlighted the national and military relevance of scientific knowledge production. This, in turn, resulted in the establishment of the National Research Council (NRC) in 1916. Nevertheless, compared to Britain, which installed the Department for Scientific and Industrial Research (DSIR) in the same year, the U.S. government showed less support for scientific research. U.S. federal science policy was still rudimentary—and certainly less intervening—than the British at that time. Whereas agricultural research funding had become a regular budget item for the U.S. government, other federal agencies confined themselves to the commission of surveys on geological or geographical issues (Dupree [1957] 1986: 149–214).

The lack of federal engagement in science policy stimulated an ongoing public debate about future organization, funding, and prestige for various strands of research in early twentieth-century America. A series of *Science* articles from the late 1880s onward illustrates how scientists, engineers, and the new industrial scientists were struggling with the ideal of pure science. This came at a time when novel concepts emerged marking a new, genuine category of research associated with different attributes such as "pure," "fundamental," "basic," "applied," and "industrial." The concept of research—singled out as the act of scientific knowledge production—started to challenge the supercategory of science. Although "pure" and "applied" science had not yet lost their rhetoric power, this new set of concepts can be considered an attempt to overcome the strict dichotomy of such language.

One of the new concepts traces back to the mid-1890s, when U.S. agronomy scientists called for more "fundamental research" (Arthur 1895: 360). Problem-oriented research in plant breeding led scientists to new general aspects of plant physiology that "pure" botany had not yet raised. Scientists of applied botany, therefore, saw little reason to distinguish between pure and applied science: "All science is one. Pure science is often immensely practical, applied science is often very pure science" (Coulter 1917: 228; see also Coulter 1919). As a part of publicly-funded experimental stations at land-

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grant colleges, however, these researchers were expected to provide results that could directly improve farming practices and increase crop yields (Marcus 1985). The demand for more fundamental research thus expounded the problem of uncertain scientific outcomes, even if a project had a clear task to fulfill. Given this uncertainty, "fundamental research" conveyed the promise of laying a cornerstone for future technologies and new products. Furthermore, since land-grant colleges were not on an equal footing with universities in terms of scientific prestige (Thelin 2004: 135–137), the new label "fundamental research" conveyed a commitment to the quest for fundamental scientific principles that, at the same time, could improve their scientific reputation. The alternative concept of "basic research" emerged for the first time also in the context of agriculture. In a congressional hearing on agricultural policy in 1919, politicians debated on the role of public support for "basic research" in agronomy. According to Roger Pielke (2012: 340–343), "basic research" was thus an offspring of this political discourse, since its use was limited to the political arena until the late 1930s.

The support for agricultural research and its effect on yield increase became a shining example for the industry, initiating a discourse on the relevance of industrial research (Little 1913: 649–650). The term "industrial research" was, in fact, a new entry in the lexicon of science policy: before World War I, it appeared only very sporadically, but after 1918 it became quite common. *Science* first mentions the term in 1910; from that point onward, the annual number of appearances saw a steep and relatively steady rise until 1945, with the first peak in 1917 (fifty-four articles mentioning the term) and second peak in 1943 (ninety-one article). One of the earliest and most cited articles was Arthur D. Little's "Industrial Research in America" (1913). Facing increased competitive pressure from the global market, the MIT-trained chemist and entrepreneur urged U.S. companies to expand their research activities, especially the "fundamental investigation of the scientific bases" on which the industry rested (Little 1913: 645). As in the field of botany, industrial scientists questioned the hierarchy between "pure" and "applied" science: "Most of us are beginning to realize that the major problems of applied chemistry are incomparably harder of solution than the problems of pure chemistry. . . . Industrial research is applied idealism: it expects rebuffs, it learns from every stumble and turns the stumbling block into stepping stone. It knows that it must pay its way. It contends that theory springs from practice" (Little 1913: 648, 655). Nevertheless, these pioneers of industrial research embraced the belief in discovering fundamental scientific principles and the quest for new knowledge—that is, ideals traditionally associated with pure science but now acting as a substitute for manufacturers' "rule-of-thumb methods" (Bacon 1914: 878).

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Although some contemporaries contrasted pure science with industrial research, the latter no longer fit into the old dichotomy of pure and applied science. "Industrial research," rather, was divided into subcategories, namely unrestricted "pure research"—or, using the term that became more popular, "fundamental research"—and "applied research," followed by a final phase of technological development. Given the "dissatisfaction with the terms 'pure science' and 'applied science'" (Carty 1916: 511) and its associated moral hierarchy—with "sublime" pure science on the top and applied science as the "prosaic activities of technology" on the bottom (Hamor 1918: 319)—the discourse on industrial research resulted not only in conceptual hybrids such as "fundamental research" but also in ample attempts to classify different types and stages of research (Hamor 1918; Balls 1926; Whitney 1927).

Studies on companies such as AT&T, General Electric, DuPont, and Eastman Kodak (Reich 1985; Wise 1985; Dennis 1987; Hounshell and Smith 1988) have pointed to the increasing relevance of fundamental research in early twentieth-century industry: the old distinction between pure and applied science was no longer appropriate for communicating research practices and goals within the industry. Historians are still puzzled, however, over how the attribute of "pure" was still appealing at the beginning of industrial research. A well-known example is the chemical company DuPont, whose leading chemist, Charles Stine, introduced a fundamental research program outlined in the memorandum "Pure Science Works." Accordingly, the building for "pioneering research" bore the nickname "purity hall" (Hounshell and Smith 1988: 223–248). Whenever industrial scientists called for more "pure research" at the beginning of the twentieth century, they intended to conjure up the scientific spirit of venturing into the unknown—a spirit, according to them, still missing among many industrial laboratories and engineers (Little 1913: 643; Bacon 1914: 871; Hamor 1918: 325).

These advocates of industrial research were aware that research was a risky, often unpredictable, and long-term endeavor in stark contrast to economic rationales—but they firmly believed that it would pay off in the end (Brown 1914: 592; Carty 1916: 512; Hamor 1918: 320). Historians David Hounshell and John Smith (1988: 247) presume that, by the end of this semantic transition, the concept of fundamental research prevailed over pure research mainly because it hinted more directly at its practical relevance for industry. Similarly, Kenneth Lipartito (2009) pointed out that Frank Jewett, director of Bell Labs, began to talk about "fundamental research" in the 1930s. His aim was to break up the traditional opposition of academic and industrial research; according to Lipartito (2009: 142), "Fundamental corporate research served business needs, though otherwise it was the same as pure research."

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Still, the increasing popularity of "fundamental research" indicated more than a growing demand for new scientific knowledge in agriculture and industry. It triggered a broad debate about the means and ends of science. "Fundamental research" denoted research revolving around basic scientific problems, the solutions for which were promised to advance technology, agriculture, industry, and thereby people's overall wellbeing. Scientists promoted fundamental research as a "national asset" (Coulter 1917) beneficial for the "national welfare" (Nutting 1917). In return, advocates of fundamental research, such as the chief engineer of AT&T John J. Carty, challenged the ideal of performing science for its own sake: "But surely this motive must be intensified by the knowledge that when the search is rewarded there is sure to be found, sooner or later, in the truth which has been discovered, the seeds of future great inventions which will increase the comfort and convenience and alleviate the sufferings of mankind" (Carty 1916: 514).

Against the background of World War I, even the National Academy of Science reformulated its objectives. Since science's social relevance had notably increased, the Academy wanted to increase the dissemination—that is, outreach—of scientific investigations vis-à-vis the general public (Hale 1914: 914). The Academy, however, still claimed the superiority of "pure science" as paving the way for new discoveries: "Immediate commercial value as a criterion of success will not often point the way to the discovery of fundamental laws" (Hale 1914: 919).

The plea for more research had, however, two sides: industrial research managers criticized American universities for neglecting both research activities and cooperation with the industry. They expected scholarship to serve "the immediate mass of humanity," not be a goal in itself: "Scholarship for the sake of scholarship—never!" (Brown 1914: 589). After companies had successfully built their own laboratories, industrial scientists doubted that universities were still the centers of scientific research:

> For the last fifty years it has been assumed that the proper home for scientific research is the university, and that scientific discovery is one of the most important—if not the most important—function which a university can fulfill. In spite of this only a few of the American universities, which are admittedly among the best equipped and most energetic of the world, devote a very large portion of their energies to research work, while quite a number prefer to divert as little energy as possible from the business of teaching . . . . Looking back on the history of science we can perceive that so far as research work has been associated with institutions, it has always been because those institutions required the results of the research for the effective performance of their own essential duties. (Mees 1914: 618)

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Only a few universities, such as the Massachusetts Institute of Technology (MIT), were exempt from this critique. Other universities reacted to the increasing industrial demand for research and scientifically trained staff. In 1913, the University of Philadelphia founded the Mellon Institute of Industrial Research, which defined itself as a "link between the world of science and the industries" (Weidlein 1935: 562). Notably, the Mellon Institute used the term "fundamental research" as a general label for their projects and training.

At the dawn of World War I, researchers identified Germany as a role model for the United States (Bacon 1914: 872–873; Hale 1914: 918). Arthur D. Little (1913: 643) wrote, "Germany has long been recognized as preeminently the country of organized research." He admired not only the German scientific spirit, but also the scientific training at German universities and academic-industrial cooperation. Kenneth Mees (1914), director of the laboratory at Eastman Kodak, called for new institutes that were entirely devoted to research without having any teaching duties. Germany had already established this type of research institute under the umbrella of the Kaiser Wilhelm Society three years before Mees's comment was published. The state and industry funded the Kaiser Wilhelm Society, and represented a national effort to bring academia and industry together. Mees and his colleagues, however, lacked such national research institutes (Brown 1914: 588).

After World War I, scientists and industrial laboratory leaders intensively discussed the relationship between industry and academic "pure science," as well as how to organize different research activities in the most efficient way possible. Many feared the neglect of training in the natural sciences. Industrial research attracted talented researchers with good salaries, making academic positions less attractive (Hale 1914: 919; Hamor 1918: 328). In the 1920s, a public campaign for "pure science" in the *New York Times* aimed to raise support for research at universities.[13] The driving force of this plea was still the same narrative of the United States lagging behind Europe's science nations. Europe's historical situation and self-image, however, had already changed as Europe suffered from the severe consequences of World War I. American scientists were apprehensive that the springs of European scientific knowledge could dry up—they had to take the lead of knowledge production in the natural sciences.[14] Finally, the stock market crash in 1929 and the resulting economic crisis showed the vulnerability of a mostly privately-funded university system and, more generally, a scientific infrastructure based largely on philanthropy and private donations (K. Compton 1934).

Yet the old brand of "pure science" for academic research proved less successful in communicating the societal benefits of science in the late 1930s. The profound reforms of U.S. federal policy under the New Deal did not include a national funding program for academic research, but rather led to

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drastic cuts in federal research funding (Dupree [1957] 1986: 344–350). The increasing use of concepts like "fundamental science" in the late 1920s and early 1930s suggests that the purity metaphor, still associated with the idea of science for its own sake, was unsuitable for increasing federal funding for universities (see, e.g., Penkins 1929; Merriam 1934: 599–601).[15] On the other hand, developments in the 1920s and early 1930s revealed that industrial research could not be taken for granted: restricted to only a few large companies, even they cancelled respective programs as soon as the economic crisis left its mark in the early 1930s. In the case of DuPont, the era of high-level scientific research ended in 1932, at least for the time being. The risky investment in industrial research was one of the reasons why the industry asked for greater support for academic research—meaning pure science.

This ambiguity applied even more so to U.S. engineers who were torn between the ideal of pure science and the wish to set themselves apart from the natural sciences. According to Ronald Kline (1995: 205), "the gospel of industrial science" during World War I did not triumph over the "gospel of high and pure science," which continued determining the identity of engineers in the first half of the twentieth century. There is also evidence, however, that engineers in the late nineteenth century tried to establish their own professional identity vis-à-vis pure science. Henry Thurston (1884: 237–238, 243), vice president of the mechanical science section at the AAAS, propagandized "a system of application of science," in which scientific knowledge was interwoven with "arts" and "industries." Thurston neither argued against the spiritual value of science nor advocated its commercialization. He did, however, oppose the humanistic ideal of education, which according to him was characterized by a disdain toward any useful or practical aspects of scientific knowledge. Other "scientist-engineers" stressed the originality of their research in order to bring engineers out of the natural sciences' shadow (Kline 1995: 203). Still, engineers stuck to the disciplinary label of applied science and thus "affirmed a cultural hierarchy subordinating technology to science," which defined technology as "the application of pure science" (Forman 2007: 31). Only in the second half of the twentieth century did the engineering sciences succeed in establishing their own distinctive language through the new core references of technology, innovation, and different types of knowledge (e.g., "design," "tacit knowledge," "mind's eye").

The case of engineers demonstrates that in the first half of the twentieth century, the supercategory of science had not yet lost its prestige and integrative power—that is, engineers still put their trust in the effect of "bandwagoning" (Harris 2005: 104; see also Kline 1995: 221). This was certainly not a one-sided effect since engineers converted the scientific promise of progress into concrete material achievements and thus lent a perceptible image to the

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abstract notion of science—a fact those scientists were well aware of (Hale 1914: 919). In contrast to industrial scientists with a background in the natural sciences, many still assumed that engineers lacked the spirit of scientific research. In the same vein as industrial scientists, engineers used alternative concepts such as "fundamental research" and "fundamental science," which indicated their struggle with the sublime motive of pure science. Yet, given the critique from their colleagues, they still had to stress the importance of pure science for the scientific training of engineers (Trowbridge 1928). Moreover, the industry expected engineers to venture into the unknown and not be restricted to research determined by given technical problems. The industrial chemist Willis Whitney (1927: 288) put it as follows: "I am only contending against the thought that anyone can long foresee what may become our major needs and thereby circumscribe pure science research. None of our necessities were planned that way, not even a wheel. Wheels came into engineering, as steam did, through curiosity." Whitney's use of the hybrid term "pure science research" suggests he did not want to refer to the other attributes associated with "pure science." In fact, he did not evoke the notion of science for its own sake, and restricted his argument to the epistemic idea that a scientific venture into the unknown could not be planned according to present needs.

All in all, we must consider the interwar period as a phase of semantic transition. The differentiation of research activities and respective institutional settings in the early twentieth century resonated with the emergence of "research" as a category, which in many respects began to substitute the older academic understanding of science. After industrial research appeared on the map, research managers and science policy experts started to deliberate on how different "spheres," "classes," or "branches" of research related to one another, as well as what kind of division of labor could guarantee an efficient organization of research as a "whole" for the benefit of national welfare and prosperity (Godin and Schauz 2016). While the use of the term "fundamental research" reflected these developments, "pure science" was still perceived as prestigious, but its meaning had been narrowed. Many used "pure science," for example, when pointing to an open-minded curiosity and training in scientific methods. The older motif of performing science for its own sake did not sell anymore.

## Semantic Fusion: Basic Research and the Revival of the Pure Science Ideal after 1945

After the incremental transformation of science and research in the first decades of the twentieth century, World War II marked an incisive experience for the U.S. scientific community, forcing the United States to much more

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profound adaptions in a very short period of time. The launch of collaborative research projects relevant to warfare—above all, the Manhattan Project at Los Alamos and radar research at MIT and Bell Labs—required national management and federal funding. Consequently, "pure science" definitively lost its grip as a leading idea, and the use of "fundamental" and "basic research" rose steeply during the war.[16] Given the immense expectations for immediate results, some scientists feared they could no longer meet the demand of new knowledge for technical development (Simons 1943: 391). Due to massive military recruitment, this fear of scarcity also applied to personnel resources in science (Barton 1943: 176; Taylor 1944: 250; A. Compton 1945: 208). In the end, the war experience created a new narrative: a concern that scientific knowledge might run short if researchers aligned knowledge production exclusively with the needs of society.

Since 1942, scientists had been discussing future institutional rearrangements. At the end of the war, plans for a new science policy regime were already on the table. Despite scientists' critique, wartime research had strengthened the position of science in society. As the U.S. government spent more money on science during World War II than ever before (Bush 1945: 82), scientists had a particular interest in perpetuating this federal commitment during peacetime. In July 1945, Vannevar Bush delivered his famous report, which justified the state's obligation to support basic research in four ways: first, the support of young researchers in the natural sciences; second, the improvement of public health through medical research; third, the advancement of public welfare, which was almost synonymous with economic growth and job security due to new technological developments; and fourth, the guarantee of national security by long-term civilian basic research, which promised to give the United States a technological edge in armaments. A plethora of literature deals with the political controversies following the Bush report (Kevles [1977] 1995; Reingold 1987; Owens 1994; Stokes 1997; Zachary 1997: 218–239, 249–260; Dennis 2004). The lines of conflict were drawn between the federal government and the states, the Republicans and the Democrats, public administration and interests of private stakeholders, as well as between scientists and federal government or industry.

In the end, "basic research" worked as an integrative political symbol (Pielke 2012). The term "basic" did the trick as it conveyed the promise to lay the foundation for all kinds of future benefits for both society and the advancement of science. Bush's short definition of basic research as "research performed without thought of practical ends" (Bush 1945: 13) contradicted the original understanding of fundamental research in the context of applications. At the same time, the Bush report envisaged *inter alia* the funding of basic research for military matters. His understanding of basic research

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was ambivalent. By focusing the funding program on the natural sciences or "basic disciplines," he dissociated them from the technical sciences. Bush seemed to detach basic research from any motive of practical or technical application, evoking the impression of returning to the old pure science ideal (Shepard 1946). The use of "basic research" shifted from industrial research toward the academic world of the natural sciences, formerly called "pure science." This image of science, however, was quite contested among Bush's peers, even among his closest colleagues (Conant 1948).

We must interpret Bush's draft against the background of the war experience. Despite the achievements made during World War II, researchers feared that the equilibrium between the production of scientific knowledge and its application would be disturbed (Bush 1945: 5, 8). The time-consuming and often unpredictable production of new knowledge became the common denominator for various definitions of basic research circulating in the late 1940s. Bush's proposal reacted to the organizational conditions of wartime research, particularly regarding security restrictions—a problem already discussed during the war (K. Compton 1942: 28). The report called for the prompt release of classified research after the war, as well as federal support for international exchange. According to G. Pascal Zachary (1997: 220), the plea for eliminating military secrecy was given not only for the sake of scientific progress but also in favor of civilian spin-offs arising from military research. With the upcoming Cold War rivalry and resulting military conflicts, however, secrecy policy proved an incessant challenge for scientific research.

Bush's proposal originally aimed to support basic disciplines in the natural sciences and medicine at universities.[17] Surprisingly, after having become the spearhead of scientific endeavors, American researchers looked back to continental Europe. Postwar proposals for higher education in the United States idealized European research universities and their humanistic ideas of education, associating them with democracy (Bender 1997: 4–5). The arguments for reinvigorating the university within an increasingly pluralistic research landscape were twofold: a growing need for scientifically trained researchers and free inquiry in academic science. This historical reminiscence evoked a revival of nineteenth-century pure science ideals with its high moral values. In this sense, Bush's definition was the basis for the NSF's definition of basic research:

> A worker in basic scientific research is motivated by a driving curiosity about the unknown. When his explorations yield new knowledge, he experiences the satisfaction of those who first attain the summit of a mountain or the upper reaches of a river flowing through unmapped territory. Discovery of truth and understanding of nature are his objectives. His professional standing among his

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> fellows depends upon the originality and soundness of his work. Creativeness in science is of a cloth with that of the poet or painter. (NSF 1953: 38)

Despite the historical references to discovering truth and understanding nature, this definition cannot conceal the fact that it focuses on the requirements of postwar science policy, particularly regarding the sustainability of knowledge production. In other words, the history of science became an argument for federally supporting autonomous science by depicting famous scientists like Michael Faraday, John Tyndall, and Louis Pasteur as pathfinders for modern technology—a strategy well known from other contexts, such as Victorian British science (e.g., Gooday 2012: 549–551). These historical narratives construed continuity from the nineteenth century to postwar America, which concealed previous struggles over the means and ends of science. In 1961, James B. Conant, chemist and one of the leading science policy advisers of postwar America, stated: "The history of science demonstrates beyond a doubt that the really revolutionary and significant advances come not from empiricism but from new theories. The development of these theories, in turn has in the past depended on free discussion of their consequences. How much can be accomplished behind a wall of secrecy remains to be determined" (Conant 1961: 30).

A "Symposium on Basic Research," held at the Rockefeller Institute in New York City under the patronage of the NAS and AAAS (Wolfle 1959), captured the semantic fusion during the heyday of basic research as shared by various government representatives and academia, as well as industry and philanthropic organizations. Regarding the training of young scientists, the ideal of basic research led to a new version of the old boundary discourse of "pure versus applied" and "theory versus practice." Biochemist Conrad Arnold Elvehjem (1959: 94), who represented state universities and academic teachers at the symposium, declared that the ideal of training "good scientists" was incompatible with military and other contract research. Even engineering sciences felt compelled to adopt pure science ideals, whereby profit interests should be taboo in institutions of higher education so long as they are part of scientific training (DuBridge 1959: 109–110). Universities specializing in the applied sciences, such as the California Institute of Technology (Caltech), understood basic research as an integral part of modern engineering and inseparable from the overall pragmatic goal of inventing new technology (DuBridge 1959: 109–110). Yet, given the fact that the old label "applied science" still denoted technical disciplines, science policy advisors sought less confusing terms, such as "analytical engineering" (Killian 1959a: 122).

In the long run, the universities only maintained this idealist teaching policy to a certain extent, as the number of military-related research projects—often including doctoral students—grew during the Cold War (Dennis 1994).

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During this time, the universities' role as "protected spaces" devoted to autonomous science (Rip 2011) was downgraded to mere symbols for Western liberal society, simultaneously providing fig-leaf camouflage for the technology-based arms race. As a political symbol, basic research offered an ideological surplus: Politicians contrasted the supposedly limited, local applications of communist countries' technology-driven research with the universality of basic research designed to benefit all mankind, with which the United States claimed ethical superiority. In his address at the symposium, President Eisenhower (1959) phrased it in his simple, well-known formula: "Science: Handmaiden of Freedom."

Support for basic research in the natural sciences was grounded in the hope that a few basic discoveries would prove sufficient to significantly broaden the potential for technological applications (Elvehjem 1959: 98). In order to protect basic research in the natural sciences, academic experts wanted such disciplines to stay clear of any kind of technical developments. As Alan T. Waterman (1959), the first president of the NSF, proclaimed at the symposium, "The growing applications of physics, chemistry, and mathematics should be shifted to engineering departments and kept out of the regular science departments." In other words, from the point of view of the natural sciences, applied research primarily meant research that yielded future technology.

This position was backed up by the revival of old academic virtues. Geophysicist Merle A. Tuve (1959: 174, 175), who represented private research institutes at the symposium in New York, stressed that "truly 'basic research' was driven by a passionate love for knowledge. Basic research thus meant 'support for ideas' in the first place" (see also Waterman 1959). This definition of basic research tended to be averse to technology. Besides, the new federal support for basic research at universities originally focused on individual researchers so as to foster "the development of the individual scientist" (Waterman 1959: 34; see also Greenewalt 1959; 128–131; Morison 1959: 230; Weaver 1959: xi). In contrast to applied research, which was supposed to be carried out in larger, work-sharing research groups, experts believed in individual creativity as the main property of outstanding scientists, enabling them to venture into the unknown. In this context, experts understood basic research as the free flow of unconstrained intellectual creativity (Tuve 1959). This praise of individualism had its roots in the myth of the American frontier society (see final chapter), which was revived in the Cold War period under the sign of democracy.[18]

While the amount of research as a direct response to economic and military demands had increased tremendously since the Korean War (Killian 1959a: 122), universities were conceived as a reservation for long-term basic research within a changing research landscape. Protecting scientific research "from

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the insistent demands of applied research" became a central argument employed by scientists, industry members, and politicians (Weaver 1959: xiv; see also Greenewalt, 1959: 128). Yet what was initially intended to protect scarce knowledge resources could, in the long run, transform into an ideal of purity.

The fact that, after World War II, many associated basic research with academic research did not imply that the differentiation between basic and applied research represented a clear-cut institutional division. Big companies continued their fundamental research programs: enterprises such as DuPont or the Bell Telephone Company had always intended to expand their participation in basic research after the war ended (Fisk 1959). Since economic rationales entailed selecting projects most likely to lead to technical inventions, however, such companies welcomed the idea of federal funding to carry out riskier projects at universities (Greenewalt 1959: 130). While companies wanted to avoid the costs of failures and deadlocks, this division of labor proved financially promising. According to the president of DuPont, Crawford Hallock Greenewalt (1959), basic research was considered a "technological savings account" for the industry.

Nevertheless, "basic research" remained important in application-oriented research fields. Given Bush's definition of basic research, however, some felt the need to redefine the term. Already in the immediate postwar years, John Steelman, science adviser to President Truman, divided basic research into two subcategories: (a) fundamental research defined as "theoretical analysis . . . directed to the extension of knowledge of the general principles governing natural or social phenomena," and (b) "background research" defined as "systematic observation, collection, organization, and presentation of facts using known principles to reach objectives that are clearly defined before the research is undertaken to provide a foundation for subsequent research" (Steelman 1947, vol. 3: 6). The discussion at the symposium and new terminology such as "mission-oriented basic research" or "mission-related basic research" from the 1960s indicate that the criterion of intention, whether it be utility-oriented or not, became problematic in the long run (Tuve 1959: 174; Waterman 1965: 15; Kistiakowsky 1966: 18).

While the ideal of basic research gained momentum in the late 1940s and 1950s, overall public spending for basic research remained quite low compared to applied research projects. During the 1959 basic research symposium—at a time when the NSF had been operating for several years—scientists criticized the low federal base rate for basic research compared to the Department of Defense's contract research, which was twice as high (Elvehjem 1959: 94; Waterman 1959: 26–27). One sophisticated argument noted that many basic research projects at universities were actually mission-directed in the service of national defense (Tuve 1959: 173–176). Basic research projects in ocean-

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ographical studies, for example, carried out within the context of naval research in the late 1940s and early 1950s, illustrate three things. First, they represented basic research highly relevant for military purposes. Second, they shed light on how the postwar discourse had redrawn a line between the natural sciences, denoted as basic research, and applied research, leading to new or improved technology. Finally, the example hints at how the distinction between basic and applied research was associated with security policies: the U.S. Office of Naval Research was a staunch supporter of basic research in oceanography, yet the question of secrecy revealed that the navy and scientists differed in their classification of basic and applied research and in their notion of utility. Oceanographers defined their investigations of topographical features or meteorological conditions of the ocean as basic research as long as they did not expressly serve the development of technology destined for use by the navy. The navy, however, developed "a more sophisticated definition of basic research," taking into account its operational nature and the strategic utility of geography for military purposes (Hamblin 2002: 27). In return, one can construe from this difference of perspectives that the semantic boundary work for scientists who wanted to shake off severe secrecy constraints by identifying their projects as basic research could turn into a battleground over scientific freedom.

In addition, the technical relevance of research for military purposes bore political and ethical dilemmas, which urged scientists to develop particular boundary strategies. After the United States dropped atomic bombs on Hiroshima and Nagasaki, the role of science in society became more controversial (Cohen 1948: 4; Conant 1961: 6–13). As scientists noted, the "atom bomb once and for all explodes the 'neutrality' of technology" (Shepard 1946: 66). Given this pitfall of modern science, natural scientists were more likely to embrace the ideal of basic research, as they could avoid assuming ethical responsibility for the application of their scientific knowledge. Furthermore, in a Cold War political climate, the label "basic research" helped scientists to avoid "politicking," which they considered a "disease" within project research dominating American universities at the time (Gates 1958: 234). During the McCarthy era, liberal and left-wing scientists often couldn't afford to participate in debates on research policy (Wang 2002). In 1956, the president of the Associated Universities stated that the question of loyalty almost inevitably arose when it came to discussing technological application:

> If a scientist expresses a strong view on some technological matter that may be contrary to the application of technology to current or to subsequent policy, he is open to the accusation of taking this view with the intent of deliberate subversion. . . . Moreover, secrecy prevents him from stating the essential technical grounds on which his view is based. Therefore, in the simple process of

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> doing his job for his country well, he is open to damaging criticism against which he is permitted to produce little defense. (Berkner 1956: 784–785)

Finally, during the Cold War, scientific involvement in governmental and military projects put the credibility of scientific knowledge at risk. In the late 1950s, the public became increasingly concerned about the growing power of the new scientific-technological elite as part of the military-industrial complex, which they considered a domestic threat to U.S. democracy (Eisenhower [1961] 2003: 414–415). Here, at last, the rhetorical power of the basic/applied distinction reached it limits. Articles on science's responsibility in the late 1950s show that public mediation between the needs of science versus society became increasingly problematic (Killian 1959b: 136; Sayre 1961; Price 1962). According to Thomas Bender (1997: 8–12), common notions of the autonomy of science—in particular the position of elitist experts and the neglect of their responsibilities—alienated science from society, evoked the impression of an academic ivory tower, and resulted in federal budget cuts for academic research.

In the mid-1960s, the distinction between basic and applied research came under severe attack because science could not meet the tremendous military and economic expectations. For example, the "Project Hindsight" study by the Department of Defense questioned the role of basic research (Abelson 1966b; Sherwin and Isenson 1967). Another study, "TRACES," challenged this questioning, reconfirming the relevance of basic research for technological development and claiming a continuing support for basic research (Thompson 1969). The debate remained ongoing, however, and the need for legitimizing basic research drastically increased. Articles and comments in *Science* document very well that the basic/applied distinction was no longer self-evident: Many commented on the "changing environment of science" (Waterman 1965), the "pressure on basic research" (Abelson 1966a), and the shifting "political tides" (Greenberg 1966). In this context, Michael Reagan (1967) asked, quite to the point, whether basic versus applied research is in actuality a "meaningful distinction." It is no coincidence that the same year, George H. Daniels (1967) published his important contribution to the historicization of science policy, describing how "pure science" functions as an ideology.

## Conclusion

Tracing the language of science policy in the United States from around 1840 to the 1960s reveals several conceptual shifts. Initially, American scientists employed primarily European concepts to make sense of science and its applications. Throughout the nineteenth century, however, they began to de-

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velop an ideal of "pure science" that was no longer in line with the European discourse. The emergence of a self-confident class of scientists, who began to distinguish themselves from commercially oriented professionals, became crucial. The way these scientists legitimated "pure science" shows that they were influenced—among other things—by an American puritanism which allowed them to interweave scientific and religious notions of truth, while their European colleagues employed the concept of pure science to battle the claim of religion determining what is right and what is wrong. Yet at the same time, the genuinely utilitarian orientation of American culture took shape in the complementary notion of "applied science," which the United States elaborated upon during the late nineteenth century and the first decades of the twentieth century.

Until the mid-nineteenth century, there were hardly any institutions stabilizing scientific endeavors in the United States. This situation triggered an existential reflection upon the role and mission of American science not monopolized by pure science advocates. For example, it was Thurston (1884: 243, original emphasis), an advocate of applied science and engineering, who stated that "there must be inaugurated a *system* of cultivation of science." In contrast, Europe had already institutionalized such a system in the forms of research universities, academies, and learned societies. For American scientists, what was at stake was the purpose of this system. For Rowland (1883a: 242), the goal was "to understand the order of the universe"; for Thurston (1884: 243–244), the goal was "the application of science to the daily work of humanity."

In short, scientists and engineers in the nineteenth century elaborated upon the supercategory of science. The emergence of new concepts alongside pure and applied science in the early twentieth century challenged this supercategory for the first time. Neologisms such as "fundamental," "industrial," and "basic research" created a new language, which we can interpret as a semantic extract separating the act of knowledge production from the more complex social systems of science and education. This is one reason why the term "science" became increasingly substituted by "research." It was for research that industry had developed an insatiable appetite: Industrial research managers considered the older notion of pure science—as it was, in their view, conducted at universities—as insufficient for covering the increasing national request for scientific knowledge. In contrast, "fundamental research" conveyed a promise to produce new scientific knowledge relevant for all kinds of applications. The prestige of science remained alluring, however, even for engineers.

The new dimension of organized research during the World War II finally underlined the relevance of scientific knowledge for the nation, first for warfare, then for welfare. As a consequence, "basic research" became the hege-

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monic concept in the postwar decades, both for the identity work of scientists and as a policy rationale for federal spending for science. As was the case with older distinctions, such as “abstract versus practical sciences” or “pure versus applied science,” the now common distinction between basic and applied research soon became fragile and contested. For a short period (from about 1945 to 1965), however, it proved an extremely successful strategy for coping with the conflicting societal and political demands on science. During this period, the old concerns about the United States’ lagging behind Europe’s basic sciences was replaced by a new concern that an exclusive alignment of science with technological applications and military-related projects could risk the sustainable advancement of science. Here, the idea of a “system” that required cultivation was crucial, as it secured the production of new scientific knowledge as well as the growth of academic offspring. We can thus summarize this new discursive arrangement after 1945 as an “argument of knowledge sustainability” (Schauz 2014: 293).

While we might, therefore, interpret the post-1945 conceptualization of “basic research” as a revival of the older pure science ideal, there was more to this new concept. “Basic research” also included a strategy to protect science and scientists against excessive claims from various stakeholders. Moreover, in this state of semantic fusion, it also shook off the former moral and religious connotations. At the same time, many associated new values with the idea: in the politically tense climate of the Cold War, older attributes from a humanistic understanding of science were converted into democratic virtues of Western civilization. To summarize, “basic research” was an ambiguous concept that conveyed a clear materialist promise yet at the same time tended to be averse to technology. It is exactly this ambiguity that has puzzled scholars dealing with the language of American politics of science.

While our chapter closes with an analysis of the heyday of basic/applied terminology in the mid-1960s, this is not the end of the story. The subsequent developments are well documented in science studies and science policy literature (see final chapter). Our aim in this chapter was to sketch the long prehistory of a discourse that mainstream literature has reduced to the period after World War II.[19] Previous studies on U.S. science policy have predominantly focused on specific events, with the publication of the Bush report in 1945 interpreted as the beginning, and the adoption of the Bayh-Dole Act in 1980 interpreted as the end, of the basic research narrative in the United States.[20] Our analysis, in contrast, shows that a longer time horizon exists and is necessary for understanding how the language of science policy evolved. This reveals that we should be careful to not mix up different historical notions, such as those associated with the nineteenth-century term “pure science” and those of the twentieth-century term “basic research.”

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## Notes

1. "The professionals distinguished between 'abstract' and 'practical' science, just as a later generation would distinguish between 'basic' and 'applied' research, or between 'science' and 'technology'" (Kevles [1977] 1995: 6). While this is not wrong, it misses the point that these different distinctions indicate actual transformations in how science and its relation to social progress were perceived and conceptualized.
2. "An Act to Incorporate the New-England Asylum for the Blind." *The North American Review* 31 (68), July 1830: 66–85, quote 67, emphasis added.
3. Most of the following quotes are not from the published manuscript, but from Tocqueville's notes and drafts. These notes have been integrated into the historical-critical edition by Eduardo Nolla, which is used as the reference here.
4. Kline (1995: 196), for example, states that the distinction between "pure" and "applied" science in the United States was not common before the 1870s, while in Britain the phrase "applied science" had existed before 1840.
5. The speech was published in *Science,* the journal established only a few years earlier by the AAAS (Rowland 1883a); in a condensed form in the British journal *Nature* (Rowland 1883b); and, addressing a broader public, in *Popular Science Monthly* (Rowland 1883c). The latter reprinted the text on the occasion of Rowland's death (Rowland 1901). Another condensed reprint can be found in *Science* celebrating the journal's one hundredth anniversary (Rowland 1980).

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6. For a more detailed discussion, see Lucier 2012. Cutrufello (2015) recently used Rowland as a case to investigate public addresses as a specific genre of scientific communication.
7. It is notable that Merton's famous text on "Science and Democracy" (1942) did not refer to any of the categorizations in use in the natural and technical sciences at that time: neither pure science nor basic or fundamental research.
8. Johnson (2008) proposes to instead write the history of science "from the perspective of applied science." Doing so, however, she reproduces the distinction of pure and applied science, which is—as a distinction—historical in character as well. By simply inverting the values, she remains within the same conceptual frame as Rowland.
9. Beside Alexander Dalls Bache, the group consisted of Joseph Henry, John Fries Frazer, Wolcott Gibbs, Benjamin Apthorp, James Dwight Dana, Louis Agassiz, Benjamin Peirce, and Cornelius Conway Felten (Beach 1972: 118–119). A slightly different list is presented by Slotten (1993: 34).
10. The role of the Scientific Lazzaroni and its protagonists are discussed by Dupree ([1957] 1986: 135–141), Miller (1970: 3–23), Reingold (1970: 163–164), Slotten (1993: 34–35), Lucier (2009: 709–711; 2012: 529–530), and Jansen (2011: 248–284).
11. In this period, the term "pure science" is used only three times (1849, 1866, and 1869). There are further some references to "pure mathematics" (1854, 1855, 1866) and "pure analysis" (1857, 1859).
12. In a similar way, Reingold (1972: 45) proposed an analytical distinction between pure and basic science: "*Basic* refers to intrinsic merit, usually scientific activities involved in formulating and verifying hypotheses and general theories. *Pure,* in contrast, refers to a psychological motivation unsullied by concerns other than the growth of scientific knowledge." Reingold, however, talks only about the nineteenth century, ignoring the fact that "basic science" as a concept did not exist at that time.
13. "Say Pure Science Lags in America: Leading Scientists and Public Men Issue an Appeal for Research Endowment," *New York Times,* 1 February 1926; "Hoover Leads Group Raising $20,000,000 to Aid Pure Science: Heads of Great Corporations Enter Campaign to Endow Research in Universities," *New York Times,* 21 April 1926; "Pure Science Study Seen at Standstill: Dr. Vernon Kellogg Warns that Industries Cannot Count on European Research," *New York Times,* 8 October 1926; "Millikan Pictures Gains by Pure Science: Modern Wonders like Radio and All Human Progress are Debtors," *New York Times,* 22 November 1928.
14. E.g., "Popular Science Monthly Award." *Science* 72 (1930): 648.
15. The concepts of "fundamental science" and "basic science" are older, but their use increased from the 1920s onward.
16. In order to get a better idea of the conceptual dissemination, here are some results of a statistical analysis of the journal *Science* (including the supplement *The Scientific Monthly*): between 1921 and 1930, the term basic research was used in 14 articles and fundamental research was used 121 times. Between 1931 and 1940, the term basic research showed up in 51 articles and fundamental research

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in 154 articles. Just five years later, the term basic research had been employed 69 times and fundamental research 182 times.

17. The question whether the social sciences should also benefit from the NSF was part of the conflicts that delayed the launch of the new funding organization. In the end, the NSF included the social sciences, but the relation between the natural and the social sciences with regard to the financial allocation remained tense (Solovey 2012).
18. A prominent example for linking individualism, democracy, and free science in contrast to the Soviet Union was Michael Polanyi (1962), the British scientist and philosopher with Hungarian roots. Polanyi's writings, which were influenced by economic theory, became even more popular in the United States than in Britain.
19. An exception is the study from Dupree ([1957] 1986), which traces the development of U.S. science policy back to the very "first attempts" starting in 1787.
20. It is a popular diagnosis that the era of basic research comes to an end around 1980. See, for example, Slaughter 1993; Johnson 2004; Forman 2007: 34, 70; Mirowski and Sent 2008: 655–658; and Shapin 2008: 87. This interpretation builds on concrete legislative changes such as the Bayh-Dole Act. The focus on language and the semantic aspect of science policy discourse does not contradict this picture, but enables a more detailed understanding of a transformation that began much earlier in the mid-1960s, where we can observe how scientists and policy makers alike began to search for new concepts beyond the basic/applied dichotomy.

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# Part II

# Conceptual Synchronization and Cultural Variation

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Chapter 4

# Fundamental Research and New Scientific Arrangements for the Development of Britain's Colonies after 1940

*Sabine Clarke*

Historians and sociologists of science have examined the shifting and contingent meanings of terms such as "pure science" and "basic research" (Calvert 2006; Clarke 2010; Gooday 2012; Schauz 2014). Exploring the function of these expressions in context has been important in disturbing any tendency to see the terms "pure science" or "basic research" as having stable meanings that have endured across time. This work has also explored the rhetorical intent behind the use of such terms and in doing so has shown concepts of "pure science" and "basic research" to be more than descriptions of scientific activities. Recognizing that categories of science have been reinterpreted at several points in the past so that they have political utility for actors has proven to be important in understanding the debates and negotiations that accompanied new funding and new arrangements for science in the twentieth century (Clarke 2010).

This chapter is concerned with the work done by the expression "fundamental research" in helping to create a system for scientific research intended to facilitate the development of Britain's colonies after 1940. New government legislation created development grants and a Research Fund of unprecedented size in 1940 to be allocated by the Colonial Office in London. Research grants were issued for the in-depth investigation of areas such as the problems of human and animal diseases, new uses for tropical products, the

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study of soils, and the improvement of cash crops, with a view to demonstrating Britain's commitment to take effective measures to develop its colonies. The creation of new funds for colonial development and scientific research was intended to restore Britain's reputation as an imperial power at a time when colonial unrest, anti-imperial critics in the United States, and German and Japanese expansionism threatened the integrity of the colonial empire. Despite its significant size and symbolic purpose, little historical work has explored the implications of the Colonial Office's Research Fund for the Colonial Empire or the research landscape in Britain.

The aim here is to account for the particular character of the new apparatus that was created for colonial research by reference to the function of the expression "fundamental research" in debates at the Colonial Office. Questions were raised in the 1940s about the type of scientific work that was needed for the development of Britain's territorial possessions. There was also disagreement over who had the authority to make decisions about the direction of research: scientific committees at the Colonial Office in London or the scientists who headed the departments of agriculture, medicine, and veterinary science in the colonies themselves. Various arguments were proposed in favor of centralized control of research at the Colonial Office that made reference to the problems in recruiting researchers, a need to improve the status of colonial research, and the importance of an efficient allocation of research money. In the course of the debate, some officials and scientists promoted the need for programs of fundamental research and endowed this activity with attributes that made metropolitan direction essential. Fundamental research was described as the investigation of widely occurring, general problems that occurred on such a scale that local supervision by individual technical departments would be inappropriate. Importantly, it was suggested that there were other types of research that were more suited to local supervision so that colonial science in fact comprised two types of research: general, fundamental research and specific and local investigations. This distinction between general and local research, rather than knowledge-generating work and the mere application of its results, helped alleviate the disquiet expressed by members of colonial technical departments that they would be denied opportunities to carry out research relevant to the problems they faced.

While the use of the term fundamental research to mean centrally directed general studies helped in negotiating the politics of colonial science, it was also clear, however, that not all investigations in the colonies were intended to have the same status. New laboratories in the colonies that were created after 1940 to address widely occurring issues were intended to have particular meanings that were not the same as the existing technical departments. In the late colonial period, there was a need for Britain to create symbols that could

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denote a modernizing process at work in the colonial territories as demonstration of the strength of Britain's commitment to colonial improvement. Colonial laboratories created with the Research Fund that were concerned with general issues that transcended the individual colony were said to allow Britain's tropical possessions to make a contribution to the advance of world science. This ability to participate in the march of international scientific progress was one way in which it was said that Britain's colonies would become part of the modern world. Importantly, only fundamental research could bestow this status; other types of scientific activity, such as research into local issues, could not promote an institution to the level of a center for world science.

When officials and scientists distinguished between work that produced generalizable knowledge and work that resolved more discrete and limited problems, they projected a hierarchy in networks of knowledge in which some types of knowledge were more mobile than others and the greater reach of some forms of knowledge was associated with higher-status work. Scholarship that has employed the ideas of circulation and networks has not considered the importance of distinctions between different species of knowledge produced by so-called Western science (on networks and circulation in science see Secord 2004; Raj 2007; Sivasundaram 2010). A focus on the way knowledge in science moves is not sufficient once we acknowledge that the expression "science" covers a number of different modes. The movement of knowledge generated by fundamental research or local research can be quite different; networks at local and global levels do not necessarily involve the same personnel, institutions, or modes of communication. A tension between the need for generalizable research or local research, rather than "pure" and "applied" as we often understand them, continues to inform debate about the activities of international research institutions in fields such as rice research (Brooks 2010; Harwood 2015). The appeal of fundamental research or its equivalent is the appeal of doing work that seeks to establish general principles of wide relevance—a contribution to new laws, principles, or big concepts in science—and this work continues to have greater cachet than studies that furnish results of relevance only on a much smaller, or local, scale, even if this work is considered to have greater relevance and utility to specific groups of farmers.

The point that issues of prestige and status can be important for the decisions that are made about the funding and organization of science is perhaps an obvious one. Less explored are the ways in which prestige and status are *produced*. This chapter explores the ways in which the idea of "fundamental research" as "general research" was used in the negotiation of issues of authority, professional standing, and colonial and national prestige in the con-

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text of discussion about the organization of research for Britain's colonies after 1940.

## A New Era of Colonial Development

In 1940, the British government passed a Colonial Development and Welfare Act (1940 CDW Act) that gave annual funds of £10 million for development in the British Colonial Empire. This was Britain's largest financial commitment to developing its colonies, and the act was renewed regularly up until the 1960s. The CDW Acts also included a substantial allocation for colonial research of £1 million each year from 1945. This made the Colonial Office the second largest government sponsor of scientific research in Britain until 1950 outside of the military sphere and also meant that the late colonial period saw Britain's greatest commitment to scientific research related to colonial problems by a considerable margin (Clarke 2007).

The need for an expansion of research activities in Britain's colonies was promoted during the 1920s, but nothing came of this suggestion because of the difficulty of securing the necessary funds from colonial and metropolitan governments. In 1938, an approach was made by the Colonial Office to the Treasury for a large research fund as recommended in a lengthy and influential volume on the state of Africa, *An African Survey.* The author of *An African Survey,* Lord Hailey (1938), produced his book after a fact-finding tour of the continent funded by the Carnegie Foundation. In *An African Survey* Hailey (1938: 1627) set out an argument that there was an urgent need for greater knowledge of conditions in Africa to ensure the success of development programs in the future: "History will doubtless look back on this period as being the most critical stage of African development; errors that are made now for lack of the knowledge which a well-considered scheme of special study might supply may well create situations which the future can rectify only at the cost of great effort and much human distress."

Hailey argued for the creation of a special research fund so that any "scheme of special study" would get the funding it needed. Calls for funds for development and money for science so as to avoid social and economic disaster in Britain's colonies in the future coincided with a moment when an acute sense of crisis existed at the Colonial Office. The Great Depression had proven disastrous for Britain's colonies, and unemployment, low wages, and the rising cost of living had led to riots in the British West Indies and elsewhere (Havinden and Meredith 1993; Harrison 2001). By the end of the 1930s, Britain faced demands from Germany for the return of its former colonies and was confronted by vocal critics of imperialism at home and in the United States. *An African Survey* was only one of a number of books and re-

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ports that pressed for reform of colonial policy, and the existence of so many critical voices proved useful in persuading the Treasury that a new commitment was needed to colonial development (Constantine 1984). Hailey's book received a great deal of publicity and had sold out by 1939, with editions released in France, Belgium, Germany, and Italy.

Apart from the need for greatly expanded funds for investigating African conditions, Hailey also spoke in *An African Survey,* and then at the Colonial Office in person, of the need for greater metropolitan direction in scientific research. He asserted that central control of research would ensure the overall coordination of scientific investigation and avoid overlap and duplication in work. Metropolitan direction of colonial research was also promoted as necessary to create greater interchange in terms of people and ideas between British universities and research institutions and colonial research centers so as to raise the standard of colonial science. In conversation with officials at the Colonial Office, Hailey suggested that a new committee was needed in London comprising eminent British scientists who would endow research in the colonies with the prestige necessary to recruit high caliber research staff.[1] The implication was that existing colonial services that worked in the fields of medicine and agriculture, for example, needed to be supplemented by new staff who were dedicated and experienced researchers with a close relationship with metropolitan centers of science. This reflected some basic assumptions about colonial science, namely that there was a danger of low-quality work in the colonies because scientists were distanced from the major centers of their disciplines and that it was difficult to attract the most ambitious and capable scientists to work in such conditions. In discussions about raising the level of colonial research, officials spoke of a need to ensure that "any research work must be of general value" and not just of "purely local interest and significance."[2] The central coordination of research by a metropolitan committee would ensure that the work done addressed the most widespread problems. This category of problem would be most likely to attract highly qualified scientists who might currently be working at a domestic institution.

The Treasury approved Hailey's recommendations, to be applied to the whole of the Colonial Empire, and new legislation that provided free grants for economic and social development and a fund specifically ear marked for research was passed in 1940. For officials, the legislation marked a turn toward a vigorous, interventionist, and ambitious program of colonial development that would be based on the data provided by scientific research (Lee and Petter 1982; Constantine 1984; Ashton and Stockwell 1996). The Colonial Office hoped that the announcement in 1940 of a large commitment to research was seen as a guarantee that the large funds now authorized for development would be put to use in an effective and meaningful way since development

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would be based on accurate knowledge about tropical conditions. At the Colonial Office, the new Research Fund was said to enable the British government to "substantiate, in as striking manner as possible, its professions of trusteeship on behalf of the subject peoples in the colonial dependencies."[3]

## Central, Regional, or Local Control of Colonial Research?

In the early years of discussions about the organization of research in the colonies, we see the term "fundamental research" being used to resolve a central question. The issue was whether the new Colonial Research Committee (CRC) in London should be the authority in research, with the power to approve research schemes, issue funds, and oversee the appointment of staff, or whether this was the role of existing technical departments in the colonies—that dealt with agriculture and medicine, for example. Heads of technical services in East Africa claimed that they should organize research for their own territories to ensure that new research funds were spent on projects that were relevant to the problems they faced. On the other hand, the CRC and officials at the Colonial Office spoke of the need for research to be centrally coordinated by a group of eminent researchers in order to raise the prestige of colonial research and facilitate recruitment of high-flying scientists. In addition, it was said that the type of project envisaged dealt with issues that were not limited to individual colonies or even a regional group and this work therefore needed metropolitan oversight. The tensions that emerged between officers of the colonial services and scientists and officials in London were resolved by suggesting that in fact there would be two types of research. One would be research into problems of general science, or issues that transcended individual colonies or regions—this was designated "fundamental research." Colonial governments would find plenty of work to be done in the investigation of more temporary, restricted, or local problems.

Based on the advice of Hailey, the Colonial Office formed a Colonial Research Committee in 1942 to oversee the spending of the Research Fund created as part of the 1940 CDW Act. The intention was to create an eminent body of leading British scientists, and the membership of the CRC included the Secretary of the Royal Society, the Director of the London School of Economics, and the heads of the research councils in Britain—the Medical Research Council (MRC), the Agricultural Research Council (ARC), and the Department of Scientific and Industrial Research. The engagement of such high-powered individuals, well known among British scientists, was intended to endow colonial research with a status similar to the work of the research councils so that scientists in British universities and research council units might consider employment in colonies. The Colonial Office gave the CRC

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a great deal of authority in organizing colonial research. It was a body that could initiate, organize, and "advise upon and co-ordinate the whole range of research in the colonies irrespective of the provenance of funds" rather than merely advise the Office in making decisions on whether schemes should be sponsored or not (Colonial Research Committee Progress Report 1943). Endowing a London-based committee with responsibility for the coordination of research activities across the whole of the Colonial Empire shifted the balance of power in the organization of colonial research away from scientists in the colonies toward the center and gave the CRC greater powers than any existing scientific committee at the Colonial Office. This was a move that proved to be controversial. It soon emerged that colonial governments had ambitions for the formation of regional research committees in East, West, and Central Africa that would plan and execute programs of scientific research across a number of territories.

In its early meetings, the CRC made a number of arguments in support of greater metropolitan supervision of colonial research. Some of these were focused on the perceived shortcomings of previous arrangements for research in the colonies. Before 1940, the medical, agricultural, and veterinary departments in Britain's colonies employed officers for work in fields such as tsetse fly and sleeping sickness control, the education of farmers, or rinderpest management. The complaint that was often made was that these technical and medical staff had a large burden of day-to-day responsibilities, such as the laboratory preparation of vaccines, and thus very little time was available for research. The result was that very little long-term and in-depth study of basic conditions was undertaken (Worthington 1938). The CRC expressed the view that "not only was research in colonial institutions often insufficiently fundamental but often there was not even a special programme of research, Many research workers find it easier to do the day to day work rather than to concentrate on more distant and more exacting work."[4] It became apparent that some members of the CRC did not have a high opinion of the abilities of members of the technical services to plan and execute research and believed a new class of experienced research worker needed to be engaged. Arguments were made for the creation of a chain of new colonial research institutes and a new Colonial Research Service in which appointments and programs of work would be supervised from London. It was reported that the secretary of the ARC, W. W. C. Topley, asserted the following at the seventh meeting of the CRC in 1942:

> He was certain that to get a good team of workers and a good institute they must be part of a central service not under any local government, with terms of service determined by their own scientific colleagues and with chances of promotion as if they had remained in this country . . . . The workers in the colonies

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> must know that they have been given their jobs by men of scientific eminence in this country and that those men would ensure them all that was necessary for carrying out their art. They must work in conditions of the same kind as they would find in universities or research institutes in this country.[5]

Metropolitan oversight of colonial research by elite representatives of research councils such as the ARC was promoted therefore as essential for the recruitment of high-caliber staff. At a meeting of another research committee that was created specificially to deal with agriculture, animal health, and forestry it was claimed that research workers were different from other types of scientists and would only submit to supervision by individuals who understood the special conditions required for proper research work: "We doubt whether anyone who has not been an active research worker for a part of his life can effectively lead a research team with the understanding and appreciation that will bring out the best of which members are capable."[6] In other words, highly qualified scientific specialists possessed a particular temperament that meant that they would not readily yield to direction by the existing heads of the Agricultural, Veterinary, or Medical Departments.

It was also said by the CRC that the nature of research itself led to a need for central oversight. Arguments for the necessity of new arrangements for colonial research were given weight by the claim that "fundamental research" was work that investigated widely occurring phenomena that were not confined to individual colonies.[7] The CRC stated in its first annual report that, in the past, research in the colonies had been "dictated too exclusively by local and temporary interests, without due regard to the scientific possibilities, or to the scale on which a given investigation must be planned if it is have any reasonable hope of success," and "It is also important to remember that the frontiers of scientific research do not coincide with political boundaries. In so far as scientific problems in various parts of the world resemble one another, the boundaries are rather lines of latitude" (Colonial Research Committee Progress Report 1943). Only a metropolitan committee could have the view from above needed to coordinate research work into major problems that occurred in different places, such as malaria and rinderpest.

The move toward the centralized control of new research programs by the CRC in London was not universally endorsed, and news reached the Colonial Office that alternative arrangements for the promotion of research had been formulated in the colonies themselves. In 1944, plans for an East African Research Council serving the East African territories of Uganda, Tanganyika, and Kenya came before the CRC, and the committee met with R. Daubney, Director of Veterinary Services in Kenya, who was an advocate of this scheme. Daubney's view was that the East African Research Council would be a body that would organize research that addressed the local needs

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of the region and that would support regional research institutes through the pooling of resources. It also emerged that plans for the regional coordination of research had been discussed by the technical departments in West Africa, and discussion was underway concerning the formation of a Central African Research Organization.[8] These bodies for the regional coordination of research accompanied a wider trend during the years immediately preceding the war toward the creation of increasing numbers of regional bodies to develop common policy and to pool information and resources. While closer union between territories in Africa in the long term, most especially political union, remained a matter of controversy, the coordination of economic and technical activities was often supported by colonial governments (Lee and Petter 1982: 88–101). The question that concerned the CRC was the degree of autonomy and authority in the allocation of funds for research that these regional research councils would enjoy in comparison to the CRC. The debate about proposals for the East African Research Council also quickly became a discussion about the type of research work that was actually needed.

In the meeting between the CRC and Daubney concerning the powers of the proposed East African Research Council, Hailey raised his concern about the ability of the council to supervise the sort of work that the CRC anticipated would be needed:

> Everyone realised that there had been a certain amount of enquiry and research undertaken by each of the separate governments involved on their own initiative and with their own funds and no doubt a combined council would be a very valuable source of advice in undertaking local enquiry and research. The difficulty was that, for instance, the Medical Research Council here might feel that it was accustomed to deal with fundamental research and had at its disposal considerable sums and a far wider field for recruiting the necessary skilled personnel than a local research council would have.[9]

Hailey's point was that the CRC comprised a direct link between the Colonial Empire and metropolitan science and so could ensure the best workers were appointed for colonial research. The MRC, represented on the CRC by its Secretary, Edward Mellanby, was also experienced in fundamental research in a way that the colonial governments were not. Daubney's response was that he did not think that fundamental research "at the very highest level" was the type of work that was needed.[10] In his opinion, the colonies needed "competent investigation of common place problems that affected economic and social development."[11] The CRC was informed that scientists in East Africa feared that the London-based committees were to dictate the nature of research in the colonies, that this work would be largely academic, and it would not be related to issues that arose at a local level or were linked to development plans.[12]

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The ensuing debate produced a number of attempts to define the type of investigation that was needed for the resolution of the problems of Britain's colonies by reference to the categories of "fundamental" and "applied" research. It became apparent that there was very little shared understanding or agreement about the exact definition of these terms. Confusion over the meanings of the two expressions raised questions about the appropriate location of research—if "fundamental research" in medicine meant molecular biology, then surely it could only be done in the laboratories of America or Europe? But if "fundamental research" referred to the determination of a cause of a disease, then it needed to be done in the field. Hailey suggested that the primary question was which body should have responsibility for apportioning research funds, the London-based CRC or the projected council in East Africa. It was decided that the priorities of the two authorities were different. While colonial governments were primarily concerned with the needs of a region, metropolitan bodies sought knowledge of the empire as a whole and were concerned with a wider scientific field. This meant that both the CRC in London and the East African Research Council were concerned with research, but of slightly different types. This resolution was endorsed by the Colonial Office's Financial Advisor, Sydney Caine, who suggested, "In practice the difference between "fundamental" and "applied" research would probably work out as the difference between those things in which the central body took the greatest interest and those in which the local council took the greatest interest."[13] Fundamental research in this vision was concerned with the issues that transcended individual colonies or groups of colonies and required metropolitan oversight. Applied research was not the application of results gained elsewhere but dealt with a different scale of problem, one that arose in the course of the work of the technical departments and did not require centralized coordination.

A distinction between centrally administered research and locally administered research was taken up, and it informed the new arrangements for colonial research that were introduced during the 1940s and early 1950s. Charles Carstairs, head of the Research Department at the Colonial Office, explained the two-tiered operation of colonial research in a letter to the Chief Secretary of the Central African Council in 1946:

> A more useful distinction perhaps [than between fundamental and applied research as normally understood], is that between the scientific and experimental work which is necessary for the day-to-day discharge of the responsibilities of agricultural and other technical departments, and the long term work bearing on the basic problems of a given area which is best conducted on a semi-independent basis and without regard to immediate results or the solution of short term problems.[14]

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The committees in London would oversee long-term work into basic problems, and the colonial administrations would be responsible for day-to-day work. The expectation was that the Research Fund would be the source of monies for centralized research, while the research done by the colonial governments would most likely be funded through the normal routes.

By 1952, around forty new research institutes had been created across the Colonial Empire using the Research Fund, and a Colonial Research Service (CRS) was created for scientists who would work in these laboratories. Significantly, both the research institutions and the specialists who joined the CRS were under metropolitan direction and did not report to colonial governments. It was said at the Colonial Office, "The regional institutions must not be slaves to local problems."[15] This meant in practice that the technical departments did not determine or vet the research programs of the new colonial laboratories, nor were they concerned with appointing the staff who worked in them. While medical, agricultural, and veterinary departments in regions such as East Africa were free to pursue their own projects, they found that institutes were created in their midst over which they had little control. The distinction between fundamental research and local research produced new apparatus in which there were two parallel systems for science in the colonies. While communication between the two was anticipated, one was not dependent on the other, so the distinction that was being employed between general fundamental and local research effectively severed any necessary relationship between the scientific work of the colonial governments and that organized by London. By rejecting a fundamental/applied research distinction in which fundamental research produced knowledge that was then utilized by technical officers, the Colonial Office avoided the implication that one area of work was the essential precursor for the other.

The key outcome of the conceptualization of research as comprising two species of investigation—general versus local—was the particular architecture created for colonial science after 1940 in which two systems coexisted and one of these was overseen from London. It did not mean that the investigations done in colonial laboratories after 1940 addressed only fundamental or basic issues, or that this work was academic, far from practical issues, or only concerned with advancing a particular scientific field. Research policy in colonial agriculture, animal health, and forestry focused on the resolution of short-term problems after 1948, partly in response to a wider British government drive to rapidly increase the volume of primary products in the empire in the face of a shortage of goods and dollars. The research agenda of colonial laboratories in other fields also often had an overt practical focus. The utility of the distinction between fundamental research as general research, coordinated across the whole Colonial Empire, and more narrowly defined

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investigation was in producing new arrangements for colonial research that incorporated a desire to see closer relationships between the investigations in the colonies and metropolitan science while not disenfranchising existing scientists in the technical services.

## A World Center for Science

The range of new research institutions created from the Research Fund after 1940 included the Colonial Microbiological Research Institute (CMRI), established in Trinidad in 1948. This laboratory had a practical and a symbolic function and the term "fundamental research" was important to the negotiation of both. The formation of this research institute illustrates well the rationale that was employed to explain why new laboratories could not fall under the jurisdiction of colonial governments. It also enables some exploration of the relationship that emerged between centrally coordinated research and the problems of colonial development after 1940.

The CMRI was established by the Colonial Products Research Council (CPRC), one of the research committees formed at the Colonial Office to oversee the spending of the Research Fund of the 1940 CDW Act. The CPRC sponsored a range of research projects at British universities into areas such as the chemistry of sucrose. During the 1930s, the price of cane sugar collapsed on the world market, resulting in low wages, high unemployment, and riots on the sugar estates of places such as Jamaica and Trinidad. In the search for a long-term solution to the problem of a sugar industry in decline, the Colonial Office decided to support scientific research that would identify new uses for sugar as a fuel or a raw material for the chemical industry, and this program was then expanded to include research into new uses for a range of tropical products. In the body of their annual reports, the CPRC laid much emphasis on the fundamental nature of the work they sponsored into tropical products, saying, for example, about investigations into eugenol, a derivative of cloves grown in Zanzibar, "It has been recognised from the outset that it would prove extremely difficult to find new uses for eugenol and its derivatives and this can only result from fundamental research" (Colonial Products Research Council 1948). This work was defined as investigations that explored the general, basic chemical reactions of a compound: "It was recognised that the experiments having as their object the finding of alternative uses for eugenol and vanillin were highly speculative and the most promising lines of attack lay in the study of the general chemical reactions of these substances" (Colonial Products Research Council 1946).

The CMRI created in Trinidad was described as a laboratory that aimed to undertake "fundamental research in microbiology whilst also helping to

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improve certain industrial processes."[16] Along with work focused on establishing general principles, the CMRI carried out investigations with a clear practical value such as examining the use of surplus sugar as a medium for the fermentation of food yeast. Food yeast was a product that was high in vitamin B that the British government was producing in a factory in Jamaica in order to address problems of malnutrition in the colonies and in Europe at the end of World War II. In addition, the CMRI was inspired by the discovery of streptomycin to survey the microbes in tropical soils for antibiotic effects and maintained a reference collection of bacterial cultures for consultation by business and universities. The laboratory also studied Panama disease in bananas, investigated the microbiological processes that occurred during cocoa fermentation, and looked to find uses for waste products generated during rum distillation. When the CMRI was described as an institution for "fundamental research" in microbiology, it did not mean that all of its investigations were long term and exploratory, or far removed from the concerns of colonial industry.[17] The designation of "fundamental research" helped create arrangements that placed the research agenda of the CMRI under the control of the director of the laboratory, A. C. Thaysen. This meant that Thaysen had the authority and freedom to direct the scientists at the CMRI to undertake long-term study of basic underlying issues if that was deemed appropriate. It did not mean that the CMRI would completely ignore more practical problems.

In discussions that immediately preceded the creation of the CMRI, the suggestion arose that administration of the laboratory might be devolved to the Trinidadian government so that the colonial government paid the scientists at the institute and they would therefore only incur local rather than U.K. rates of tax. In this arrangement, a member of the Trinidadian legislature would be nominated to attend meetings of the CMRI. This suggestion prompted a furious response from Thaysen. He complained to the Colonial Office that such arrangements were inappropriate when the CMRI was charged with investigating issues that went beyond the concerns of Trinidad alone. "It is intended that this research institute should be a centre for microbiological research not only for the Colonies but for the Commonwealth as a whole," he wrote.[18] Thaysen argued that this international standing of the CMRI would be compromised if the laboratory was attached to the Trinidadian government. So far, he said, "I have been recognised as an outsider, working, not under instructions from the local authorities, but on behalf of the Colonial Office which in starting our institute has had the interest at heart, not only of Trinidad or the West Indies, but the whole Tropical Empire."[19]

Thaysen was very concerned with the public perception of the CMRI and worked hard to promote the laboratory as a high profile site of international science. He insisted that the laboratory have a prominent location, rejecting

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a site at the Imperial College of Tropical Agriculture in central Trinidad and instead locating the CMRI in the capital, Port-of-Spain, saying he "wished to have his laboratory sited centrally and conspicuously so as to show people that colonial research means something."[20] He commissioned the architects W. H. Watkins and Partners to produce plans for the laboratory and the modernist design was subsequently featured in an article in an architectural magazine, *The Builder,* in September 1948. Thaysen also promoted the CMRI in the Trinidadian press. The institute was the subject of numerous articles and reports during the 1940s and 1950s in Trinidad's daily newspaper, *The Trinidad Guardian.* In the publicity surrounding the formation and opening of the CMRI, the international standing of the institute was emphasized over and over again. Shortly after arriving on the island in 1947, Thaysen informed *The Trinidad Guardian* that the CMRI was not purely a Trinidadian institute but would be serving the empire.[21] In 1948, the newspaper was told that no individual colonial government was paying for the institute, and therefore controlled it, but it "was a completely British Government venture."[22] In one of a number of articles to mark the opening of the CMRI titled "Micro institute to be world centre," the writer told how the CMRI would "put Trinidad on the scientific map" and quoted Thaysen as saying, "No effort has been spared, in fact, to make the Institute worthy of British science and of its position as a world research centre." The writer then followed this point with this declaration: "The Trinidad Institute is we believe the only one of its kind in the British Commonwealth and one of the few such institutions to be found anywhere in the world. Its importance is attested by the number of distinguished scientists and other guests who have assembled for the opening and this Colony is fortunate to have been chosen as its home."[23]

The message was clear, the function of the laboratory was to tackle microbiological problems of international importance, and this gave the institute great cachet. Trinidad would have the honor of acting as host to a world center for scientific research. Evidence that the CMRI was a world center for tropical microbiology was supposedly found in the eminent scientists that came to work at the laboratory from overseas. Thaysen was described in *The Trinidad Guardian* as a "scientist of the top rank," and the arrival of new colleagues W. C. Forsyth, from the Macaulay Soil Research Institute in Aberdeen, and J. E. Rombouts, from the Netherlands, both received coverage in the Trinidadian press. While describing such individuals as "top grade staff," the newspaper also raised the question of whether there would be room for Caribbean staff at the institute: "The chance to share in work of such an important nature should not be missed by talented and suitably equipped young men and women in the Caribbean." Thaysen's response to the enquiry about employing local staff was, "You must remember that the institute to be formed is not

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purely a Trinidadian, but an Imperial affair. West Indians will be as welcome as anyone else." In other words, there might be room for Caribbean staff, but since the CMRI was a world center, scientists from the West Indies would not be prioritized over workers from elsewhere.[24] This remark gives an indication of the tension that could be produced with the creation of international status for new laboratories. These institutions might be symbolically important, but that did not mean they gave opportunities or other concrete benefits to local colonial populations.

The emphasis on centrally coordinated fundamental research had previously led to questions about attitudes of colonial peoples to the agricultural laboratories that were being created in their midst. The Colonial Office's Agricultural Advisor, Harold Tempany, questioned whether new arrangements for fundamental research would mean this work was valued by the inhabitants of Britain's colonies. According to meeting minutes, "he thought that central control might well prove deadening rather than enlivening to research generally and it would certainly make it very difficult to put across the results. The public in the colonies themselves would not feel the same sense of ownership and responsibility for what was going on, even though the quality of research was somewhat higher."[25] This reservation was brushed aside by the scientists on the CRC with the assertion that metropolitan oversight of research was necessary if high caliber scientists were to be recruited for colonial research. The secretary of the ARC, W. W. C. Topley, had the final word: "To get a good team of workers and a good institute they must be part of a central service not under any local government."[26]

The international standing of the CMRI was intended to enhance both the status of the laboratory and Trinidad and be a source of pride for local people. It was intended to be a symbol, signaling Britain's commitment to developing and modernizing its colony. If the CMRI was to denote a nascent modernity for Trinidad, then it was because the laboratory was able to transcend its locality and participate in the global circulation of knowledge. Not all knowledge produced in Britain's colonies was equally mobile. Some forms of knowledge might have only limited use, and, while valuable, this knowledge could not denote the modernity and high status for a colony that the Colonial Office was seeking in its sponsorship of scientific research. Fundamental research, however, produced knowledge that was likely to circulate widely as it explored the most basic and universal scientific phenomena. The ability to participate in the international advance of science could make a place like Trinidad part of the "modern, atomic-age world," in the words of a pamphlet published in 1956 advertising the work done under Britain's CDW Acts (Lewis 1956).

The fact that the international character and prestige of the CMRI was emphasized more often than the utility and relevance of its research work

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demonstrates the extent of official concern with the public perception of British government actions after 1945. This confirms the point made by historians that the new colonial policy inaugurated by the CDW Act of 1940 was considered to have an important role in improving Britain's reputation as a colonial power (Constantine 1984; Havinden and Meredith 1993). In the second half of the 1930s, the British government had found itself having to defend its reputation as an imperial power in the face of vocal criticism by commentators in the United States and at home. There were revelations of high levels of malnutrition and disease and appalling housing conditions in many of the British colonies, and these were ammunition for those who wished to see an end to British imperialism or, as in the case of Germany, wished to have former colonies returned to them. Highly visible British interventions to modernize and improve economic conditions were particularly necessary in the British West Indies, as they had been the site of widely publicized riots in the 1930s. The British Caribbean was also a region where British imperialism was most open to U.S. scrutiny at a point where there were strident anti-imperialist voices in America (Constantine 1984). The issue that arises is whether the search for status through the creation of institutions of fundamental research led to the neglect of problems more pertinent to the development of individual locales. In addition, while the Colonial Office appeared to believe that colonial populations would appreciate the creation of laboratories in their midst that participated in the international advance of science, we can wonder about the sense of ownership that these institutions fostered in practice.

The CMRI was intended to be a highly visible demonstration of Britain's commitment to colonial modernization. It worked on a range of problems related to diseases of agriculture and the discovery of new drugs, but it had few successes in practice, and there is little evidence that it fulfilled the ambitions of the officials and scientists that were involved in its creation. The Colonial Office believed that the laboratories it created in such large numbers after 1940 would be a permanent legacy for the colonies after independence. It believed that siting institutions like the CMRI in British-controlled territories would teach the local inhabitants the value of scientific research. The reality was that, while medical laboratories often continued, many of the other institutions did not survive. While the Colonial Office and scientists in its employ worked to shape the meanings of these laboratories as emblems of a modernizing program it was clear in many cases that the focus on improving cash crops for export, for example, meant these institutions were firmly redolent of the exploitative nature of British imperialism for independent nations (Roseboom, Pardey, and Beintema 1998). The beneficiaries of the work of the CMRI were most likely to be British or American-owned businesses, so it

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is perhaps not surprising that there was little interest in continuing its work after 1962 when Trinidad gained its independence.

## Conclusion

After 1940, officials and scientists at the Colonial Office in London considered an expansion of scientific research across the Colonial Empire. In their discussions, they often referred to the need for a focus on programs of fundamental research. The term was not usually deployed in the same way as the older expression "pure science," which most frequently was used to denote work done without concern for application. Instead, "fundamental research" was used to refer to the investigation of general principles, or the most widely occurring phenomena, in contrast to more discrete problems. It was said to require administrative arrangements that provided a "view from above" and circumvented local oversight. By engaging with the ways that this term was being deployed after 1940 in discussions of the colonial empire, we can gain an insight into how actors negotiated issues of status and prestige that were important to them. Key was the claim that fundamental research produced knowledge that could contribute to science at an international level, giving stature to the workers, institutions, and even colonies engaged in this work. Fundamental research was general, imperial, international, or global science. It was work that produced the most mobile forms of knowledge.

While historians have considered the social and material factors that were important for the movement of knowledge in the past, this paper has been concerned with the rhetorical and symbolic functions of the idea of the circulation of knowledge. An examination of the science/development discourse that accompanied the creation of large numbers of new laboratories in the late colonial period shows that these institutions had meanings that worked on more than one level. The value of colonial laboratories was said to be their role in providing the necessary information for colonial development through fundamental research. In practice, the way in which knowledge generated by these institutions might inform the other functions executed by either metropolitan government or colonial administrations was not well articulated or apparent. The creation of colonial laboratories as semi-autonomous institutes that that undertook work that transcended the preexisting functions of the technical departments of the colonies and were overseen by metropolitan committees seemed to run counter to claims about the utility of an expansion of fundamental research. The new arrangements for fundamental research can be explained, however, when we realize that these laboratories also had a symbolic function—acting as both emblems of Britain's modernizing in-

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tentions and also representing special arrangements where scientists had full control over their research. The mobility of knowledge that fundamental research produced was related to wider issues of professional status, visions of colonial development and incipient modernity, and national reputation and self-esteem.

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## Notes

1. National Archives, London, U.K. (NA), CO 847/13/13, "Central direction of research."
2. NA, CO 847/13/13, "Central direction of research."
3. NA, CO 847/13/13, "Central direction of research."
4. NA, CO 900/1, First meeting of CRC, 29/6/42.
5. NA, CO 900/1, Seventh meeting of CRC, 16/11/42.
6. NA, CO 927/88/5, "General policy."
7. NA, CO 927/88/6, "Agricultural Research Policy Sub-Committee: organization of colonial agricultural research."
8. NA, CO 927/3/1, "Regional co-ordination of research: East Africa."
9. NA, CO 900/2, Minutes of the 23rd meeting of the CRC, 23/3/44.
10. NA, CO 900/2, Minutes of the 23rd meeting of the CRC, 23/3/44.
11. NA, CO 900/2, Minutes of the 23rd meeting of the CRC, 23/3/44.
12. NA, CO 927/2/6, CRC 137, "Social Research in East Africa: note by Secretary," 26/10/44.
13. NA, CO 900/2, Minutes of the 23rd meeting of the CRC, 23/3/44.
14. NA, CO 927/3/4, "Regional coordination of research: central Africa."
15. NA, CO 900/1, Minutes; CO 927/2/6, "Regional coordination of research: East Africa."
16. *Nature,* 25 September 1948, p. 162.
17. *Nature,* 25 September 1948, p. 162.
18. NA, CO 1042/132, "Establishment of Colonial Microbiological Research Institute in Trinidad."
19. NA, CO 1042/132, "Establishment of Colonial Microbiological Research Institute in Trinidad."

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20. NA, CO 1042/132, "Establishment of Colonial Microbiological Research Institute in Trinidad."
21. "Microbiological institute to get top grade staff," *The Trinidad Guardian,* 12 January 1947.
22. "Chemist from Scotland to Join 'Micro' Staff," *The Trinidad Guardian,* 2 July 1948.
23. "Micro Institute will be World Centre," *The Trinidad Guardian,* 4 July 1948.
24. "Microbiological Institute to get Top Grade Staff," *The Trinidad Guardian,* 12 January 1947.
25. NA, CO 900/1, Minutes.
26. NA, CO 900/1, Minutes.

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Chapter 5

# Basic Research in the Max Planck Society

## Science Policy in the Federal Republic of Germany, 1945–1970

*Carola Sachse*

The Max Planck Society for the Advancement of Science (MPG, Max-Planck-Gesellschaft zur Förderung der Wissenschaften) is commonly considered a stronghold of basic research in the German research landscape. When it was reestablished in 1948 in the Western zones of occupation, the society adopted the defining part of its predecessor's name, the Kaiser Wilhelm Society for the Advancement of Science (KWG, Kaiser-Wilhelm-Gesellschaft zur Förderung der Wissenschaften), which had been founded in 1911. In contrast to its organizational counterpart, the Fraunhofer Society for the Advancement of Applied Research (Fraunhofer-Gesellschaft zur Förderung der angewandten Forschung), whose distinctive purpose has been incorporated into its name since its foundation in 1949 (Trischler and vom Bruch 1999), the MPG did not include the term "basic research" in its full name, nor does the term appear in any of its statutes of the last one hundred years. The official documents of the MPG instead refer to "scientific research" in the broad German sense of *wissenschaftliche Forschung* and combine it in varying formulations with the terms "freedom" and "independence."[1]

Both of these terms are cornerstones in what became known as *Harnack-Prinzip* (Harnack principle). Named after Adolf von Harnack, the initiator and first and longtime serving president of the KWG, this principle included two main aspects: first, institutes should be founded only if a man was available (women were not considered) for the post of managing director "who has

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proven himself as an excellent researcher in the field of experimental science through his great successes" (Harnack [1909] 1961: 88). Second, having the privilege of being appointed for life, a director should have the freedom to choose his research topics, methods and his scientific support staff, as well as possible scientific and industrial cooperation partners, independent of government, industrial sponsors, or directives from executive committees at the KWG/MPG (Vierhaus 1990; Harwood 1994; Laitko 2014). Thus freedom and independence of research were not understood as a democratic or an epistemological principle, but as a meritocratic distinction of the chosen few.

Like its predecessor, the MPG is neither an assembly of scholars like the academies of sciences, nor is it linked to a university. Instead, it is an additional type of institution dedicated exclusively to research. In the mid-1940s, the KWG comprised approximately forty institutes and research centers, which were scattered across Germany. In the last months of World War II, however, most of them were evacuated to the Western parts of the country. Thanks to the support of British scientists and occupation officers, as well as representatives of the Rockefeller Foundation, nearly all of them could be reintegrated into the MPG, even against profound reservations maintained by the U.S. military government in the early postwar years. In spring 1949, the so-called Königstein Agreement laid down the structure of the West German science system and regulated the responsibilities of *Bund* (federal government) and *Länder* (states) for funding science and research (Pfuhl 1959). Since then, the MPG has been financed mainly from state budgets, and operates as the institution responsible for "basic research" in the Federal Republic of Germany's new division of scientific labor. The number of member institutes grew substantially in the late 1950s and during the 1960s, then again after 1990 in the course of the German reunification process; in recent years it has leveled off at around eighty institutes.[2] The institutes undertake research in a very wide range of fields, mainly within the natural sciences but also in law, the humanities, and the social sciences.

Against this historical background, the MPG describes itself on its homepage as "the most successful institution of basic research in Germany," and emphasizes this claim with a programmatic quotation from Max Planck: "Insight must precede application."[3] This slogan evokes the notion of what later became known as the "linear model" of knowledge transfer from basic research at universities or independent institutes such as the Max Planck Institutes (MPI) to applied research and development in industry. However, by focusing this idea, the MPG seems to disregard the fact that the linear model has been for a long time subject to dispute and severe criticism within science studies, as well as in German science and technology policy.[4]

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Furthermore, the allegedly self-evident term "basic research" is often used casually and inconsistently in documents issued by the MPG. For example, in a Festschrift published to celebrate the MPG's one hundredth anniversary (Gruss and Rürup 2010), the then acting president, Peter Gruss, attributed "heterogeneity and lively diversity" to research done at MPG and acknowledged, suitable to the occasion, a history "of more than 100 years of successful basic research" (Gruss 2010: 11). While some authors completely avoided the term in the following portraits of selected institutes, others, quite naturally, affirmed that MPIs had conducted "basic research" from the beginning or had placed their focus on it later on. These portraits included the Institutes for Metals Research (MPI für Metallforschung, Stuttgart; Maier 2010: 332), Iron Research (MPI für Eisenforschung, Düsseldorf; Flachowsky 2010: 134), and Plant Breeding Research (MPI für Pflanzenzüchtungsforschung, Köln; Heim and Kaulen 2010: 357), which had originally been founded in close cooperation with industry in the last months and in the aftermath of World War I. In the same Festschrift, historians of science characterized the research programs of such institutes (with some restrictions) as *angewandte oder anwendbare Grundlagenforschung*—translated by the authors as "basic applied science" and "applied or applicable basic research" (Renn and Kant 2010: 75, English in the original). They legitimized the venerable status of these MPIs by referring to their continuously demonstrated innovative potential. The question of how useful basic research was *allowed* to be at the MPG was of great importance after 1945, but was no longer being addressed in the anniversary year 2011. The evidently more pressing question of how useful basic research at the MPG *needs* to be—in today's times of bitter competition for limited research funds—was met with a quotation that is ascribed to Einstein, and that popularizes Planck's previously quoted leitmotif: "If we left research exclusively to engineers, we would end up with a perfectly functioning kerosene lamp, but without electricity" (Renn and Kant 2010: 78; transl. C. S.).

In the last two decades, the history of science has increasingly concerned itself with the significance of the sciences in National Socialism and Stalinism, and based on this, has discussed the relationship between politics and science in modern societies, partly in a comparative mode (e.g., Ash 2002; Walker 2003; Sachse and Walker 2005; Trischler and Walker 2010; Roelcke 2010). In this context, the dichotomy between "basic" and "applied research" was often uncritically employed as a heuristic tool to analyze the complex interrelations of science, the economy, and politics in industrialized societies. Only recently, historians of science have been showing an increased interest in the origin, the historical contexts of use, and the varying semantic meanings of this conceptual pair, as well as the different objects for whose denomination this pair has been employed. The historians involved in these discussions

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principally agree on the historicity of the two terms, each of which have their own terminological traditions that go back to different times and places in history.[5]

Recent studies analyzing the emergence of basic or fundamental research shed light on how the new concepts contrasted to the older term of "pure science" and its antonym, "applied science." In fact, the English terms "fundamental research" (or "basic research," as it was increasingly called from the 1940s on) and "applied research," as well as the German terms *Grundlagenforschung* and *angewandte Forschung,* have not always appeared together as a dichotomous conceptual pair. The way the new term "fundamental research" was used first in 1895 in the context of funding plant breeding research in the United States aimed rather at overcoming the old dichotomy between pure and applied science. Only a few years later, the British Department of Scientific and Industrial Research (DSIR), founded in 1916 to counter Germany's investment in research for military purposes and encourage the British industry to invest in research, deployed the new term in a similar way (Clarke 2010; Gooday 2012: 553). The adjective "fundamental" differentiated the research both from "pure" scientific curiosity and from industrial research, and described it as epistemologically important and practically useful at the same time. It was supposed to only slightly, but not principally, diverge from applied science in that its research findings were not directly applicable industrially and in that its research questions were relevant in a broader kind of context. It was not until the end of World War II that the terms "fundamental research"—or, as it was going to be called from then on, "basic research"—and "applied research" would be used in a dichotomous, delimiting way again.[6]

In this chapter, I follow this strand of research. I assume that the meaning of "basic" and "applied research," as well as the relationship between the terms and the objects they signify, has to be understood as historically variable. At the same time, it is not sufficient to expose the discourse on basic research as a rhetorical strategy in historical-political contexts. With regard to the MPG, this is evident and can be easily illustrated in the phase of its transformation after the collapse of the so-called Third Reich and its subsequent dissociation from Nazi research policy. Such a perspective would, however, not allow any conclusions on how what was accepted as basic research and what was marginalized as applied research were related to each other, how the objects bearing different denominations and the communication about these developed, and which institutional, organizational, and epistemically relevant decisions and actions were involved in this process.

The dichotomous use of the terms "basic" and "applied research," which became common in the first decades of the Federal Republic of Germany

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(F.R.G.), should be understood as part of a broader discursive practice. When seen against the entanglement of science and politics in Nazi Germany, on the one hand, and the dropping of the atomic bombs on Hiroshima and Nagasaki, on the other, this discursive practice redefined the concept of scientific research, whose promotion had been the stated aim of the KWG/MPG since its founding in 1911. It established new scientific, institutional, and epistemological frameworks, which gave the MPG a position in the scientific landscape of the F.R.G. that was different from the one occupied by the KWG during the National Socialist (NS) regime. In this context, the distinction between basic research, which fell within the purview of the MPG, and applied research, which was to be carried out by other institutions, functions as a master signifier that defines both the administrative organization of scientific research in postwar West Germany and its symbolic meaning. That is, this distinction functions as what Lutz Raphael (2008) and Christoph Dipper (2010: 9) have described as an *Ordnungsmuster,* an ensemble composed of semantics and patterns of perception and experience, which guides the interpretation and design of elementary social processes; in our story, the most important social process is the growing relevance of scientific knowledge in the twentieth century.

To this day, it remains a challenge for the MPG to present itself internally as well as externally to the public, the media, and its patrons as a wellspring of basic research, no matter how the political, economic, and epistemic constellations of knowledge production may change. This chapter, however, focuses on the first two postwar decades, during which the leading representatives of the KWG/MPG, threatened by dissolution in the early postwar years, were forced to reinvent themselves and their institution. In this process, "basic research" functioned as a key argument, which was not tied to a definite idea, let alone a concept that was shared by all stakeholders. Nonetheless, strategically well placed, this argument could decide whether a specific research area was admitted, continued, or excluded from the MPG—it worked as an *Ordnungsmuster,* a symbolic and administrative formation in the previously mentioned sense.

In the following section, I will explain why the three parties that had been arguing about the future of the KWG/MPG from 1946 to 1948, namely the West German scientists, the British occupation officers, and their American counterparts, sang the song of "basic research" together, even if their voices were far from being harmoniously in tune with each other. In the last section, I will describe some episodes from the 1950s and 1960s in which discussion revolved around the question whether a specific research field could assert itself as basic research or not. These episodes illustrate, on the one hand, that the distinction between basic and applied research as an *Ordnungsmuster* did,

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in fact, penetrate into self-perception, self-description, and decision-making processes. On the other hand, they demonstrate that this development was not a self-driven process, but often required strenuous terminological efforts, which rarely ended in consistency, but nevertheless had a decisive effect on shaping reality.

## "Basic Research" as the (Re-)Founding Myth of the MPG

From the foundation of the Kaiser Wilhelm Society in 1911 until the end of the Second World War, the term "basic research"—or "fundamental research," according to the British meaning as coined in World War I—would have been adequate to describe the KWG's program and a large part of its research: both were conceptually hybrid and oscillated between pure and applied research, just as the KWG and its institutes held an intermediary position between academic and industrial research. The reason the term was not included either in the society's statute from 1911 or in later revisions in the interwar period is probably that, at the time, there simply was no need in Germany (in contrast to Great Britain and the United States) to accurately differentiate the research fields of the KWG from those related to industry, on the one hand, and from those that were funded publicly and investigated at universities and academies, on the other. The latter might have been suspicious in regard to a new rival in the competition for public funds, but industry, military, and state power agreed—at least in principle—on the necessity of nonuniversity and nonindustrial scientific research. They also agreed that this research should be institutionalized as an independent society, and financed by a combination of public and private endowments—a consensus that persisted beyond the German Empire and the First World War, while the proportion of public funds made available for this type of research grew continuously (Hachtmann 2007: 56–59, 81–258).[7] In negotiations within the NS regime, of which the KWG formed an integral part, the argument of basic research (if used at all) was only helpful when governmental financiers could be convinced that the respective research conducted at the KWG would have an actual or potential application, or rather, as the war progressed, that it was somehow relevant for armament and warfare. Here, the aim was to place the KWG's projects as high as possible within the ranking of levels of military urgency (Schmaltz 2005; Maier 2007).

With the collapse of the so-called Third Reich and the occupation of Germany by the allied forces, other terminological accentuations became appropriate. First, the meaning of "basic research" had experienced another shift since the 1940s. The flexible English term had again become part of a conceptual pair that was constructed dichotomously and formulated within military

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jargon: basic research, understood as "long range strategic investigations," and applied science, understood as "immediate tactical investigations" (Clarke 2010: 305). "Basic research" assembled notions of the older term "pure science," which had been understood as a purely knowledge-oriented, disinterested, and self-sufficient type of research. Additionally, it comprised new meanings related to the early period of the Cold War, in which a "democratic" and "free science" was contrasted with "totalitarian," "communist," and "planned science." The latter was described in a way similar to science in the National Socialism and understood to be bound by ideological guidelines and societal (or rather military) interests.[8] Even if one tried to denigrate the research conducted by the opposing party as being "unscientific" or "pseudoscientific," the potential risks it posed were still to be taken seriously (Thiel and Walther 2008).

After World War II, influential political actors in the United States were of the opinion that Europe, and Germany in particular, should no longer be a location for research that could have any kind of military consequences. The KWG, which was viewed as having greatly contributed to the scientific advancement of NS armament and particularly to the regime's nuclear project Uranverein (uranium club) was thus to be closed down without replacement. Other American voices, especially from the natural sciences and the Rockefeller Foundation, as well as the British side, opposed this view: they argued that the West could not dispense with Europe's intellectual potential.[9] In order to conciliate the more radical U.S. positions regarding the future of research in Europe, they differentiated systematically between basic and applied research—against better knowledge of the inseparable interrelations between insight and interest: applied research, whose technical inventions could possibly become a threat, was, for the time being, to be conducted only on the American continent—a safe distance from the European scene of the Cold War. The domain of basic research, however, was not only suitable for flying the flag of freedom of science against the Eastern bloc; there was also no risk. The massive technological advantage of the United States would guarantee that European researchers would not be able to enter the hazard zone of technological application themselves. Thus U.S. researchers would be able to exploit Western European competences, including those of the KWG/MPG. Moreover, as John Krige (2006: 11, 3) has shown, promoting basic research in Western Europe by grants, exchange programs, and delivery of isotopes and scientific instrumentation was not only meant to help strengthen "long-term economic prosperity of the Continent" and thus stabilize the anticommunist bloc, but also to "reconfigure the European scientific landscape and to build an Atlantic community with common practices and values under U.S. leadership."

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The MPG's president Otto Hahn and his secretary-general, Ernst Telschow, took up this line of argumentation. Ultimately, their fight against the winding-up order that had been imposed by the U.S. occupying force was successful, the result being the reestablishment of the MPG in the first bizonal, and later trizonal, area of West Germany.[10] In a position paper from February 1947, which was directed at the head of the American military government, General Lucius D. Clay, Hahn and Telschow drew up an organizational profile for the future of the MPG in the F.R.G.[11] This profile was, however, not the result of a critical evaluation of the MPG's previous research programs and their respective involvement with three political systems between 1911 and 1945. Instead, it was a reaction to the three central allegations which the American occupying force (whether rightly or wrongly) had brought forth against the MPG: first, the incorrect allegation that the KWG had been a tightly structured "research trust" (*Forschungs-Trust*) was countered by emphasizing the institutes' scientific autonomy. However, the idea of autonomy was more or less restricted to the "Harnack principle," according to which the directors were free to choose their areas of research and free to adjust the respective extent to which they cooperated with industry, government, and military. The second accusation, which Hahn and Telschow rejected, was the inappropriate interpretation that the KWG had lost its independence in 1933 and had, from then on, served as a henchman for the National Socialists. Instead of accurately explaining the KWG's involvement into the NS regime, this accusation was turned around and used offensively to argue for a future institutional independence of the MPG from any political authority. Finally, the correct allegation that the KWG had actually conducted military research was countered by assuring that even during wartimes, the KWG had, as had always been its tradition, concentrated on *Grundlagenforschung* (basic research). The following detailed explanations of what exactly this meant alluded to the complex contemporary understanding of basic research in the early postwar era.

On the one hand, it is stated in the paper that whenever problems were tackled that were relevant for the war economy, the reason was that they were "of scientific importance" (*von wissenschaftlicher Bedeutung*) as well. This clearly reflects the British understanding of basic research as long-range strategic research. On the other hand, Hahn and Telschow referred to the scientific commissions that had been sent by the allied forces to inspect the Kaiser Wilhelm Institutes (KWIs). Those, they stressed, had noticed "with astonishment to what high extent . . . pure research work (*reine Forschungsarbeit*) had been done" and how minor the proportion of "war work (*Kriegsarbeit*)" had been (Sachse 2009: 124–128).[12] At this point, the older concept of "pure science" was revived and added to the British understanding of "basic

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research"—a combination that shortly after would be used to celebrate the "freedom" of science in Western democracies.

Generally, this reaction to the American allegations conformed to the discursive field of transatlantic science policy in the beginning Cold War. At the same time, it formulated specific arguments for the future positioning of the MPG within the West German research landscape. The paper constructed a conceptual triangle consisting of basic research, scientific autonomy, and political-institutional independence. Each corner of the triangle helped define the other two, and all three corners taken together formed a stable argumentative unit. This framework was democratically legitimized by its firm dissociation from the totalitarian construct that the American military government believed it had to combat. It was put to use repeatedly in the following years—namely, whenever the MPG had to press for public funding, scientific autonomy, and institutional independence in negotiations with West German science policy makers and supervisory authorities.

## "Basic Research" as the Structuring Principle within the MPG

In a report of the German Research Foundation (DFG, Deutsche Forschungsgemeinschaft) for the fiscal year 1953–54, the distinction between basic research and applied, purposive (*zweckbestimmt*), or industry-oriented (*industrienah*) research was described as both inevitable and problematic.[13] Before, the German Research Council (Deutscher Forschungsrat), a short-lived science policy institution of the early F.R.G. that merged with the DFG in 1952, was dealing with the same terminological problem. According to the will of its most important protagonists, nuclear physicist Werner Heisenberg and biochemist Adolf Butenandt—both Nobel Prize laureates and leading MPG members—the mission of the Research Council was to regulate the relationship of science, state, and industry. Or, to put it with Heiko Stoff in Latour's style, the problem was to at once dissociate and associate industrial research and publicly funded research at universities and nonuniversity institutions (Carson and Gubser 2002; Stoff 2004; Carson 2010).

At first glance, the business of dissociation and association of basic and applied research was organized by means of a division of labor between various institutions in scientific research: the MPG was responsible for basic research, the Fraunhofer Society for applied research, and industry-affiliated laboratories did more immediate applied research. A closer look, however, reveals that the matter is more complicated. The business of dissociation and association had to be done also within these institutions and especially within the MPG. Even though, after the war, the MPG had declared itself an institution exclusively focused on basic research, it had inherited a rather

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mixed portfolio of research institutes—according to the hybrid sense of basic research prevalent in the early 1940s—from its predecessor, the KWG. The internal delineations proved to be principally unending. With every new decision about the opening, closing, continuance, or outsourcing of whole institutes or single departments, these boundaries had to be negotiated anew and often led to controversial discussions. Propagated by these negotiations, the MPG's self-commitment to basic research as its formative pattern, which had been established in the early postwar era, had a lasting impact on both the MPG as a whole and its individual institutes and the conceptualization of their research programs. That is what the following examples are meant to illustrate.

## The Agricultural Science Heritage of the Third Reich

In the final years of the Second World War, a group of KWG scientists began to voice their discontent about the foundation of numerous new institutes, particularly in the agricultural science sector. The new, more applied-oriented institutes were supported by Ernst Telschow, the long-standing secretary-general of the MPG, and Herbert Backe, a member of the senate, the highest decision-making body of the KWG, who at the same time held the positions of state secretary and, since 1941, minister of food and agriculture in the German Reich (Heim 2003; Hachtmann 2007, ch. 9). Among the critics of that development was first of all Adolf Butenandt, who was backed by the biologists Alfred Kühn, Max Hartmann, and Georg Melchers. Butenandt repeatedly demanded "to preserve the holy flame of pure insight."[14] Toward the end of the war, the group—also known as *Tübinger Herren* (the men from Tübingen)—evacuated their institutes from Berlin and moved them to southwest Germany, which would soon become the French occupation zone, and urged a stronger focus on "basic research" (*Grundlagenforschung*) in the peacetime that was to come (Lewis 2004; Gausemeier 2005: 307). In their view, basic research was more or less synonymous with their own approaches in biochemistry, plant physiology, zoology, and genetics. In fact, the senate of the MPG followed this view in March 1949 in reaction to financial decisions of the eleven *Länder* of the future F.R.G. that had not completely fulfilled the MPG's demand for funding. In view of the society's tight budget, it was decided to exclude three institutes that did not fall under this category of basic research. These included—along with the Institut für Phonometrie (Institute for Phonometry)—two agricultural science facilities, namely the Forschungsstelle für Pflanzenbau und Pflanzenzüchtung (Research Center for Plant Cultivation and Breeding) and the Institut für Seenforschung und Seenbewirtschaftung (Institute for Lake Research and Lake Management).[15]

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In the following years, various calls were made to downsize the MPG to the core areas of basic research. Almost all of the institutes that had been able to escape to West Germany were maintained, however, including the remaining agricultural institutes that had been founded during wartime, namely the MPI für Bastfaserforschung (Bast Fiber Research), the MPI für Tierzucht und Tierernährung (Stock Breeding and Animal Nutrition), and the MPI für Kulturpflanzenforschung (Cultivated Plants Research). It was only when the institutes' directors, who had consistently held office throughout the years after the war, retired that some agricultural research facilities were shut down. In 1957, the MPI für Bastfaserforschung was closed; in 1968/69, the MPI für Kulturpflanzenforschung followed; and throughout the 1970s, other institutes from the area of agricultural science and breeding research were either shut down or consigned to new sponsors.[16] Due to the Harnack principle, the dissociation of basic bioscientific research from applied agricultural research followed the interests of the directors: as long as they were in office, their institutes and research trajectories were sustained.

## Reintegration of the German Research Institute of Psychiatry in 1954

The Deutsche Forschungsanstalt für Psychiatrie (DFA, German Research Institute for Psychiatry)—founded 1917 in Munich and integrated into the Kaiser Wilhelm Society in 1924—was situated in the American zone of occupation. Here, barely any research was conducted after World War II. The reasons were manifold: the institute had suffered severe damage when the city was bombed during the war; the institute's director, Ernst Rüdin, inevitably had to be dismissed after his unparalleled involvement in the National Socialist eugenics program; several other executives were laid off as well; and the institute's clinical department was impounded by the occupation forces. The Central Administration Board of the KWG/MPG, which was located in Göttingen and thus fell under British military government, would not have been able to support the DFA, even if they had wanted to do so, as the American occupying force had serious doubts about the continuation of the KWG/MPG and nonuniversity research facilities in general. The remaining employees, headed by executive director Willibald Scholz, financed their work and the provisional maintenance of the remaining infrastructure with the help of grants by the Bavarian state government and reserve funds, which were, however, criticized by the Bavarian Court of Auditors. Another important resource for the institute's funding was an "increase in cases of syphilis" combined with the "occupation force's fear of an epidemic," which resulted in sixty thousand serological tests being administered by the in-

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stitute per year, when in previous years it had only been an average of six thousand tests.[17]

By 1949, the DFA was almost fully incorporated into the medical supply system of the city of Munich, and thus excluded from the budgetary planning of the KWG/MPG. It therefore came as a surprise, when, in March 1949, the representatives of the *Länder* decided to include the grant for the DFA into the overall funding of the MPG. This decision was motivated strongly by the prospective reunification of the KWG/MPG across three of the four occupation zones. The MPG, however, did not include the DFA as a full-fledged MPI, but only agreed to take up the administrative responsibility, with the further condition "that the Max Planck Society was to appoint the institute's director in agreement with the Bavarian state government."[18] The position was given to the neuroscientist Willibald Scholz, a long-time member of the KWG and a reliable head of department, who had succeeded in maneuvering the DFA through the turmoil of the postwar era. It was not until five years later, in 1954, however, that the DFA was reintegrated into the MPG as an emancipated research institute under the name of MPI for Psychiatry.

Acknowledging this step with a contribution to the MPG's yearbook and identifying the new MPI as an institute for basic research was the obvious thing to do. The contribution was not written by the acting director Scholz, however, but by Werner Wagner, who had been appointed head of the clinical institute of the DFA in 1952. Wagner, who died too young (in 1956) to leave visible traces in the history of the MPG, felt compelled to explicate the reasons why his department stood "out from the range" of all the other institutes of clinical medicine outside the MPG that were engaged only with "applied natural science" (*angewandte Naturwissenschaft*) and why its claim for a place within the MPG family was justified. "Basic research," as he understood it, no longer inquired about the "causes" of mental illnesses, but about their "essence" (*Wesen*). To strengthen this argument, Wagner directly referred to his colleague Heisenberg. As Wagner understood it, Heisenberg as well did not "study causes in physics," but investigated the "essence and modalities of matter" (*Wesen und Weisen der Materie*). Wagner also borrowed terms from the liberal arts–oriented field of anthropology. According to him, these terms were helpful in understanding, for example, the psychopathologic symptoms of the senescent human being not as a consequence of arteriosclerotic processes, but as originating in the "essence" of his individual "being-in-the-world" (*In-der-Welt-Seins*). From there, it was only a small step to Martin Heidegger, whose "fundamental-ontological thinking" could, supposedly, make the "existential reason for the existence of all things" (*Seinsgrund alles Seienden*), and thus also that of mental illnesses, phenomenologically accessible and comprehensible. Wagner (1955: 242, 246, 253–255, 259–260, 266;

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transl. CS) summarizes: "At this point, psychiatry becomes the cause for research, but at the same time it provides the material for basic research in the proper sense of the term."

Wagner's understanding of basic research was probably quite different from Heisenberg's and from what the British and American authorities had in mind when, after hesitating for quite some time, they finally approved the continuation of the KWG/MPG. However, this specific understanding was in vogue in the intellectual discourse in West Germany in the early 1950s. Not only did Heisenberg and Heidegger both make major contributions to the discourse, but they even explicitly referred to each other's positions. They did so, for example, at a lecture series organized by Heidegger at the Bayerische Akademie der Schönen Künste (Bavarian Academy of Fine Arts) in the fall of 1953. There, they presented contributions on "The Picture of Nature in Physics" (Heisenberg 1956) and "The Question concerning Technology" (Heidegger 1956), and reflected on "The Arts in the Technical Age" (Heidegger 1956; Carson 2010: 109–113; Vagt 2011).

## From Human Genetics to Molecular Genetics, 1958–1964

In the field of genetics, the understanding of basic research changed in a different way. This shifting emphasis was reflected in the complicated transition from the MPI für Vergleichende Erbbiologie und Erbpathologie (Comparative Genetic Biology and Genetic Pathology) to the MPI für Molekulare Genetik (Molecular Genetics)—a process that started in 1958 and was not completed until 1964 (Sachse 2011). The retiring director, Hans Nachtsheim, who had conducted his hereditary research on rabbits under the roof of the KWI für Anthropologie, menschliche Erblehre und Eugenik (Anthropology, Human Heredity, and Eugenics), which would, soon after 1945, be notorious for its dubious research practices during the Nazi era, was one of the people who persistently ignored the discursive turn toward basic research in the postwar era. In his view, it was the importance of his findings from experiments on animals for human genetics—or rather eugenic practice in accordance with the law on forced sterilization of 1933, which would remain the *ultima ratio* of his research. Nachtsheim even persistently defended the law as not being fundamentally National Socialist in nature, but rather as being scientifically reasonable and even progressive as compared to international standards (Schwerin 2000, 2004).

Karl Thomas, however, emeritus director of the Medizinische Forschungsanstalt (Medical Research Center) at the MPG in Göttingen, who tried to mediate in the conflict about Nachtsheim's succession, believed that human genetics and basic research were mutually incompatible:

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> The main focus of research done at Max Planck Institutes is on basic research. Can human genetics contribute to this? Wouldn't the field rather have to be situated within applied research—given that one can draw a clear line between the two at all? In my opinion, such considerations only further underline that transferring [the MPI's Department of Human Genetics, C. S.] to the Free University [in Berlin-West, C. S.] as its new sponsor is a sensible decision.[19]

Eventually, this view would in fact prevail within the MPG. The Department of Human Genetics and its head, Friedrich Vogel (a student of Nachtsheim's), were not transferred to the Free University, however, but taken over by the University of Heidelberg. This clearing up was implemented despite the prospect of additional funding offered by the Federal Ministry of Nuclear Affairs, who asked the MPG to continue investigating the human genetic consequences of radiation exposure, which was expected to increase in the course of the "atomic age" (Sachse 2011).

Nachtsheim's direct successor Fritz Kaudewitz, who, only after a few years as director (1960–65), left Berlin and relocated to the University of Munich, was not at all interested in the applicability of his molecular genetics research to human genetics. He was prepared to make concessions to woo his audience, however, for example when he gave his inaugural lecture (which would at the same time be his farewell speech) on "Basic Research in Genetics Today" at the annual general meeting of the MPG in 1963: after giving a detailed and complicated account of the latest discoveries about the molecular processes involved in DNS-duplication and the transmission of "genetic information," which had been made mainly in experiments with microorganisms, he drew upon a schoolbook example from human genetics (sickle cell anemia) to make the practical relevance of this kind of research comprehensible also to the lay people in the audience (Kaudewitz 1964: 34–64). About ten years earlier, the British pathologist A. C. Allison (1954: 290–294) had traced the inheritance of sickle cell anemia, which was coupled with malaria resistance, while performing highly questionable human subject research on African men.[20] Kaudewitz explained further that he could now—thanks to the comparative analysis of the respective amino acid sequences in "normal hemoglobin" and "sickle cell hemoglobin"—show in which position exactly this "minor change" within the polypeptide chains "had taken place in the gamete of one human being thousands of years ago." Similarly, he continued, he could show how molecular genetic methods could sometimes be employed in order to formulate hypotheses about human evolution: "Using this example, we can see that biological basic research, even though today it preferably uses microorganisms as experiment subjects, fulfills the '*tua res agitur*' just as well as human genetics does" (Kaudewitz 1964: 60–61, transl. C. S.). Finally, human genetics

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found its way back into the institute, which was relabeled in 1964 as MPI für Molekulare Genetik. It remains to be investigated how this discipline had to prove itself in order to be considered "basic research" (Vingron 2014).

## Founding of the Max Planck Institute for Research into Living Conditions of the Scientific-Technical World, 1967–1969

The founding of a new institute with the complicated name MPI zur Erforschung der Lebensbedingungen der wissenschaftlich-technischen Welt (MPI for Research into Living Conditions of the Scientific-Technical World) was the subject of great controversy. The case is particularly interesting because it forced the humanities section of the MPI to reflect on the issue of "basic research." The nuclear physicist and philosopher Carl-Friedrich von Weizsäcker, who—together with his teacher Heisenberg—had been part of the "uranium club," was the instigator and designated director of the "Starnberg institute," as it would soon commonly be referred to after its location in a Bavarian town. Weizsäcker did in fact not use the term *Grundlagenforschung* (basic research) in his foundation proposal. Rather, he preferred the rarely used term *Grundwissenschaften* (basic sciences), which he used to denote disciplines such as information theory, game theory, systems theory, and cybernetics. His "ambitious goal" was to "conclusively" unravel the "connections of these sciences with each other and with other sciences," but also with their "potential practical applications." Furthermore, his institute aimed to confront these "abstract sciences" with "concrete issues of immediate practical relevance." The array of issues to be tackled ranged from "feeding the world," "structural problems of highly industrialized societies," "civil technology," "consequences of biology," and "arms technology and strategy" to possible "objectives of global politics" and the "future structure of Europe."[21]

Thus, it was not a remoteness from practical issues and applied research that was supposed to distinguish his institute from the numerous other West German institutes and think tanks that were being founded at the time and that concerned themselves with political consulting. The Stiftung Wissenschaft und Politik (German Institute for International and Security Affairs), for example, which was located in the neighboring town of Ebenhausen, conducted its "analyses of foreign policy and strategy preferably by government mandate." Even though von Weizsäcker announced prospective cooperation with this institute, he emphasized that, in Starnberg, research topics would be chosen freely, "without the influence of any client." Furthermore, he asserted that any research at his institute would "consider the big picture with sufficient responsibility."[22]

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Most of the other MPG members who had to decide whether the institute as conceptualized by von Weizsäcker should be incorporated into the MPG probably agreed with his ideas of scientific autonomy concerning the selection of research topics and institutional autonomy of governmental funding. In view of the wide range of possible topics to be researched by the institute, however, it remained unclear to most of them what exactly von Weizsäcker had in mind when he referred to "the big picture." With regard to the name of the institute, a variety of alternatives were discussed to make it "mannerly and comprehensible," as Fritz Münch, an expert in international law, urged.[23] Under consideration were denominations such as "futurology," "peace studies," or "foundation of planning sciences" (*Grundlegung der Planungswissenschaften*). The name "theoretical and experimental prognostics" was rejected because of its—all too apparent—allusions to physics. Von Weizsäcker himself preferred the denomination *Mellontik,* which no one but himself understood.[24] Ultimately, von Weizsäcker's original proposal was accepted—without the debutants being able to really discern the conceptual outlines of the institute to be founded. During the final session of the humanities section in February 1969, which was critical for the decision about the foundation of the institute, the chairman of the humanities section, art historian Wolfgang Lotz of the Bibliotheca Hertziana, made the decisive argument in favor of Weizsäcker and his proposal: "By current definition, the Max Planck Society conducts basic research. If there exists something like basic research, it seems to me that this project . . . deserves the name and requires basic research. The individual projects that shall be realized can only be realized if the necessary foundation is given, either in the shape or in the ideas of Mr. von Weizsäcker or the institute's director."[25]

As a response to the doubts that had been voiced concerning the feasibility of von Weizsäcker's program, Lotz called upon Max Weber's spirit for support: "I think that if Mr. Max Weber was with us right now, he would also vote in favor of this institute, because it seems to me that the path that Max Weber has laid out with his work is leading directly to the issue at hand."[26] Apparently, basic research in the humanities remained inseparably linked to the imposing intellectual stature of the heroic researcher to whom it was ascribed and who could claim it exclusively for himself. This claim of von Weizsäcker's was recognized among the members of the MPG, yet did nothing to reveal anything about the type of research programs that would be implemented in Starnberg in the future. Above all, his claim by no means ruled out practical policy counseling, the way, for example, the MPI director von Weizsäcker practiced it from 1974 to 1977 as chairperson of the advisory committee of the Federal Minister of Research and Technology.[27]

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## Conclusion

For the English-speaking context, Clarke and other authors have shown how specific science policy constellations in the United States and Great Britain resulted in the introduction of the terms "fundamental research" and "basic research" around the turn of the twentieth century. In this context, the terms formed a hybrid concept that oscillated between notions of pure and applied research. At that time, however, the German term *Grundlagenforschung* was used rather incidentally, even though in the 1930s it became more common. Under the very different, but yet again very specific, conditions of the mid-1940s, which historically spanned both the end of the Second World War and the beginning of the Cold War, and which saw the Western part of the former German Reich become the junior partner of the Western alliance, the hybrid understanding of basic research evolved into a dichotomous concept: mainly due to the influence of the contemporary U.S. discourse, basic research was then contrasted conceptually with applied research and described as clearly distinguishable from the latter. At the same time, "basic research" was enriched with allusions to former conceptualizations of "pure science." It was presented as a symbol of freedom, for which the Western democracies stood firm against the only remaining totalitarian enemy: the Stalinist system. Basic research was now understood as being far from application, not immediately relevant to political or military purposes, and autonomous or free in the sense of the Western democratic self-conception.

It was in this particular political constellation and with this semantic conceptualization that basic research became programmatic for the KWG/MPG. After 1945, the concept contributed to the process of the MPG's reorganization in three different ways: first, regarding the society's struggles of dealing with its recent past, references to "basic research" helped to obscure the character of the more or less hybrid research, particularly within the field of armament research, with which the KWG had proved itself as a part of the NS regime. Second, in the postwar constellation, the new concept was the key to recovering the MPG's organizational integrity, to defending its institutional independence, and to maintaining the scientific autonomy of its members. Third, when it came to the formulation of future directions, the commitment to basic research remained a perennial mission. As we have seen in the discussions from the first two decades of the new MPG as exemplified above, it was by no means always transparent to the respective actors how exactly this commitment was to be implemented, for instance in the conceptualization of institutes, research fields, and projects.

One could be inclined to dismiss the strenuous conceptual efforts presented above as simple entries in a collection of stylistic blunders concern-

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ing the topic of basic research in the first decades of the Federal Republic of Germany. Each of the speakers cited here did understand basic research in another way. Even those, however, who openly articulated their puzzlement as to what basic research really meant when applied to the concrete case of a person doing research, or a department, or a whole institute, did not question the self-commitment of the MPG, its institutes, and its current and especially its prospective future members to basic research. In the debates presented in this chapter, concerning the shutdown, reorganization, or foundation of institutes or departments, the respective research proposals were measured against the standard of "basic research" without ever having established a binding definition or scale. For the early postwar years, a common understanding was hardly to be expected. As far as the debate concerning the foundation of the Starnberg institute can be interpreted as symptomatic of this conceptual uncertainty, it can be postulated that even two decades later, the MPG members and bodies had not come to a satisfactory and reliable agreement about the exact nature of what was still undisputedly regarded as the decisive parameter in such decision-making processes. It is up to future investigations to explore what kind of understanding of basic research substantiated in the research practices of the MPG's members and institutes in the 1970s—when debates about science and its social responsibilities reemerged and public funding became less self-evident.

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## Notes

1. Archive of the History of the Max Planck Society (MPA, *Archiv zur Geschichte der Max-Planck-Gesellschaft*), II/IA-1, 1/1, 1/2.
2. In 1948, when the MPG was reestablished, twenty-three MPIs were counted (apparently not including the institutes in Berlin and the Soviet and French zones of occupation); in 1960, forty (including the institutes in Berlin-West and the former French zone of occupation); in 1972, fifty-two; in 1984, fifty-eight; in

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1990, sixty-one; in 2002, eighty (Gruss and Rürup 2010, 14). On the MPG's reorganization see Hachtmann 2007: 1041–1156; and Sachse 2009.

3. See "A Portrait of the Max Planck Society," Max-Planck-Gesellschaft website, retrieved 2 June 2015 from http://www.mpg.de/short-portrait.
4. Lucier (2012: 535) recently declared the linear model to be "entirely dead" and referred to Grandin, Worbs, and Widmalm 2004. See also final chapter in this volume.
5. For further discussion of relevant literature see the introduction and the other case studies in this volume.
6. According to Clarke (2010: 205), the DSIR has been using the adjective "basic" as a synonym for "fundamental research" ever since its 33rd Annual Report (1947–48).
7. For a more detailed analysis of the importance and use of "basic research" in Interwar Germany, see Schauz 2014 and chapter 2 in this volume.
8. A precursor to this debate can be found in arguments between the Marxist-oriented British scientists close to John Desmond Bernal (see particularly Bernal 1939) and the *Society for Freedom in Science,* which was founded by Michael Polanyi and John Randall Baker in 1940 (McGucken 1978; Werskey 1988; Clarke 2010: 307–310). It was continued in the United States' *Congress for Cultural Freedom* (Polanyi 1954: 17–27; Berghahn 2004: 175, 178).
9. On the role of the Rockefeller Foundation as a moderator with regard to the U.S. view of the KWG and on the following, see Sachse 2009. On the reconstruction of West Germany's scientific landscape, see Osietzki 1984; Eckert and Osietzki 1989; Trischler and vom Bruch 1999; Weingart 2001; Carson 2010; Walker et al. 2013; and the series *Studien zur Geschichte der Deutschen Forschungsgemeinschaft,* edited by Rüdiger vom Bruch, Ulrich Herbert, and Patrick Wagner, vol. 1–10 (Stuttgart: Franz Steiner Verlag, 2007–2015).
10. On the transformation from KWG to MPG, see Hachtmann 2007: esp. 1159–1168.
11. MPA, II/1A/(5–5)12: Denkschrift, 14 February 1947. All following citations are taken from this document, transl. C. S.
12. All citations: MPA, II/1A/(5–5)12: Denkschrift, 14 February 1947.
13. DFG report (4 April to 31 March 1954: 18), here cited (with the author's kind permission) from Heiko Stoff's paper presented at the conference "Anwendungsorientierung in der universitären Forschung. Historische Perspektiven auf eine aktuelle Debatte," Münchener Zentrum Wissenschafts- und Technikgeschichte, March 2011 (Lax 2011).
14. Butenandt 1941: 20: "die heilige Flamme des reinen Erkenntnistriebs (…) hüten"; Citation taken from Gausemeier 2005: 287.
15. MPA: Niederschriften von Senatssitzungen, No. 4–6, 1949, meeting on 18/19 March 1949: 4.
16. These included MPI für Zellphysiologie (Cytophysiology) in 1970; MPI für Eiweiß- und Lederforschung (Protein and Leather Research) in 1973; MPI für Tierzucht und Tierernährung (Stock Breeding and Animal Nutrition), handed

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over to the Bundesforschungsanstalt für Landwirtschaft (Federal Research Institute for Agriculture), in 1974; MPI für Landarbeit und Landtechnik (Agricultural Labor and Technology) in 1976; and MPI für Pflanzengenetik (Plant Genetics) in 1978. The only nonagricultural institute that was passed on to a new sponsor was the MPI für Silikatforschung (Silicate Research), which was handed over to the Fraunhofer-Society in 1970. In the research area of physics, however, institutes were not closed, but older institutes that covered a broad range of topics were split up into separate branches and remained within the MPG (Henning and Kazemi 2011). I am grateful to Florian Schmaltz for a list of former and closed MPIs.

17. MPA Vc/4, KWG Nr. 1: Geschichte der Kaiser-Wilhelm-Gesellschaft und der Max-Planck-Gesellschaft 1945–1949, Göttingen 1949 (=Festschrift zum 70. Geburtstag des MPG-Präsidenten Otto Hahn), 259 (transl. C. S).
18. MPA Niederschrift von Senatssitzungen, Nr. 4–6 (1949), meeting on 18/19 March 1949: 5 (transl. C. S).
19. MPA II/1 A-IB, MPIVEE, Kaudewitz, vol. 1, Thomas to Ballreich, 12 May 1961 (transl. C. S.).
20. For Nachtsheim's comment on this discovery, see Sachse 2011: 44–45.
21. All citations: MPA II/9/13: Vorschlag zur Gründung eines Max-Planck-Instituts zur Erforschung der Lebensbedingungen der wissenschaftlich-technischen Welt, 11 November 1967, signed by Carl Friedrich v. Weizsäcker, Wolfgang Bargmann, Klaus v. Bismarck, Hermann Heimpel, Walther Gerlach, and Werner Heisenberg (transl. C. S.).
22. MPA II/9/13: Vorschlag zur Gründung eines Max-Planck-Instituts zur Erforschung der Lebensbedingungen der wissenschaftlich-technischen Welt, 1 November 1967, signed by Carl Friedrich v. Weizsäcker, Wolfgang Bargmann, Klaus v. Bismarck, Hermann Heimpel, Walther Gerlach, and Werner Heisenberg (transl. C. S.).
23. MPA II/1A-IB, IL1/Lebensbedingungen: Abschrift der Tonaufzeichnung der Sitzung der Geisteswissenschaftlichen Sektion 11 February 1969, 65 (transl. C. S.).
24. MPA II/IA 76. VP, Bd. 2: Niederschrift über die Sitzung des Beratungskreises über die Errichtung eines MPIL, 2 February 1968; Niederschrift über die 76. Sitzung des Verwaltungsrats und des Vorstands der MPG, 4 March 1968 (transl. C. S.).
25. MPA II/1A-IB, IL1/Lebensbedingungen: Wortprotokoll der Sitzung der Geisteswissenschaftlichen Sektion, 11 February 1969, 61–62 (transl. C. S).
26. MPA II/1A-IB, IL1/Lebensbedingungen: Wortprotokoll der Sitzung der Geisteswissenschaftlichen Sektion, 11 February 1969, 74 (transl. C. S).
27. MPA II/9/20: MPG-Spiegel 4/1974, 20; Beratungsplan des Bundesministeriums für Forschung und Technologie 1974; Gottstein to Weizsäcker, 24 June 1974, Weizsäcker to Gottstein 1 July 1974 (see also Leendertz 2010, 2014).

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Chapter 6

# Beyond the Basic/ Applied Distinction?

## The Scientific-Technological Revolution in the German Democratic Republic, 1945–1989

*Manuel Schramm*

Science policy in the German Democratic Republic (G.D.R.) was shaped by different, often contradictory, influences. However, three distinct (though overlapping) phases can be distinguished. In the first phase, the traditional German university ideal persisted at least in the early years of the G.D.R. It stipulated the unity of teaching and research, and the purity of science. On the other hand, Marxist thinkers such as John Desmond Bernal (1901–1971) or Gerhard Kosel (1909–2003) developed the concept of the so-called scientific-technological revolution in the 1940s and 1950s, claiming that science became a force of production in its own right, similar to the theory of the "knowledge society" in the West.[1] It increasingly motivated science policy from the late 1950s onward (second phase), and tended to undermine the traditional dichotomy between pure and applied science. Living in the age of scientific-technological revolution meant that results of pure science (now increasingly called "basic research") would quickly become productive, making the very distinction between pure and applied obsolete, or at least reduce it to a matter of degree. However, the optimism of the 1960s was not sustained, and with the beginning of the 1970s there was a renewed emphasis on basic research without ever dropping the claims of the scientific-technological revolution (third phase). Party leaders argued that a neglect of basic research would be counterproductive, and favored what amounted to a linear model from basic to applied research to technical development. The following essay

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discusses the distinction between basic and applied research, and between pure and applied science, in each of the three phases mentioned above. The semantics were intimately connected with the direction of science policy in general.

The oscillation between traditional views of the distinction between pure and applied science and new ideological concepts mirrors ideological shifts in other areas, notably in economic policy. But it also betrays a fundamental uncertainty that Communist leaders faced when they found themselves in charge after the fall of Nazi Germany. On the one hand, Lenin had already proclaimed in 1919 that a socialist society could not be built without the help of bourgeois specialists, who were mostly not in favor of socialism. This approach would lead to a cautious science policy, mainly aiming at securing the cooperation of scientists and engineers by providing material incentives and accepting freedom of research. On the other hand, there had always been a certain anti-intellectualism in Marxist thinking that classified science as part of the superstructure and regarded it, therefore, as less important than material production, especially as scientists had been regarded as tools and allies of the bourgeoisie (Graham 1993: 88–90). This inherent tension was never resolved, but it may go some way toward explaining the more or less sudden shifts in G.D.R. science policy. The next section will provide a general overview of science policy and scientific institutions in the G.D.R. The following sections discuss the use of the basic/applied research distinction in the context of a changing science policy in a roughly chronological order, corresponding to the three phases outlined above. Special attention will be given to the notion of a scientific-technological revolution in the 1960s.

## The Context: Science Policy and Scientific Institutions in the G.D.R.

Research in the G.D.R. rested on three pillars: the universities, the Academy of Science, and industrial research. This structure was largely inherited from earlier times, dating back to the German *Kaiserreich*. The institutes of the Academy of Science were successors to the old Kaiser Wilhelm Institutes, which had a tradition of cooperation with industry, especially in the context of military research during National Socialism (Nötzoldt 1999; Maier 2007; Rürup 2013). German science had had a good reputation in the first half of the twentieth century, so the G.D.R. inherited an already established system of research universities and institutes, even though there had been a considerable brain-drain both in the Nazi era and after the war. The universities had different research profiles according to their prewar traditions, and had to balance research and teaching duties. Even though research at the Academy in-

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stitutes had a higher reputation, university research remained indispensable for its broader scope of research. The expenses for research and development in the 1970s and 1980s were high, with 2.8 percent of GNP in 1970 and 3.4 percent in 1986, surpassing those of the Federal Republic of Germany with 2.2 percent in 1970 and 2.8 percent in 1986 (Scherzinger 1990).

In science policy, three distinctive phases can be distinguished. In the first phase, from 1945 to the late 1950s, many of the old structures were left intact. The party leaders tried to keep scientists in East Germany and were therefore willing to make concessions. Ideological pressure was applied on the humanities and social sciences from the late 1940s onward, but hardly on the natural sciences or engineering (Malycha 2002: 90–93). However, some changes were introduced in the early 1950s. New universities for the engineering sciences were founded in Ilmenau, Magdeburg, Dresden, Leipzig, Weimar, Merseburg, and Karl-Marx-Stadt (Chemnitz) between 1952 and 1954; some attempts at planning were introduced; and the curriculum was reformed by introducing a mandatory course in Marxism-Leninism (Connelly 1997, 2000).

The second phase, from the late 1950s to the early 1970s, was not only the heyday of the "scientific-technological revolution," but also a major reform period in the society at large and in science policy. From the late 1950s onward, the G.D.R. leadership tried to introduce new methods of planning and coordination of research—for example by establishing a research council in 1957 or by promoting contract research. The so-called third university reform from 1967 to 1969 restructured universities by abolishing the old faculties and establishing new departments (*Sektionen*). In addition, university research in science and engineering had to be oriented to the needs of industry, though to varying degrees, according to local conditions. In the same vein, the reform of the Academy of Science produced bigger institutes and promoted cooperation with industry as well (Zimmermann 1981; Scherzinger 1990; Laitko 1996; Fraunholz and Schramm 2005). Many scientists perceived this as a threat to their freedom of research, complaining they would be demoted to measuring servants (*Messknechte*) for industry (Schramm 2008: 91).

The third phase, from the beginning of the 1970s to the end of the G.D.R., was marked by the end of the reform period when Erich Honecker (1912–1994) came to power in 1971. Although the theory of the scientific-technological revolution was never officially renounced, science policy became more pragmatic in this period. The orientation of research at Academy and university institutes toward the needs of industry was eased, but never completely abolished. Party leaders acknowledged that basic research followed a logic of its own and could not be planned and directed in the same way as applied research or technical development. In effect, science policy adopted the linear

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model (basic research, applied research, development). The organizational structures remained basically the same after 1970.

## Basic and Applied Research in the Early Years, 1945–1958

After 1945, science policy was directed mostly by people who neither had experience in the organization of science nor in scientific research itself. One of them was Anton Ackermann (1905–1973), who had a background as a textile worker in East Germany and whose educational credentials were confined to the International Lenin School in Moscow, a party school run by the Communist International (Müller-Enbergs 2010). In a speech in 1948, he recommended Marxism-Leninism as a foundation for all sciences and emphasized that Marxism and science formed a unity (Ackermann 1948: 33).[2] Therefore, it was not necessary to make a distinction between applied and basic science, as all science would finally arrive at dialectical materialism—that is, Marxism-Leninism. To back his claim, Ackermann repeated the well-known quotation from Lenin that "Marxism is omnipotent because it is true" (Malycha 2003: 247).

In a similar vein, Fred Oelßner (1903–1977) argued in 1951 that the natural sciences were neither part of the base nor part of the superstructure, in contrast to the social sciences or philosophy, which were either bourgeois or proletarian—that is, Marxist. Oelßner was a former clerk who had served an apprenticeship in commerce but did not have a degree of higher education. In 1949, he became the party secretary for propaganda and science (Malycha 2003: 379; Müller-Enbergs 2010). According to him, the natural sciences were connected to production because they arose out of practical requirements and helped with producing material goods. The natural sciences were therefore not part of the superstructure of capitalist society, but indifferent to class (Oelßner 1951: 785).

It was certainly reassuring for scientists that party leaders recognized the importance of scientific research and did not succumb to anti-intellectualist tendencies. However, this did not answer the question as to how scientific research should be organized in a socialist state, and where the boundaries between applied and basic research were to be drawn. Already in 1948, the German Economic Commission (Deutsche Wirtschaftskommission) admonished the Institute for Optics of the Academy of Sciences to conduct not only basic research, but also applied research (*Zweckforschung*) for industry (Hauser 1987: 7). The topic was raised again in 1951, when an attempt was made to separate the tasks of the Academy from the ones of the Central Office for Research and Technology (ZFT, Zentralamt für Forschung und Technik), a body founded in 1950 under the roof of the State Planning Commission

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(Staatliche Plankommission).[3] Scientists from the Academy, such as physicist Walter Friedrich (1883–1968) and mathematician Josef Naas (1906–1993), complained that the ZFT tried to separate research in a way that would leave only basic research with the Academy, whereas all tasks of applied research would be carried out in industrial research institutes. This, the professors complained, was unfeasible and would separate the Academy from "practice" (*Praxis*).

In fact, in many Academy institutes there was no clear-cut division between basic and applied research. For example, the Institute for Optics carried out both basic experimental research and contract research for the optical industry. The reply from Paul Strassenberger (1910–1956), deputy chief of the State Planning Commission, was instructive. He argued that the current structure of the Academy of Science had evolved through personal factors and was more or less arbitrary. There should be a separation of tasks in the sense that the Academy institutes would mainly carry out basic research, and industrial research institutes would do applied research linked to industry. He admitted that there could not be a neat separation between basic and applied research, but it still had a certain justification (Malycha 2003: 396–397). In the end, both sides agreed on the formula that the main part of basic research was to lie with the Academy of Science, whereas the main part of industrial research (*Industrieforschung*) belonged to the ZFT. In addition, it was agreed that the Academy was free to choose areas of research, but was accountable to the State Planning Commission for its use of material resources (Malycha 2003: 403).

What kind of idea was behind this distinction between basic and applied research? First, despite a suggestion by Hans Wittbrodt of the ZFT that one should not talk about basic and applied research (Malycha 2003: 398), the distinction was upheld in the end. Second, what made research basic or applied was neither the way research was conducted nor the place where it was carried out, but rather the relationship to industry: applied research was research carried out at the request of or in cooperation with industrial enterprises.

This, however, was not the only definition used at the time. In 1952, the State Planning Commission renewed its critique of the Academy of Science. In a detailed analysis of the Academy's achievements and shortcomings, the Commission criticized the Academy for undertaking too little basic research, which was its original task. Instead, the institutes most often preferred to work on "technical problems," which were easier to solve: the three biggest institutes (Institute for Optics and Precision Mechanics, Heinrich Hertz Institute for Research on Oscillations, and Institute for Fiber Research) engaged primarily in applied research and development without, however, having the necessary contacts to industry (Malycha 2003: 419–420). Here a

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different notion of applied research was employed: it was not research in an industrial context, because presumably this context did not exist, but rather work on "technical problems." In this regard, the distinction between basic and applied research was intertwined with the distinction between science and technology: the more technological a research problem was, the more likely it came to be called applied research.

In this first phase of G.D.R. science policy, the distinction between basic and applied research was not at the core of the political-ideological discourse, even though it could be used in arguments about funding and jurisdiction. Rather, party leaders such as Ackermann and Oelßner tried to clarify the relationship between science in general and Marxism-Leninism. Their answer was that the two coexisted easily in the sense that one was the foundation for the other. To strengthen applied research in order to enhance its benefits was not yet regarded as necessary, and definitions of applied research varied. This was going to change with the advent of the so-called scientific-technological revolution.

## The "Scientific-Technological Revolution"

In the late 1950s, science policy in the G.D.R. underwent a fundamental transformation. Instead of the cautious approach of the late 1940s and early 1950s, a more assertive attempt was made at planning science and at restructuring scientific organizations. The aim was twofold: for one thing, the reforms were meant to support the ambitious goals of the seven-year plan of 1957 in building socialism. For another, it was an attempt to bring the science sector under the control of the party. From the early 1960s onward, these reforms were accompanied by economic reforms aimed at a modernization of the East German economy.

The most prominent achievement was the establishment of a research council (*Forschungsrat*) in 1957, which served as an advisory body for the government but was also supposed to coordinate research in the G.D.R. The ideological justification for these reforms was the idea of a "scientific-technological revolution," which would take place on a global scale and make science a force of production in its own right. Indeed, the Department of Science of the Central Committee wrote already in May 1957 that science was becoming a "decisive force of production in human society" (Malycha 2003: 553). It recommended, therefore, a restructuring of the scientific and technical faculties at the universities.

The ideological roots of the "scientific-technological revolution" go back to the interwar years and discussions about the relationship between science and society in Marxist circles. John Desmond Bernal (1901–1971), for in-

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stance, was inspired by the Soviet physicist and historian of science Boris Hessen's presentation on the social and economic roots of Newton's *Principia* at a history of science conference in 1931 (Hessen 1931; Steiner 2003: 19). Learning from the history of science that science was always influenced by social and economic factors, he took the analysis one step further. In *The Social Function of Science,* Bernal (1939) argued against the ideal of pure science. In contrast to thinkers such as the Hungarian-British chemist and social scientist Michael Polanyi (1891–1976), Bernal maintained that science could and should be planned. In this publication, he did not mention a scientific-technological revolution, although he acknowledged that science's influence on productive methods was, and would for a long time remain, its most important influence on society (Bernal 1939: 386).

More important for Bernal's reception in the G.D.R. was his book *Science in History* (1954), which went through several editions and translations. In 1961, it was published in German. Basically, Bernal argues that science has historically always been in contact with society, especially with production processes. Moreover, science needs this continuous exchange with production, lest it become sterile (Bernal 1961: 29, 36, 41). Bernal regarded science primarily as practice, just as many historians and sociologists of science have done for the last twenty-five years or so (Pickering 1992). He criticized the ideal of pure science mainly for two reasons: empirically, science never existed in isolation, but was linked to industry or trade; theoretically, it was wrong because it did not recognize that science was only complete when its results were put into practice. It could not be isolated from technology (Bernal 1961: 30). The ideal of pure science would effectively halt the progress of science and benefit the reactionaries. The same criticism applied to the distinction between pure and applied science, which were but two aspects of the same organism. Science, Bernal (1961: 869) repeated, had never been completely "free."

The conclusion made sense. If pure science had never existed and could only exist in a degenerated form, it made no sense to speak of pure versus applied science. For Bernal, all science was (more or less) applied science, because it ultimately derived from production processes. He did not use the terms "basic" and "applied research"; however, he used the term "applied science" (versus "pure science"), equating it with technology (Bernal 1961: 31). The "scientific-technological revolution" was not a central term in Bernal's book, although it was included in a slightly different form in the second English edition of 1957. In it, Bernal ([1954] 1957: 960) referred both to the spectacular breakthroughs of science (discovery of the nuclear atom, theory of relativity, quantum theory, electronic computers, etc.) and the fact that science had come to dominate industry and agriculture, saying, "The revolution

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might perhaps more justly be called the scientific-technical revolution." In the first edition, he had spoken of a "second scientific revolution." In his later writings he would cling to the term "scientific-technological revolution," and claimed the term could be detected as far back as the 1930s (Teich 1986: 317–318).

Another intellectual father of the "scientific-technological revolution" was Gerhard Kosel (1909–2003), a German-Russian architect, and a pioneer of prefabricated buildings. He emigrated to the USSR in 1936 and returned to the G.D.R. in 1954, where he became a member of the Central Committee from 1958 to 1967 and president of the German Academy of Architecture (Deutsche Bauakademie) from 1961 to 1965 (Müller-Enbergs 2010). Already in 1951, Kosel published a book about how socialist society could make better use of scientific results. But it was published in Russian, and was translated into German only in 1986 (Kosel 1986). More influential was his book published in 1957 on science as a force of production (*Produktivkraft Wissenschaft*; Kosel 1957). In the following years, it became a central tenet of adherents of the "scientific-technological revolution" that science had become an immediate force of production in its own right.

Kosel explained that science and technology had made great progress since the time of Karl Marx. For that reason, it was necessary for Marxist theory to recognize that science has become a potency in its own right, because the natural and engineering sciences serve as starting points for the revolution of production (Kosel 1957: 48). Like Bernal, Kosel (1957: 78) saw science as a systematization and generalization of human experience that was found in production processes, stressing the interaction (or, with Lenin, "dialectics") of scientific-technological and material production. This similarity to Bernal's position would imply a similar critical stance toward the pure/applied science distinction. However, Kosel used the terms basic and applied research, but put "applied" (but not basic) in quotation marks. As for basic research, he argued that in capitalist states much of it would actually be carried out by big enterprises, like General Motors. Basic research would provide information about those factors that have *not yet* had any influence on technology (Kosel 1957: 44). This phrasing turned the basic/applied distinction into a mere temporal one, with basic research being the research that is not yet immediately relevant for production processes. Kosel did not see any essential difference between basic and applied research, although he stopped short of denouncing the distinction.

While the distinction between basic and applied research was compatible with socialism, the ideal of pure science or pure research was viewed with suspicion. Prominent scientists, such as the chemist Peter Adolf Thiessen (1899–1990), chairman of the research council from 1957 to 1965, and phys-

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icist Max Steenbeck (1904–1981), distanced themselves from it in public. Both confessed that they had adhered to the ideal of pure research when they had been young, but Steenbeck (1973: 146) denounced it as arrogant, elitist, and dangerous because it implied that every use of research results was inferior and dirty. Thiessen (1979: 133), for his part, claimed in 1961 that he had never thought of practical implications of his research when he was director of the Kaiser Wilhelm Institute for Physical Chemistry between 1933 and 1945. It was only when he went to the Soviet Union after the war (as participant in the Soviet nuclear arms project) that he discovered that pure research was a necessary, but not sufficient, part of science, and that only research and technology together constitute science. Thiessen obviously used pure research (*reine Forschung*) and basic research (*Grundlagenforschung*) synonymously in this speech. A similar remark was made by Max Steenbeck (1967: 18–19) in 1959 when he spoke of pure and uncommitted basic research (*reine und zweckfreie Grundlagenforschung*).

That leading scientists distanced themselves from the ideal of pure research did not mean that they wholeheartedly embraced the idea of a scientific-technological revolution. As late as 1957, Thiessen (1979: 41) argued that mechanization and automation would sometimes be called a new technological revolution, but doubted if this was more than a buzzword, because in his view it did not require any new parameters. In 1961, however, he acknowledged the existence of a "new technology" (*neue Technik*) that was at the same time applied research (*angewandte Forschung*) but also had repercussions for research and even participated in research. He went as far as to suggest that there was no substantial difference between scientific research in laboratories and industrial production, because intelligent workers, foremen, technicians, and engineers would work according to the same scientific principles as scientists do (Thiessen 1979: 132, 134). Six years later, he finally spoke of a scientific-technological revolution, the essence of which was the sharp rise in the growth of scientific knowledge and a close interaction and interweaving of research and technology (Thiessen 1979: 222). For Steenbeck (1967: 205–206), the main corollary of the scientific-technological revolution was that every work would become part of a collective effort without devaluing the effort of the individual.

G.D.R. science policy never went quite as far as abolishing the basic/applied distinction altogether. While the idea of science as a force of production was taken up quickly, as early as in 1957, this did not mark an end to the distinction between basic and applied research in policy documents. On the contrary, the idea that science was a source of technological innovation could also lead to a renewed emphasis on basic research. At least the Central Committee thought so in May 1957 when it announced that science was a productive

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force. The corollary was not that all science should be made to serve industry's needs—that came only later in the so-called third university reform at the end of the sixties. In 1957, the Central Committee argued that it would be a mistake to have a too-narrow-minded conception of science—for example, to reduce nuclear physics to research on the requirements of nuclear power plants. Rather, science should fundamentally produce new research results that would revolutionize production (Malycha 2003: 559–560). The corollary for science policy was that a difference had to be made between theoretical research (*theoretische Forschung*) and research oriented toward the immediate needs of production (*unmittelbare Hilfe in der Produktion*), the former being to a certain extent unable to adhere to strict schedules. This distinction, the Central Committee claimed, had not been sufficiently respected in the past.

Why the Central Committee chose to use the term "theoretical research" instead of "basic research" is not quite clear. Earlier, the same document argued that theoretical research in the sciences had been a strength of German science for a long time, and it had the additional advantage of being relatively cheap (Malycha 2003: 558). However, using the distinction "theoretical versus applied research" could be seen to imply that only theoretical research was exempt from the strict planning and accounting requirements for applied research, whereas all other forms of science (especially experimental science) was not. This reading of the source would be in line with the repeated attempts to plan, coordinate, and control science since the late 1950s.

Other leading officials went further. In 1961, Werner Hartke (1907–1993), president of the Academy of Sciences, attacked what he regarded as bourgeois notions about the relationship between research, teaching, and practice. Hartke was an ancient historian who came from an educated middle-class family (Müller-Enbergs 2010). Having been a member of the National Socialist German Workers' Party (Nationalsozialistische Deutsche Arbeiterpartei) since 1937, he joined the German Communist Party (Kommunistische Partei Deutschlands) only after the war in 1946. Maybe that is why he felt the need to prove his ideological reliability and present himself as a hardliner.

Hartke argued that in a socialist society, science had acquired a completely new position and played a dominant role not only in research, as it had before, but also more and more in education and practice (meaning production processes). Although Hartke did not use the term "scientific-technological revolution," what he had in mind was very similar, especially the growing influence of science on all parts of society. At the time, the term "scientific-technological revolution" was not yet widely used in the G.D.R. Hartke preferred, therefore, to speak of a "unity of research, teaching and practice" (Malycha 2003: 647). This was a modification of the traditional German uni-

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versity ideal which used to postulate a unity of teaching and research (vom Bruch 1997). Hartke argued that in a socialist society like the G.D.R., all the preconditions were met to achieve the unity of research, teaching, and practice, but that it had not yet been achieved because of personal failures and wrong decisions by the government. He advocated a tighter and more centralized control in all parts of science (Academy, universities, and industry) to remedy the situation.

In particular, he criticized "old bourgeois notions" (Malycha 2003: 652) of the relationship between research, teaching, and practice. One of these notions pertained to basic research. While there is no indication that Hartke refused the distinction between basic and applied research as such, he was critical of the "late bourgeois" idea that basic research could be carried out in isolation from teaching and practice (Malycha 2003: 651). In the end, this would have meant a centralized control of all scientific activities without any exemptions, such as the Central Committee had advocated in 1957. He failed to explain exactly how he envisioned the relationship between basic and applied science or between science and practice. However, to speak of the insoluble connection between research, teaching, and practice would imply a limited autonomy of basic research at best.

The university and Academy reforms of the late 1960s largely went along the lines that Hartke had already sketched out in 1961. The distinction between basic and applied research was never fully abandoned. However, the distinction became fragile and tended to collapse insofar as basic research had to serve a social as well as an economic purpose. For example, the state secretary of universities and colleges wrote in 1966 that universities should concentrate on complex tasks of basic research, but within the latter on preparatory research (*Vorlaufforschung*) for the economy or social progress in general. For that purpose, the secretary called for a close cooperation between scientific institutions to integrate basic research, applied research, and technical development (Staatssekretariat für das Hoch- und Fachschulwesen 1966: 8–9).

Similar statements about a close relationship between basic and applied research can be found in the years of the university reform of 1968–1969. At this time, new terms needed to be invented to describe the integration of basic and applied research without refuting the distinction altogether. For example, the rector of the Friedrich-Schiller-Universität Jena, Franz Bolck (1918–2000), described the research profile of the newly founded physics department of his university as application-oriented preparatory and basic research (*anwendungsorientierte Vorlauf- und Grundlagenforschung*; Stutz, Kaiser, and Hoßfeld 2007: 291). The difference between application-oriented research (*anwendungsorientierte Forschung*) and applied research (*angewandte Forschung*) was important for Bolck, because he argued that in the former the

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university remained in control of the content of the research, even if it was contract research for industrial enterprises such as Carl Zeiss. At the Technical University Karl-Marx-Stadt, the academic senate had already committed to so-called targeted basic research (*gezielte Grundlagenforschung*) in 1966 (Lambrecht 2007: 146–147). Still earlier, in 1962, the research director of Carl Zeiss, Paul Görlich, had spoken of basic research for a specific purpose (*zweckgebundene Grundlagenforschung*; Schramm 2008: 82).

The invention of new terms between basic and applied research betrays a certain uneasiness on the part of G.D.R. science officials. On the one hand, they were convinced (or had to pretend to be convinced) of a scientific-technological revolution that would undermine the old distinction. On the other hand, there was a reluctance by universities to abandon the distinction between basic and applied research altogether, because they feared becoming mere adjuncts of industrial combines. So new terms were created in order to convince party leaders that university research was ultimately not pure in an old-fashioned sense, but oriented toward practical results, and yet still different enough from industrial research to legitimate claims for partial autonomy.

## The Triumph of the Linear Model: The 1970s and 1980s

Officially, the "scientific-technological revolution" was never renounced. Speeches and policy documents from the 1970s and 1980s are full of references to it. In addition, the idea that science as a whole was not independent from society but should be made useful for it (economically or otherwise) remained a central tenet of party doctrine. In 1975, Otto Reinhold (1925–2016), the president of the Academy of Social Sciences, wrote that science in general, and scientific-technical progress in particular, was directed toward raising living standards of the working people and served to form well-educated socialist personalities along Humboldtian lines. While he did not mention the distinction between applied and basic research, the key problem for him was the application of research results in production processes, and he lamented an underestimation of production technology (Reinhold 1975: 496–497).

The important point was that the blame for the (relative) lack of innovations was no longer apportioned to basic research and scientists. In the 1960s, the general aim of science policy (such as the reforms of the universities and the Academy) had been to increase their economic productiveness. The concept of the scientific-technological revolution had been the ideological justification for this policy. But from the early 1970s onward, party leaders were more willing than before to accept that there were limits to planning in basic research, and therefore not all basic science could be redirected toward useful purposes. There were several reasons for this reorientation. Some of the

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new structures, like big research centers (*Großforschungszentren*) and big research associations (*Großforschungsverbände*), did not seem to work well, and complaints were voiced both by industry and by scientists (Schramm 2008: 88–91). In addition, the change in party leadership from Walter Ulbricht to Erich Honecker in May 1971 brought an end to the economic reforms of the 1960s. This affected science policy, as the reforms in economic and science policy were understood as a comprehensive attempt to modernize society.

This new tendency becomes clear in the statements of Kurt Hager (1912–1989), who was the director of the Department of Science in the party's Central Committee (Müller-Enbergs 2010). In 1972, he reiterated the importance of the "scientific-technological revolution." Science, he explained, was more than just a means of production, in many cases it was a point of departure, meaning that science pervaded all aspects of society. Therefore, science was not only a means of economic managers, but rather a precondition of economic management. However, he warned that the "scientific-technological revolution" would not proceed automatically but only through its conscious development and effective use by the working people (Hager 1987: 10, 20).

The cardinal question for the material basis of communism, Hager went on to explain, was the organic union of science and production. But he did not conclude that science always had to be oriented toward specific social aims. Rather, an orientation toward the needs of only the next few years was as inappropriate for science as an orientation toward the distant future (Hager 1987: 17).[4] Therefore, socialist science policy had to determine the optimal relation between basic research, applied research, and development. Basic research for Hager was the quest for fundamentally new knowledge about hitherto unknown objective relationships based on natural laws. It gained a new legitimacy by creating the theoretical basis for applied research, which drew on the stock of knowledge created by basic research. This meant that basic research could not be measured according to its economic usefulness and that there were limits to planning, because basic research was more oriented toward long-term results and also contained a high risk of failure (Hager 1987: 30–32). In the end, his idea of the relationship between basic and applied research was very close to what in the West would be described as the linear model of innovation. While he acknowledged that a clear distinction between basic and applied research was often difficult to draw, he insisted that there were a number of different stages between basic research and technical implementation. Applied research takes the ideas and results from basic research and transforms them into construction plans or technical designs, which are then implemented in industry or other branches of the economy (Hager 1987: 33).

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The continuing rhetoric of the "scientific-technological revolution" obscured the fundamental shift in science policy. Basic research was explicitly recognized as both different from applied research and as a worthy undertaking. From the early 1970s onward, it no longer faced the same ideological pressure that had led to the introduction of terms such as "targeted basic research" or "application-oriented research" in the late 1960s. Even when Kurt Hager (1987: 131) claimed in 1981 that science and production would melt into a dialectical unity, this did not mark a return to the ideology of the 1960s. On the contrary, he stressed the importance of the linear model again by stating that there should be a uniform process from knowledge production to material production—that is, from basic research to applied research to technical development and innovation. He particularly emphasized the importance of the transitions between the different stages. In addition, he maintained that science was not only a force of production, but also "a means to realize our humanist ideals" (Hager 1987: 135). In view of this, he distanced himself more explicitly than he had done before from the idea of the scientific-technological revolution, in which science was primarily seen as a productive force.

The linear model did not remain confined to the realm of rhetoric. In 1975, it was institutionalized in the planning of research projects that were either concerned with basic research, applied research, or development. Each project was divided into several stages: basic research (*Grundlagenforschung,* G1–G4), applied research (*angewandte Forschung,* A1–A4), and, if appropriate, development (either *Konstruktion,* K1–K11, for the production of material goods; *Elektronische Datenverarbeitung,* E1–E6, for software development; or *Verfahrensentwicklung,* V1–V11, for process technologies; Gläser and Meske 1996: 150–151). The latest stage (K11, E6, or V11) meant the introduction of the new product or process into serial production. All in all, the process comprised up to nineteen stages, depending on the type of project. At certain stages, especially at the beginning of a project and at the end of the basic research or applied research phase (G4 and A4), the researchers had to defend their research program or their results in front of their contract partners, either state bureaucracy or industry. In principle, the party and state bureaucracy had a scheme at their disposal with which they tried to steer a research project from the very beginning to the introduction of its results into industrial production or some other form of application. That scientists tried to circumvent this bureaucratic control is another matter, which cannot be dealt with here in detail. The important point, however, is that the original idea of the "scientific-technological revolution," that science was so intertwined with society to make any distinction between basic and applied research obsolete, was silently dropped. What took its place can only be called a bureaucratic and overformalized version of the familiar linear model.

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What can we learn from these developments? Science policy in the G.D.R. showed some parallels to the one pursued in Western countries, but also some differences. Perhaps most striking is the difference in timing. Whereas in Western countries new paradigms like "mode 2" or "knowledge society" or "triple helix" vied to replace the linear model from the 1970s and 1980s onward, in the G.D.R. the "scientific-technological revolution" dominated in the 1960s, but was then replaced by a formalized linear model. It is interesting to see that similar (though by no means identical) solutions have been tried at different times in different societies, but it is difficult to draw any straightforward conclusions. For example, it would be tempting to argue that scientists in the G.D.R. could have achieved more without the constraints of an overly bureaucratic linear model imposed on their research, but it is difficult to measure effectiveness in science beyond the number of citations, and therefore it is difficult to attribute any effects to science policy.

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## Notes

1. There are reasons to assume that the theory of the "Scientific-Technological Revolution" was a precursor to theories of the "knowledge society" and of "mode 2" of knowledge production. For example, Stehr (1994) points out that science becomes an "immediate productive force" in the twentieth century, a point that had already been made by Marxist thinker Gerhard Kosel (1957: 48), who had argued as early as 1957 that economic theory had to acknowledge science as a productive force in its own right. In addition, the sociologists of science arguing for a "mode 2" of knowledge production argue along the same lines. They speak of an "increased marketability of science (and not only of technology)" (Gibbons et al. 1994: 46). Even the point that "mode 2" knowledge is generated in the context of application (Gibbons et al. 1994: 54) can be found in the work of John Desmond Bernal, who argued that science in general arises out of an understanding and mastering of production processes (Bernal 1961: 28).

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2. The following documents were used: Anton Ackermann, *Die Bedeutung des dialektischen Materialismus für die Natur- und Gesellschaftswissenschaften,* lecture, 14 October 1948 (Malycha 2003: 242–249); Fred Oelßner, *Die Bedeutung der Arbeiten des Genossen Stalin über den Marxismus und die Fragen der Sprachwissenschaft für die Entwicklung der Wissenschaften,* lecture, 23 June 1951 (Malycha 2003: 379–386); Minutes of the meeting of representatives of the Academy of Science with President Otto Grotewohl, 28 November 1951 (Malycha 2003: 395–403); State Planning Commission, *Die wissenschaftliche Arbeit der Deutschen Akademie der Wissenschaften im Zusammenhang mit den Volkswirtschaftsplänen,* November 1952 (Malycha 2003: 417–425); Central Comittee, Science Section (ZK-Abteilung Wissenschaft), *Die nächsten Aufgaben an den naturwissenschaftlichen, technischen, medizinischen und landwirtschaftlichen Fakultäten,* May 1957 (Malycha 2003: 550–569); Werner Hartke, *Das Verhältnis der Akademie zu den Hochschulen,* report, 24 April 1961 (Malycha 2003: 647–654); all translations by the author.
3. Unlike the research council (*Forschungsrat*) established in 1957, the jurisdiction of the ZFT comprised only industrial research, and not research conducted at universities and Academy institutes. The research council was situated on a higher administrative level, being subject only to the government (Ministerrat), and not to the State Planning Commission (Staatliche Plankommission).
4. This point has also been made in other contexts, such as the United States or Nazi Germany (Schauz 2014).

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Chapter 7

# Applied Science in Stalin's Time

## Hungary, 1945–1953

*György Péteri*

In its East Central European mutation of the immediate postwar years, as in several later phases of the region's history, the distinction between pure and applied science manifested itself as a conflict-ridden relationship between, on the one hand, the autonomy and freedom of science (*a tudomány autonómiája és szabadsága*), and, on the other hand, the central planning of the scientific endeavor (*a tudomány tervezése*), revealing invariably the relationship between politics and academia as the underlying issue of the debates.[1] Even though the bipolarity "autonomous versus planned science" is admittedly not coextensive with the bipolarity "basic versus applied science," the two distinctions overlap to a significant extent. From an early point of time in the debates dividing the academic elite and in the actual reforms carried through before 1947 with regard to the Hungarian Academy of Sciences (HAS), the tendency that asserted itself most forcefully was to promote the positions of applied and technological sciences, to curb the old-time predominance of arts and humanities, and to accept not merely the public accountability of science and scientists (and not merely the idea of planning *for* science), but eventually also to yield to the Communist design to extend central planning over the domain of academic endeavor.[2] Concurrently, principles such as the freedom of scientific inquiry, the autonomy of scientists and scientific institutions, a viable balance between basic and applied research, the unity of research and teaching (in higher education), etc., were ridiculed as illusory or even demonized as reactionary agendas. Indeed, by the early 1950s, universities, research insti-

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tutes, and the various sections of the Academy had to give regular reports on "the use of the achievements of Soviet Science" and on "the struggle against foreign and domestic hostile ideologies."[3] In a draft note qualified as "strictly secret" from 16 March 1953, Chief Group Leader of Organization István Juhász pointed to the following examples of troubling "idealistic tendencies":

a. The unwillingness to address theoretical issues, practicism.
b. Adherence to the [idea of] "pure science" [*tiszta tudomány*].
c. [skeptical] Attitude towards the possibility of planning science.[4]

Between 1945 and 1948, in the so-called coalition era preceding the open Communist takeover, there were reasons to hope that the new emphasis placed on applied science could be beneficial, both in terms of an improved social status (and better funding) of science and scientists, and in terms of promoting the production of more useful knowledge. By the 1950s, however, the regime's apparent preoccupation with applied knowledge revealed its full destructive potential, particularly in the field of social sciences. In what follows, I will first discuss how the autonomy (or freedom) of science became "the enemy of social progress" in the discourses of the political left of the coalition era (1945–1948). Thereafter I will have a look at the period of high Stalinism (1948–1953) and show the consequences of the cult of applied ("practice-oriented") knowledge in the field of economics by discussing the contemporary (Marxist-Leninist) meanings of the concept of "practice."

## Applied Science, Planning, and Social Progress, 1945–1948

The January 1947 inaugural lecture of political scientist István Bibó could hardly have treated a topic more timely than the separation of powers. Bibó, newly elected into a corresponding membership by the HAS and a centrist member of the National Peasant Party's leadership, was a scholar deeply involved in the political debates of the coalition period. The lecture on the separation of powers was neither the first nor the only one of Bibó's works from this period that reflected his increasing apprehensions as to the fate of basic norms and institutions of democratic politics and society. He urged the preservation of and adherence to the old European tradition of separation of powers, for he regarded it as a principle of great relevance for the present as well as for the times to come. Of the rich content of this principle, Bibó (1982: 555) gave special emphasis to its central idea that "technically, power can most effectively be subjected to the need of acquiring moral legitimacy by the disruption of power concentration, by the separation of functions from one another, and by the establishment of power centers opposing one another

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and generating particular identities." One field of social activity where, in his view, "the demoralizing impact of power concentration" was most dangerous was that of "intellectual life, culture." He reiterated the increasing practical and ideological significance of science for the state. This, in combination with the technological revolution that had created the mass media, with their enormous efficiency and power in shaping public opinion, and had created mass culture, providing a major arena for political propaganda, exerted a mighty push toward a concentration of power beyond all previously known proportions. From the viewpoint of democracy, this trend implied grave dangers by "bringing mass culture into a relation of dependence to the objectives of state power and, on the other hand, [by] making state power a prisoner of its own propaganda" (Bibó 1982: 557). As a "classic example" and a signal warning of a universal tendency, Bibó (1982: 557) referred to German national socialism "which, if it did not want to lose all its momentum, had to follow its own propaganda and, exactly by following its own propaganda, ran directly into its own great historical catastrophe." Bibó therefore urged for an improved defense of democracy by measures taken to "make the scientific, artistic, and educational professions, similarly to the position of the judiciary, autonomous." He believed "state power and science have to be separated from one another lest their fusion leads necessarily to the complete corruption of intellectual life and cultural production" (Bibó 1982: 557).

It is at this point that Bibó's actual agenda with the essay emerges—namely, to engage in the ongoing debate on university and academic autonomy in Hungary and to argue against suggestions to destroy institutional autonomy with reference to the authoritarian (eventually fascist) rule in the country preceding 1945:

> In connection with this question, quite a few people mention the universities and the Academy which more or less defend their historical autonomy. These [institutions] are exposed to assaults on grounds that their autonomies are merely crystallization points of certain personal and social power relations. However, this only means that the Academy or the universities provide too narrow frameworks. The recognition of that justifies not the destruction of autonomy but, on the contrary, the organization of it on an even larger scale. It confirms that the whole intellectual life, the whole cultural production and the consumption of mass culture necessitates the establishment of some apparatus of autonomy. . . . The contours, again, of some kind of a scientific or cultural "state power" are taking shape, which, just like the judiciary, will have to gain by struggle its independence, its autonomy, and its constitutionally guaranteed separation from the concentration of power. (Bibó 1982: 557–558)

In fact, Bibó's attention to the problem was prompted by the serious threats that the country's academic life was exposed to, rather than by any distinct

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trend of emerging or increasing autonomy in the various fields of intellectual endeavor—and the challenge came from the Communists' side. Neither home Communists nor those returning from Moscow can be said to have resumed their political activity in the possession of a coherent set of objectives and policy proposals concerning science in late 1944. Matters pertaining to cultural policy, and especially to the organizations and practitioners of science, seem to have been, at least until 1948, of interest for them strictly from a political point of view. The fear that "reactionary" (anti-Communist) politics may find shelter under the roofs of the country's academic institutions appears to have been the main underlying motive of their utterances. This is reflected in their preoccupation with the ideological affinities and political affiliations prevalent in the academic community.

This, of course, is not to say that before 1948, there had been no indications whatsoever of how Communists envisaged the role of science in society and its mode of operation at a future "phase of development." The demands they formulated with an increasing clarity and resolution concerned the position of Marxism-Leninism in the country's intellectual-academic life, and, more specifically, the closely related issues of academic autonomy and planning. I will discuss the latter point in greater detail in what follows.

For Communists, academic autonomy was, from an early point of time, a thorn in the flesh. As we have seen, the fact that certain personalities of the Horthy era's conservative establishment could find (for a rather short time) shelter in university autonomy was regarded by István Bibó as an argument *for* broadening and further consolidating the constitutionally guaranteed autonomy of intellectual life. The Communists had a diametrically opposite view. A strengthening and consolidation of institutional autonomy with regard to the HAS and the universities was the hope of many other conservative reformers in the country's academic life, such as historian István Hajnal, professor of law Gyula Moór, or the musicologist and composer Zoltán Kodály. In the coalition government formed after the November 1945 elections, literary historian Dezső Keresztury, of the National Peasant Party, became the minister of religion and education. His policies may rightly be characterized as "democratic and well-balanced" (Borbándi 1983: 409). The point of departure, and, at the same time, one of the major objectives of his policies was the vision of a democratic Hungarian society, in which the political-cultural life was arranged along and according to the community of interests of peasantry, workers, and intelligentsia. Being a true democrat, he wanted to promote the freeing of society from feudalistic barriers to social mobility; he wished to contribute to the development of a societal organization and stratification, shaped not by the distribution of status by birth but by the division of labor, the very basis of human-social life. He wished to contrib-

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ute to the education system reforms so as to sustain a healthier distribution of social positions according to competence and expertise. He welcomed the land reform and its effects upon the Church—namely, that the latter ceased to function as one of the mightiest landlords—because he thought this was a precondition for their return to their true mission of taking care of people's souls and working for a Christian socialism. He supported the democratization of culture, which for him meant "the freedom of thought, inquiry, and opinion, and the freedom of conscience as well as free access to education, [and] the right to share the products, weapons and tools of human intellect" (Mai magyar művelődéspolitika 1946: 7–35). Keresztury, similarly to other conservative reformers, was aware of the need for modernizing reforms in the organization of academic activity. He wished to transform the HAS into a nodal center for coordinating the nation's scientific enterprise. But he was also deeply cautious of the dangers implied in the abandonment of the principle of autonomy. Therefore, in 1946, he played a crucial role in restoring the consensus and unity within the HAS[5] and did his best to secure funding for it so as to preserve its integrity (Mai magyar művelődéspolitika 1946: 109–110). His policies were soon fiercely criticized by the Communist and the Social Democratic parties and, eventually, in late 1946, he was forced to resign. In March 1947, Gyula Ortutay of the Smallholders' Party took over Keresztury's chair.

Ortutay proved to be an easy match for the Communists. Despite his seeming resistance, he was not more than a supernumerary in the historical drama of the *Gleichschaltung* of Hungarian cultural life staged and directed by Mátyás Rákosi and his party. Ortutay yielded to Communist pressures toward the introduction of centralized administration of academic life, while at the same time he tried to avoid the use of coercion in openly violating the principle of autonomy. He pointed, therefore, to the "excellent example of the Soviet Union for good academic organization," where central administration and coordination was combined with "the possibility of completely free research." This example could be followed by little Hungary only if the Academy voluntarily undertook to renew itself and was prepared to assume the role of centrally administering the nation's intellectual endeavor (Ortutay 1949: 81–82). As Communist political dominance became increasingly obvious and self-asserting, however, Ortutay, too, started using a sterner voice. On 23 February 1948, he warned the HAS in a presumptuous speech delivered in the Parliament that he would proceed with his (!) plan to establish a "highest council for sciences" even without the Academy's cooperation, if the latter could not find their way to cooperate with his ministry.[6] He also declared "with the greatest resolution"—as if he had run any risk of resistance on the part of the universities—that in his view the only acceptable reason to

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preserve university autonomy was to guarantee a satisfactory standard in the nominations to professorships and senior fellowships submitted by the universities to the ministry of education.[7] Half a year later he asserted that "the interest of the Republic" was superior to all autonomies in the cultural sphere and demanded that the universities secure "ideological purity" not only at the departments of social sciences but in other faculties too.

Until 1948, György Lukács had been one of the highest Communist authorities on issues of cultural and science policy. His views are worthy of careful consideration also because they were representative of the "right wing" (as distinct from the "hardliners") of postwar Hungarian communism. According to his article published in the Communist party daily, the *Szabad Nép,* culture was the sector where the ancien régime had the strongest positions (Lukács 1946). Part of the explanation was, in Lukács's opinion, to be found in the fact that "for quite a long time, until after the national elections, the ministry of education had been one of the main strongholds of the reaction organizing its counter-attack." Another factor he named was the "excessive loyalty" on the part of the democratic parties toward the major institutions of Hungarian culture, including the universities and the HAS. This "excessive loyalty" manifested itself, Lukács wrote, in that "the democratic parties left completely intact the autonomy of the universities and the Academy, leaving to a generous extent to their discretion to decide upon their own transformation, on the renewal of the content, organization, and personnel aspects of their work." Right after the country's liberation in 1945, Lukács contended, radical reforms in both of the institutions would have met little resistance. Notwithstanding, a whole year had gone during which, so Lukács echoed his party's judgment, democratic reforms in the cultural field had made no progress. This they regarded only the more disappointing as "the new tasks would be enormous." Among the latter, as listed by Lukács, "national organization and planning of the natural and social sciences" figured first, a task that in his view the Academy was incapable of solving "on account of its organization and the composition of its membership."

Another active spokesman of the Communist Party in matters of science policy was the young historian Károly Vigh, member of the Teleki Institute and secretary of the Communist Party Organization of Scientific Institutions, established in October 1946.[8] Vigh was actually the first to spell out the Communist views on matters pertaining to the HAS and to science policy in general. On 7 September 1945, he delivered an opening speech on "University, Science and Academy," arranged by the Free Union of Hungarian Pedagogues. In a sweeping attack on the universities and the HAS, he described the whole edifice of Hungarian learning as thoroughly reactionary in outlook and ideology, and alarmingly backward compared to the science of the "great

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democracies"—meaning the Soviet Union, Great Britain, and the United States.[9] As the least tolerable feature of the state of Hungarian science, he named the lack of organization and leadership in the country's academic life. In this respect, he suggested following the Soviet example, since "the question of the management of science was first solved there." That meant that the Academy was, in his view, to play the role of the central policymaking and planning authority over science. In order to be able to fulfill that role, however, the HAS, Vigh contended, had to go through a purification whereby the "democratic forces" would take over the leadership in it. He warned the Academy that although the democratic government respected their autonomy, they would not be able to wait too long for the renewal (Vigh 1945: 130–133).

During the later years of the coalition period, Vigh was a regular contributor to the communist weekly *Tovább,* where he specialized in matters pertaining to science. His two-part article, "The Organic Disorders of Our Academic Life" (Vigh 1947a, b), published more than a year after Lukács's unfavorable diagnosis, bears witness to increasing Communist discontent with the slow adjustment of academic institutions to the demands of "new democracy." Vigh found that "the Horthy era's counter-revolutionary superstructure" was hardly affected by the changes after 1945. He urged, therefore, the launching of a "concentrated ideological crusade by the progressive forces of Hungarian intellectual life." He approvingly recalled a lecture delivered by Béla Fogarasi that highlighted the danger that a gap might emerge between academic life and democratic development in other areas, and warned that "the reaction, under such guises as the demands of competence, freedom from political influence, autonomy, etc., would undermine democracy" (Vigh 1947a: 8). Vigh demanded that in the higher education of social sciences, especially at the faculties of arts, economics, and law, a greater number of the professors should be from among "the progressive representatives of science." By demanding this, he meant to make the presence of Marxism—meaning, of course, the Stalinist variant of Marxism-Leninism—in higher education correspond "to the avant-garde role undertaken by the workers' class in Hungarian democratic development" (Vigh 1947b: 8). Among other things, he also demanded that the curriculum at the universities and high schools should be centrally prescribed and obligatory, "as it is a wrong interpretation of academic freedom when the professor teaches and the student takes whatever they prefer instead of what really is *needed.*" In this article too, "lack of planning" was pinpointed as one of the main "organic disorders" of academic activity, and Vigh suggested the establishment of "a planning committee for culture, universities and science to coordinate the three-year plans of the individual research institutions."

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A fairly detailed presentation of the Communist view on major issues of cultural (science) policy was published by István Király in July 1946 in *Társadalmi Szemle*, the theoretical journal of the Communist Party. Király alleged that Keresztury's ministry had been involved in a consistent sabotage against "democratic cultural policies." In the field of science policy, he contended, the ministry refused "to go to the root of the problem and solve the issue of the Academy of Sciences, the scientific institutes and the universities according to uniform standards" (Király 1946: 520). These "uniform standards," he suggested, should be modeled on the "academic industries of great democracies." Király, like many of his party comrades involved in the siege on the "reactionary academy," did not fail to seize an opportunity in the ongoing debate within the academic community at that time. He maintained that what professor Szent-Györgyi, in the lead of "our excellent, progressive scientists," recognized was "that the development of modern sciences, their complexity, [and] the great tasks they are facing necessitate the planning of scientific activity by concentrating the individual parts into a big totality" (Király 1946: 526).

Király implied that the ministry's failure to address the problems of science policy in a proper and effective manner was mostly dependent on the defeatism they exhibited in relation to academic autonomy. He argued that academic autonomy had two main aspects. One was its concrete, historical form. The prevailing historical form of autonomy was, he reminded, "born in the struggle of bourgeoisie against feudalism." Capitalism wanted to set science free from the Church's guardianship "in order to pave the way for a rapid and free development of the forces of production." But there was a universal aspect of academic autonomy too, not subject to historical determination, Király continued. This universal meaning of autonomy was that "scientific activity is only possible in complete freedom." In Király's understanding the relation between the two aspects of academic autonomy was a dialectical relationship between form and content.

> The historical and universal meanings of autonomy come in the course of societal development into conflict with one another. . . . The contradiction between historically determined and universal autonomies manifests itself under reactionary [regimes] . . . . Ruling reactionary regimes, surviving their own historical time, often wish to impede the development of society and the forces of production. Under such circumstances, sciences that find themselves inside the autonomy would become "official sciences" in the negative sense of the word, trying to resist life, while the science that genuinely serves development, together with its representatives, would be forced to stay outside the walls of official institutions. This false academic autonomy tends to undermine its own fundamental principles: it is sustained by brute force and, following the col-

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> lapse of the regime, it will, sooner or later, necessarily also fall to give its place for a new harmony emerging between the two meanings of autonomy. (Király 1946: 525)

Király (1946: 526) made no secret about his and the party's opinion that in Hungary, "even today, it is mostly the 'official scholars and scientists' of the past who hide themselves behind the academic autonomy; with the help of autonomy they wish not to promote but to impede development." He insisted it was the duty of Keresztury's ministry to help the purging of academic life and to support within the autonomy "the progressive forces in their struggle against reaction." Being, however, itself dominated by a "reactionary personnel," the ministry had, in Király's view, "built a common front with . . . the religious and academic reaction against progress and democratization."

György Lukács too, as we have already shown, viewed the universities and the HAS as "the citadels of reaction." An unmistakable proof for this was, he suggested, the fact that "the leading ideologues of democracy have not yet been able to take their due positions" (Szent-Györgyi et al. 1946: 9). Like Király and Vigh, Lukács saw a direct relationship between the "academic reaction's" ability to persist and the misuse of academic freedom with the support of the ministry of education. This view was shared by yet another Communist personality of significance in matters of cultural policy, Géza Losonczy (Szent-Györgyi et al. 1946, 15–16).

A similar opinion was held by Béla Fogarasi (1948: 202–203), who said, "The incorrect interpretation and use of autonomy is one of the organic diseases of our universities."[10] He saw it as a continuation of a bad tradition from the pre-1945 era that the received understanding of autonomy "gives, from a professional-scientific point of view, no guarantee for the [proper] selection of professors . . . as it is not the objective professional criteria that are decisive." Similarly to other Communists who made public their view on the issue, Fogarasi asserted that "autonomy, as practiced in our country" had "become a barrier to development." Whereas, he added, the people's democracy actually supports "a genuine academic self-government," it "must not permit such abuses as are being done with autonomy" (Fogarasi 1948: 202–203). The same ungenerous attitude was applied to "academic freedom for the professors," meaning, "that they are free to publish and teach the results of their research, but it does not mean that they should even today have the right to teach law and economics in the spirit of the Horthy era [*Horthy-szellemben*] as it is being done in certain faculties. This would mean freedom to be unscientific and not freedom of science" (Fogarasi 1948: 208–209).

Fogarasi was also a leading propagandist of the idea of academic planning. He envisaged the transition of Hungarian science into modernity through

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"uniting our atomistic academic life."[11] In an article from early 1947, he hailed the three-year economic plan as the necessary basis for a "truly democratic" culture. Fogarasi, in the footsteps of Lukács, distinguished between "formal" and "genuine or true democracy." He found that while in the field of economy "true democracy" was developing due to the introduction of central planning, in cultural life "formal democracy" prevailed. This explained, said Fogarasi (1948: 192–193), why "there is hardly any other field of our public life where the reactionary forces fighting against progress have managed to persist in their positions acquired during the Horthy era to such an extent as they have in culture." Fogarasi's argument built on a distinction between a democratic and an aristocratic notion of culture, the former meaning "the introduction of the idea of planning into cultural construction," while the latter was associated with "a chaotic, unorganized, and atomistic state of culture." This was mainly a replica of another simplistic train of thought contrasting the ideal types of planned and market economies. In both cases, of course, it is central planning that comes out of the comparison triumphant. Moreover, economic and cultural planning, within the frameworks of the totalitarian design, necessitate and legitimate one another:

> The economic plan is the basis for the planned management of culture. Without [planning the economy] cultural planning would remain empty words. However, the realization of economic plans makes the planned management of culture necessary too. It is obvious, that a precondition of the realization of economic plans is the provision of the necessary intellectual labor which, in turn, demands the planned management of the whole education. From the viewpoint of the realization of economic plans the planned development of sciences is of enormous importance too. (Fogarasi 1948: 194)

Understandably enough, Communists paid little attention to the risks central planning might entail from the points of view of academic autonomy and freedom and, thus, for the development of intellectual endeavor. In their vision of the world, there did not, and should not, exist small "intermundia" where intellectuals could hide and devote themselves to an uncompromising search for truth. They started out from the firm belief, inherent in their class-relativist epistemology, that "the 'autonomy' of science is an illusion . . . . *Science has never been independent of society,* nor has it been independent of the ruling class in the society. . . . The autonomy of research institutes and university departments has also been but an appearance" (Kornai 1948: 4). Instead of chasing the "illusion of autonomy," the academic communities were advised to accommodate themselves to "the needs of society"; after all, "it is the needs of the development of the forces of production which determine the directions and subjects of research and not the other way around!" (Fogarasi

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1948: 188). Applying Engels's concept of freedom, Fogarasi even managed to make the goods he offered look like "freedom":

> Only when science becomes aware of those societal relationships that determine its development, only then it will be free in the truly scientific sense of the concept. . . . The planned management of sciences is the organizational expression of this awareness. . . . We know also from the practice of the Soviet Union that the government does not in any way impede individual initiative either in academic life or in the whole of social life. Rather, they support it in every respect. (Fogarasi 1948: 188)[12]

After 1948, Communist writings on the "necessity" of planning scientific research on a national scale ceased to contain the early, vague references to the experience of "great democracies." What remained was, on the one hand, the imperialist West characterized by decadent art and bourgeois science incapable of development, and, on the other hand, the Soviet Union with intellectual achievements "impossible to surpass." Given the two-camp structure of the world as seen by the Cominform, the Soviet-type academy became the model to be followed by Hungary too. Soviet academic life was regarded as the case proving the advantages of central planning and organization. The Soviet example was cited against those who attached excessive importance to the role of coincidence, intuition, and to the genius of individuals—other than Marx, Engels, Lenin, and Stalin—in scientific progress. The Soviet example was declared to have proven that science developed best if determined, through central planning, "by the needs of the time." When the Hungarian Council of Sciences (HCS)[13] was just about to start its activities to introduce "planning and organization" into Hungary's academic life, the conflict between central planning and academic autonomy was dealt with in an orthodox Stalinist manner: "We do not promise some nonexistent 'autonomy' to the sciences. We would like to develop Hungarian scholarship to something like the Soviet one, which—as was stated by Stalin—'does not isolate itself from the people, does not keep itself away from the people, but [is] ready to serve the people, ready to give the people all the results of sciences and serves the people not under compulsion but voluntarily and happily'" (Kornai 1949: 11).[14]

## The Meanings of "Practice"—Hungarian Economics under Stalin

Studying the research programs of the early period of the new Marxist-Leninist economics, one cannot help being profoundly impressed by two features: first, the excessive role of ideology and propaganda, including the boom in the production of stenciled coursebooks; and, second, a particular

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kind of utilitarianism—namely, the urge to be of use for what was defined as "practice" (*gyakorlat* or *gyakorlati élet*). Indeed, it seems that in this second phase of academic life under Stalin (1949–1953), we can observe yet another discursive mutation of the basic versus applied science controversy—one that may be best articulated as some kind of bipolarity between *l'art pour l'art* theorizing on the one hand and practice-oriented knowledge on the other.

The first "scientific plan" defining the program of economic research for five years consisted of a total of twenty-one projects.[15] These projects were as follows:

1. studying political economy, Soviet works, translation of university books (Institute of Economics)
2. translation and publishing of classical works of Marxism-Leninism (*Szikra* Publishing House)
3. writing a book in political economy for secondary schools (Institute of Economics)
4. writing a textbook in political economy for universities
5. writing a university textbook in economic history
6. studying the theoretical and methodological literature of planning in the Soviet Union and in the people's democracies, and the translation of appropriate works in this field (Institute of Economics together with the Hungarian-Soviet Economic Review, and the Centre of Economic Documentation)
7. studying the cooperative forms in the Soviet Union and the people's democracies (Institute of Agricultural Organization and Institute of Economics)
8. developing a Hungarian terminology for the socialist planned economy (for this purpose a separate "interdisciplinary" Committee of Economic Terminology was established)
9. studying the system of national balances (compilation of balances of national income and gross social product, *társadalmi termék*) (Institute of Economics)
10. studying the Marxist theory and methods of calculating national income (translation of relevant soviet works) (Institute of Economics)
11. developing methods for the planning of wage funds (*munkabéralapok*) (Institute of Economics)
12. developing the best methods for performance-bound wage policies (various ministerial organs under the leadership of the National Office of Labor and Wages, with the cooperation of the Institute of Economics)
13. assessment of productive fixed capital (*termelés állóalapjainak felmérése*) and of the necessary extent of writing down its value

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14. problems of monetary planning (Ministry of Finance, Institute of Economics)
15. development of methods for assessing and planning the productivity of labor (Planning Office, Statistical Office, Office of Innovations)
16. development of methods for assessing, planning, and reducing costs (economic ministries and Planning Office)
17. studying the problems of working capital on macro, branch, and micro levels (Ministry of Finance)
18. translation of the Soviet university book in General Statistics
19. studying the systems of accountancy and the experience of the Soviet Union and the people's democracies in planning, organizing, and controlling industrial units (University of Economics, University of Technology)
20. developing methods for industrial planning, organization, and control (University of Economics, University of Technology)
21. providing information about, studying, and assessing the economic life of the Soviet Union and the people's democracies, with a special view to economic cooperation (Institute of Economics)

The program shows quite clearly the unsatisfactory direction into which economic research was forced by the new regime: the alternatives were either to engage in ideological propaganda, most safely and effectively done by translating the output in political economy of the Soviet Union and the people's democracies, or to support the operative, day-to-day nitty-gritty of central economic management in the various authorities—for example, by helping to develop methods and instruments to assess and control economic processes. Several items of the program may have necessitated the work of trained professionals, but none of the enlisted projects, nor any combination of them, belonged to the proper domain of economic research. Indeed, the early programs of 1950–1953 indicate a conspicuous absence of explicit theoretical assumptions waiting to be confirmed or refuted and, especially, of serious questions addressed to economic "reality" at all. This latter feature appears to be all the more perplexing in the light of the claim that new Marxist-Leninist science would be—to a hitherto unprecedented extent—dedicated to "practice." This seeming contradiction cannot be solved unless we consider the various meanings of the concept of "practice" within the academic culture of the early 1950s.

Practice as a criterion steering research activity was a concept of great complexity in the contemporary usage. Communists had little sympathy for the idea of "pure science." They believed that, for science, there was only one source of inspiration and only one legitimate objective: the needs of practical

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life. What was termed "applied science" in other political and academic cultures appears, therefore, to have been closer to their ideal—in the sense of a scientific ethos evoking high responsiveness to the call and needs of society. The concept of practice was often used even to denote "objective reality," one that is "reflected" in scientific propositions. Yet another shade of the concept can be identified in its use as the ultimate and only reliable test of all scientific propositions. When "practice" was used in this sense, the contemporary masters of academic life would have readily quoted what they claimed was Engels' favorite phrase: "The test of the pudding is eating it." In this latter role, the meaning of "practice" appears to be quite close to what normally is called the predictive strength of a proposition or theory in the process of validation.

In the reality of the Stalinist academic regime, however, "practice" would have been first of all an epitome of the goals and projections adhered to by the central political power. The latter regarded itself as the only depository of information concerning "the needs of society," and claimed to possess exclusive mandates to interpret and act upon those needs. Only in this meaning of the concept—that is, as the conversion by the party into worldly reality of the utopian project for mankind's state socialist salvation—would "practice" have indeed functioned as a selection criterion identifying "true science."

The adjustment to this latter meaning of practice is in evidence in the various versions of "scientific plans" produced in the early 1950s. The fundamental principle governing the designers of the five-year plan of economics was stated as follows: "The decisive task of economics is to promote the solution of economic problems to which the building of socialism gives rise in our country."[16] But just as the task of "building socialism" was a business to be decided upon by the party leadership, so was the definition of economic problems arising out of it—as well as the solutions applied to them, of course. Thus, the concept of practice, as employed in science-policy discourses, meant "reality" (the subject of scientific inquiry) only as far as it was identical with "reality" as defined by the political power. In 1950, the Second Section of the HAS was preparing an exhibition to demonstrate the socialist renewal of social sciences and to popularize, among others, the activities of the Institute of Economics. What was required from the Institute, therefore, was a suggestion of what should figure as their "exhibition material." Péter Erdős, although skeptical of the whole idea of popularizing economics in this manner, returned to the Second Section with the proposition, first, to produce a poster showing the growth of the number of "scientific topics" in which the Institute was engaged, and, second, to create yet another poster that would list all the organizations with which the Institute was in touch (government departments, national authorities, state companies, party organizations, etc.).

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This poster had the caption "Practice is the vital essence of science" (*A tudomány éltető eleme a gyakorlat*). Finally, the slogan suggested by Erdős as a heading for the Institute's exhibition board was a revealingly twisted version of a well-known Leninian bit of wisdom: "Politics is concentrated economics. The work of the Institute of Economics too supports our peace policy."[17]

The conflation of the two "realities"—neither of which remotely brought to mind the reality of everyday life as experienced by mortal members of the society—was the very basis for that central ingredient of the official academic culture of our period: the regular exaltations so generously devoted to "works" of top Communist leaders. The imposition of the political definition of reality upon science also provides the explanation for an apparently bizarre episode that took place around 1954 or 1955: the head of the science policy section of the Central Committee of the Communist Party, Erzsébet Andics, instructed economists pleading for access to classified statistical materials to instead study and satisfy themselves with the party's daily, the *Szabad Nép.*[18] In fact, within the frameworks of the Stalinist academic regime, Andics's reaction was normal, and the plea from the economists an anomaly. Politics was the exclusive domain of the party. If politics was "concentrated economics," then the supreme competence to take care of economics was to be invested with the leadership of the party. Another side of the same coin was that, in the period between 1948 and 1956, all previously regular publications of statistical data ceased to exist. The Central Office of Statistics (COS) produced a series of reports as "Strictly Secret!" manuscripts, covering major socioeconomic developments. There existed, however, only ten to sixty copies of these reports, accessible exclusively for the highest echelons of the party-state. Between 1949 and 1954, more than 70 percent of all the copies of such reports went to members of the Political Bureau, secretaries of the Central Committee, and to members of the so-called Organization Bureau of the Central Committee—a couple dozen people at most, constituting the very core of the highest party leadership. In the distribution lists of the COS from the period, I found the name of only one person who could be classified as a researcher in economics at the time, with even this occurring only a few times (Péteri 1993b: 152–153).

After Stalin's death and in the wake of the general crisis that left no aspect of social life unaffected, quite a few high-level leaders of the domain of economic policy[19] reacted to the crisis in a scientistic manner, claiming that the main error had been to strangle unbiased empirical economic research, which left economic policy makers blindly improvising and seldom finding the right course (Péteri 1993b, 1997). Thus, as a contemporary party document claimed, the publications in economics of the years of high Stalinism

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> would not, in general, go beyond the confines of . . . propagandistic arguments on some theoretical questions. . . . The great distance from practical life [*gyakorlati élet*] is indicated also by the fact that the discussions on problems of economics, arranged either by the various departments of the university of economics and of the high party school, or by the editorial boards of periodicals, tend to assume a scholastic direction and to end up in [debates on] how to interpret and explain certain definitions and concepts.[20]

Significantly, as is indicated in this document, the reform era following Stalin's death and its empiricist turn in economics came to assert itself by way of reconstructing the discourse of "practice," nudging it toward the meaning of a social economic reality that defied the projections of political power, and that needed to be studied and understood before it could be affected in accordance with political intentions.

But while Stalin and, in Hungary, Mátyás Rákosi, were in unchallenged power, economists had to make do with studying *Szabad Nép* as the major source of information about economic reality, and with regarding party congresses, Central Committee meetings, and the publication of works of party leaders as the most important "epoch-making" events of academic life. Even the authors of the very first printed book to appear in political economy in the Soviet Bloc unequivocally regarded the resolutions of the Communist parties and the works of the leaders of these parties as major sources and embodiments of new knowledge in economics (Ostrovit'anov et al. 1956: 17). There was, furthermore, no reason to doubt, in accordance with the meaning of "practice" and "reality" specific to the official Stalinist academic culture, that top Communist leaders were by definition the best scientists too. Moreover, as their activities covered a wide range of fields, there seemed to be reasons at hand to believe that Renaissance ideals had come true in them. This is the impression one could get reading, for example, János Kornai's (1950: 921) review on Ernő Gerő's volume of speeches, *In Struggle for the Socialist People's Economy*:

> Besides comrade Rákosi, even the writings of comrade Gerő demonstrate for Hungarian Communists how the doctrines of the classics should be courageously applied (and thereby further developed) to the given situation, to the conditions of people's democracy, of Hungary. This example of the leaders of our Party ought to be emphasized especially because . . . many of our theoretical cadres with an excessive "precaution" and theoretical cowardice shrink from the task of dealing with the elaboration of the theoretical issues of people's democracy. . . . Marxism-Leninism gives the Bolshevik leader a key to the solution of all sorts of questions. But merely by relying on the general doctrines of Marxism-Leninism, without having the concrete special knowledge of the

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> various fields of work, one cannot solve the tasks successfully. Comrade Gerő provides in his book a grandiose example of the Communist leader being always a true *specialist*. He is a specialist—if so demanded by the Party—of the railways, of communication, he is a specialist of finance, agriculture, or of some other field of work. The inseparable unity of theory and practice emanates from these writings.

The academic culture of Stalinism recognized no borderlines between politics, propaganda, and science. One of the first official (although not public) histories on the development of social sciences following the Communist takeover describes this phenomenon as follows: in the period between 1949 and 1953,

> the highest priority for the practitioners of social sciences was to get to know and propagate the doctrines of the classics of Marxism. . . . Besides a certain neglect shown toward research, the importance of propaganda work became paramount. . . . This went hand in hand with the view that blurred the border between research work and scientific propaganda and which undervalued scientific research work. . . . to a great extent, creative scientific work in the fields of Marxist social sciences was replaced by dogmatism, the repetition of classical theses, and vulgarization [of these classical theses]. . . . All in all, it can be stated, that there was hardly any fruitful research work carried on in the social sciences during this period of our development.[21]

Economic research, that is, economics as an intellectual-academic endeavor, could not be restored as long as the "inseparable unity of theory and practice" referred to by young János Kornai prevailed. The possibility to distinguish between ideology and practice, between policy objectives and reality, and between normative and positive statements was a necessary precondition of breathing life into the sleeping beauty of social science. But if such distinctions were to be meaningful at all, practitioners of economics had to have access to the very raw material of their knowledge production: to statistical data and other information embodying "factual observations" of the economy. To achieve that, Stalin's version of state socialism had to undergo a major crisis and some far-reaching reforms in the long decade after the death of Joseph V. Stalin himself.[22]

## In Lieu of a Conclusion

In Stalin's shadow, the concepts of "basic science" (*alaptudomány*) and "applied science" (*alkalmazott tudomány*) may have been absent from science policy discourses of the early years of Communist rule in Hungary. The ten-

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sion characterizing the career of this distinction elsewhere, however, was no doubt present, although the bipolarity assumed varying shapes and shades both in form and contents in accordance with the changing political and (what I had no room here to discuss) academic-cultural contexts.

Seizing on the opportunity offered by the turn of tide in 1944/1945 and, just as importantly, by the overwhelming presence of the Soviet occupation forces, the political left of the coalition era used notions of "autonomous" or "pure" science as an accusation to debunk its political opponents in academia and science policy. In contrast, applied science—that is, knowledge geared to and therefore "useful" for, the grand task of economic and social progress—provided the discursive platform from where relevant and irrelevant, good and bad revealed themselves. The mobilization of scientific knowledge toward the objectives of a social reconstruction that was to bring with it an entire new social order constituted the context in which the binary opposition assumed the shape of "free or pure" versus "planned science." When firmly in power, "planned science" assumed new verbal garments, and the Stalinist order of state-socialism brought social research and thought under its control by imposing the cult of "practice" with meanings all tied to what appeared to be expedient (politically and ideologically correct) for those in power who demanded to be served, not critically studied and understood. This was the very reason Hungary's reform Communism had its origins in the New Course era (1953–1956), when high *apparatschiki* with a scientistic understanding of the crisis of the state-socialist social order and young Communist intellectuals frustrated with, humiliated by, and disillusioned with the Stalinist regime joined forces in promoting the breakthrough of an empiricist research program. At the same time, this was the very reason why the reformist science policy discourse could only make a breakthrough happen by way of (re)conquering the concept of "practice" and thus restoring its objectivistic meaning.

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## Notes

1. In this respect, the authoritarian regime holding sway in Hungary these days, under Viktor Orbán, is no exception. Since 2010 they have displayed in a number of ways their eagerness to impose on cultural and academic life a disciplinary regime organized around *magyar* ethnonationalism and loyalty to conservative values (and to the party of Viktor Orbán) as selective criteria. Government policies propelling the Hungarian Academy of Arts (Magyar Művészeti Akadémia) into a ruling position over the country's artistic life, the ever-increasing governmental-political control over the universities, and more particularly, the ongoing *Gleichschaltung* of historical research and scholarship are clear indications of this tendency.
2. For the history of the rift, the reform, and the Communist takeover in the HAS in the years 1945–1949, see Péteri 1998: ch. 1–4. By 1949/1950, science and higher education in Hungary had shown many characteristics of a Soviet-type academic regime: a large part of the country's research endeavor (its resources and personnel) had been reorganized in the newly established institutes of the HAS, and the Academy assumed a role similar to that of a government department, performing the central planning and management of the national research effort.
3. See, e.g., the report of the Second Section of the HAS on the year 1952: *Jelentés a szovjet tudomány eredményeinek felhasználásáról az 1952. évben* [Report on the utilization of the achievements of Soviet science in the year of 1952], n.d., Archives of the Hungarian Academy of Sciences, II. Social Sciences Division (hereafter MTA LT, II. oszt.), 18/8. Throughout this chapter, all quotes from sources originally in Hungarian have been translated by the author. No punctuation, emphases, etc. are added by the author without explicitly noting it. Original Hungarian wording as well as complementary words sometimes required to enable comprehension, as and when deemed necessary, are given within square brackets: [ ].
4. MTA LT, II. oszt., 16/4. According to an intern letter of the Chief Group Leader, by 18 March 1953 the reports on the struggle against hostile ideologies had no longer been needed, but the questions enlisted in the note "could still be used in compiling the annual reports."
5. In 1945, under the leadership of Albert Szent-Györgyi, a group of scientists left the HAS and established a rival institution, the Academy of Natural Sciences. They then reunited with the HAS in 1946 (Péteri 1991, 1993a).
6. *Országgyűlési Napló,* 51. űlés [Journal of Parliament, 51st session], 23 February 1948: 583. See also his address to the Council of Public Education, 5 February 1948 (Ortutay 1949: 97–98).
7. *Országgyűlési Napló,* 51. űlés, 23 February 1948: 582–583.
8. Cf. "Megalakult a Tudományos Intézmények Kommunista Pártszervezete," *Szabad Nép,* 26 October 1946: 2.
9. From early 1946 and on, references to the "great democracies" were abandoned, and the Soviet academic regime was singled out as the example to be followed.

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There was also a significant silence about Western authors (politically often Left-oriented) proposing, since the first half of the 1930s, reforms in the organization of science toward more planning at the macro level (at least, planning *for* science). For the history of Soviet-type academic regimes in Russia and in post-1945 East Central Europe, and for a discussion of possible influences in postwar Hungary of the British "social relations of science" movement, see David-Fox and Péteri 2000.

10. The text, included in Fogarasi's 1948 book, was a lecture delivered on 4 March 1948 as part of a series of lectures on questions of sciences and arts arranged by the Hungarian Communist party. It may be of importance to note that Fogarasi's bitter accusations as to the misuse of autonomy by the "academic-cultural reaction" date from the time before he himself was appointed to a professorship at the Budapest University. For a list of professors, most of whom were Communists or sympathizers of the party, appointed from 1945 onward to the Faculty of Arts in Budapest, see Sinkovics 1970: 390.
11. Fogarasi's speech on a meeting arranged by the Communist Party in December 1946 is referred to in *Köznevelés* 3, No. 1–2 (15 January 1947): 16.
12. To provide greater credibility to his argument, Fogarasi described in a footnote his own twelve-year experience in the Institute of Philosophy of the Soviet Academy of Sciences where, he assured his anxious readers, he elaborated his individual plans always according to his own intentions. This proves to be an unqualified lie in the light of some of his own letters from Moscow to his wife from that time: "What I would like to write, I cannot write," he complained in one of them. In an autobiographical note from 1957, he looks back on his years in emigration and writes this: "'My career as a philosopher' between 1920 and 1945: 25 years practically lost" (cited in Karádi 1983: 42–43, 46).
13. Following the merger of the Hungarian Communist Party and the Social-Democratic Party on 12 June 1948, in the process of establishing an open Communist dictatorship, the HCS was established as the top governmental organization to perform the central planning of research activities in the country, with far-reaching mandates as to the use of resources provided in the national economic plans and as to hiring (and firing) all the key personnel, including university professors and the senior scientists and scholars at various research institutes. Even though the HCS's design seemingly followed the academy model in the sense that it had a collegium of thirty members—leading scholars and scientists, most of whom were Communists—the HCS could have proved a Hungarian idiosyncrasy in that it arose out of the determination of the Communist leadership (especially Ernő Gerő, the number one power in matters pertinent to economic and related policies) to *sideline* the HAS, allowing it to sink into oblivion, instead of Sovietizing it and assigning it the role that the HCS was to perform. A Soviet intervention in 1949 put an end to this Hungarian *Sonderweg* and, after a thorough purge in December 1949, even in Hungary, the HAS took over the role of the top organization of science (Péteri 1989).

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14. See also Kornai's (1948: 4) open letter to a chemist, starting with the statement, "The 'independence' of science is an illusion."
15. MTA LT, II. oszt., 182/3, "Terv munkái" (this was probably the first draft of the five-year plan of economic research), dated 23 March 1950. For later, more elaborated and extended versions of the plan, see MTA LT, II. oszt., 182/2, "A közgazdaságtudomány ötéves terve" [The five-year plan of economics], by Tamás Nagy, Árpád Haász, Péter Erdős, and Margit Siklós; and "A közgazdaságtudomány 1950-es részletterve" [Detailed plan for 1950 of economics], typescript, 9 May 1950.
16. MTA LT, II. oszt., 182/6, "A közgazdaságtudomány ötéves terve" [The five-year plan of economics], first draft made by Tamás Nagy, Árpád Haász, Péter Erdős, and Margit Siklós, 1950.
17. MTA LT, II. oszt., 182/3, Péter Erdős to Klára Fejér, 2 November 1950.
18. This story was confirmed and retold, although with varying timing, by several of my informants whom I interviewed in the course of my work. For a detailed discussion on the politics of statistical information in the period covered here, see Péteri 1993b.
19. Such as Béla Szalai and István Friss, to mention only the two most significant names in this regard.
20. MTA LT, Papers of the President, Registered files, 58/6. The document, classified "Confidential," is dated 20 September 1954, and is an attachment to Deputy Section Chief Albert Kónya's letter to the president of the Academy, István Rusznyák, dated 20 September 1954. It belonged to a group of documents preparing the establishment of the new Institute of Economics within the Academy of Sciences, with an empiricist research program, under the directorship of István Friss (Péteri 1997).
21. MTA LT, II. oszt., 26/1, "Tervezet, a MTA Társadalmi-Történeti Tudományok Osztálya vezetőségének beszámolója" [Draft report by the managing board of the Social-Historical Sciences Section of the Academy on the ten-year period 1949–1959] , 1959: 2–3.
22. For a detailed discussion of the reforms and changes in the New Course era and the 1960s in Hungarian economic research, see Péteri 1996, 1997.

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Chapter 8

# Theory Attached to Practice

## Chinese Debates over Basic Research from Thought Remolding to the Bomb, 1949–1966

*Zuoyue Wang*

In 1963, the Chinese Academy of Sciences (CAS), which had come under repeated political attacks in the past for pursuing impractical "theory in detachment from practice," sought to take advantage of an interlude of ideological liberalization by codifying the optimum proportions of its various programs. The Academy formulated a set of "work regulations" governing four types of research, which it would engage in during the coming decade: "15–20 percent basic research, 35–45 percent applied basic research, 30–40 percent applied research, and 5–10 percent extension research."[1]

Both the contents of the above policy pronouncement and the fact that it was made at all reflected the central position of the debate over basic and applied research—in its various semantic guises—in the politics of science and technology policy of the People's Republic of China (P.R.C.) under Communist leader Mao Zedong both before and after 1963.The objective of this chapter is to use the basic/applied debate to explore the interactions of historical forces at work in shaping Chinese science and technology policy during the Mao era, which started with the founding of the P.R.C. in 1949 and ended with his death and the termination of the Cultural Revolution in 1976. In what forms did the basic/applied debate emerge in Maoist China as a matter of politics, policy, and rhetoric, and how did it reflect and shape the relationships between the party state and the scientific elite? Did the course of the debate fit, for example, into the pattern of repeated shifts, back and forth, between the techno-bureaucratic "regularization" and the radical "mobilization" models of Mao-era science policy and politics as observed by the China

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scholar Richard P. Suttmeier (1974) and others? And finally but crucially, did the debate manifest itself differently when one moves the focus from the civilian sector and public arena—the focus of most of the studies done so far—to that of the secret world of national security science and technology?

In this chapter, I explore the above questions by examining three distinct phases when the basic/applied debate was key to Chinese science policy and politics: the "Thought Remolding" campaign in the early 1950s to convert a mostly Western-trained and influenced Chinese scientific elite into loyal followers of Communist ideology through a drive to "attach theory to practice"; the paradoxical late 1950s, marked by the decision to build the atomic bomb and political turmoil that led to the emergence of the articulation of the hybrid category of "applied basic research," adopted by the CAS; and then the liberal early 1960s, when the bomb project both drove a pragmatic science policy and shaped the theory/practice or basic/applied debate. Underlining all three periods was the question of balance between needs for political control of scientists and engineers and those for achieving developmental and national security objectives.

## "Science for Science's Sake" and the Thought Remolding Campaign

When the Communist party-state launched the so-called Thought Remolding campaign in 1951 during the Korean War and a brutal anti-embezzlement drive, one of its main objectives was to force Chinese scientists (and other intellectuals) to abandon any illusion of the legitimacy of "science for science's sake" and to serve the practical needs of the state wholeheartedly. The phrase "science for science's sake" was often subsumed under the broader label "theory detached from practice," which was used to attack intellectuals both inside and outside science throughout the Mao years. In the early 1950s, such labels were also dangerously associated with bourgeois values imported from the enemy United States, where many of the senior scientists had received their graduate education, even though by then the American discourse had replaced the concept of "science for science's sake" or "pure science" with the politically more palatable "basic research."[2] For example, a commentary in the official *People's Daily* in early 1951 compared those who sought to pursue "objective" science regardless of its political implications or practical uses to the person who exclaimed "what a sharp chopper!" even after his head was cut off. "To those gentlemen," it continued more ominously, "who uphold 'science for science's sake' and who continue to send their scientific reports and papers to research organs of the American imperialists, aren't you afraid that your scientific reports would be tainted by the sacred blood of the

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Korean People's Army and the Chinese People's Volunteers?" (Yang 1951). This hostile political environment became particularly menacing in the form of shaming or struggle sessions ("criticism and self-criticism"), in which Western-trained senior scientists had to engage in self-criticism of their past bourgeois ways, including beliefs in personal interests and pure science. Such self-criticisms were often followed by painful denunciations and damaging revelations by one's colleagues and former students, which pressured everyone to engage in ever more destructive self-criticisms.

The remarkably detailed and systematic diaries of Zhu Kezhen, a meteorologist who had received his Ph.D. from Harvard University in 1918 and who was appointed as vice president of the CAS in 1949, chronicled both the party-state directives and some of these shaming sessions, including his own in 1951–1952.[3] For example, on 8 November 1951, Zhu noted that he read the published text of a speech by the Chinese premier Zhou Enlai, "On the Issue of the Remolding of Intellectuals," that was delivered on 29 September 1951 (Zhou [1951] 1984). Marking the official start of the Remolding campaign, Zhou's speech was moderate in tone but already highlighted, in Zhu's paraphrasing, "the problem of knowledge, which needs to be tied to practice."[4] On 10 January 1952, Zhu attended a Remolding session at the Institute of Botanic Taxonomy of the CAS, and recorded one criticism that was aimed at the senior scientists at the institute: "the compilation of *The Flora of Hebei Province* was useless and not enough attention was paid to the windbreak forests in the Northeast."[5] In preparing for his own self-criticism, Zhu had to acknowledge that he himself was deeply influenced by Charles William Eliot, president of Harvard in 1869–1909, especially in terms of the ideal of "science for science's sake."[6] At the actual criticism sessions, Zhu, along with several other senior scientists, was forced to admit the sin of "worshipping America." Meanwhile, the unrelenting pressure from both the Remolding and anti-embezzlement campaigns reached a breaking point when Liu Dawei, a CAS scientist in Shanghai, and his wife committed suicide and Wu Youxun, the veteran physicist serving as vice president of the CAS, attempted suicide in early 1952, among other such cases.[7] These incidents helped to moderate the campaigns within the CAS, but not within the universities (Y. Wang 2014: esp. 21, 35, 46).

The attacks on "science for science's sake" can be traced back to Mao Zedong's famous "Talks at the Yenan [Yanan] Forum on Literature and Art" in May 1942 during the Rectification campaign. Mao implored writers and artists to make their work useful for the masses. "Is this attitude of ours utilitarian?" he asked, and then answered yes: "We are proletarian revolutionary utilitarians and take as our point of departure the unity of the present and future interests of the broadest masses, who constitute over 90 per cent of the

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population; hence we are revolutionary utilitarians aiming for the broadest and the most long-range objectives." He then concluded that "there is in fact no such thing as art for art's sake, art that stands above classes or art that is detached from or independent of politics" (Mao [1942] 1965: 86). Mao's early emphasis on revolutionary utilitarianism was extended from art to science in the 1950s. To Mao and his fellow ideological enforcers, "science for science's sake" was not just an issue in the academic debate over the relationship between basic and applied research, but one of political standing and class identity, sometimes carrying with it deadly consequences during the Remolding campaign and afterward. Indeed the text of these Mao talks was required reading during the Thought Remolding campaign which itself was explicitly modeled after the original Rectification campaign (Y. Wang 2014: esp. 13, 17, 56).

Once the Thought Remolding made it clear that "science for science's sake" had no place in Chinese science and science policy, the party-state pushed scientists to work on practical problems. On 21 May 1952, for example, at a high-level CAS meeting, Lu Dingyi, the party's propaganda chief, emphasized that "the priority of scientific research has to be applications," mentioning specifically agriculture and the oceans.[8] Shortly afterward, when the government removed the notorious Chinese Lysenkoist Le Tianyu from his position as director of the Academy's Workshop for Genetic Breeding due to his unusually harsh treatment of Western-trained scientists, it also redoubled its push for the biological theory of Ivan Michurin that had been promoted by Lysenko in the Soviet Union and Le in China.[9] Le was removed partly because he did not carry out the Soviet program correctly and effectively. The point of the government's campaign for Michurinism, Zhu Kezhen noted in his diary, was for biologists "to serve the state in increasing agricultural production."[10] A widely publicized article on the Le case in the authoritative *People's Daily* made this point explicitly and ominously: "The greatness of Michurinian biology was that it thoroughly serves to enhance agricultural production and conquer nature, but this point has not seemed to catch the attention of our biologists."[11]

The same emphasis on utilitarianism was made by Chen Boda, an influential party theorist who served as one of Mao Zedong's secretaries and as one of the CAS's vice presidents, in a major, Mao-cleared speech at the Academy on 18 July 1952 (see Y. Wang 2014: 37). It reflected the new policy of political moderation in Remolding but also firmly came down on the Academy's applied mission while appealing to the scientists' Chinese nationalism: "As to the orientation of the work of the Academy, this is now quite obvious. Much of the work of the Academy should be following the needs of the people, the tasks of the nation at the present, and the tasks in the national construction plans. This requires that the scientists be engaged with practice at the most

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essential level and to the broadest extent possible . . . . To help the nation industrialize is the sacred duty of our scientists" (Chen 1952).[12] However, Chen proposed that research with long-term or indirect applications should not be banned. The point was rather that any personal interests "had to be aligned with that of the entire mass." "Science for science's sake," which carried a particularly individualistic connotation, was out of the question: "one should not conduct research for research's sake; one should conduct research for the people." He also allowed research on "important questions in science," but here the key was who decided what counted as important questions. In his speech, Chen criticized "a few scientists who study the problem of fertilizer without considering soil" as falsely "detaching research subject from its surrounding conditions" (Chen 1952). Yet, five years later, one of the official science popularization journals (by then almost all journals in China were state-owned) published an article translated from a Soviet publication on the merits of "soilless cultivation" (Maertinuofu 1960).

Chen's articulation of science for serving people and practice also reflected the party-state's drive for Sovietization in science policy. In his 1952 speech, he cited not only Soviet political leaders Vladimir Lenin and Josef Stalin but also Soviet scientists Ivan Pavlov and Michuarin to justify a moderate but firmly utilitarian science policy. He urged the scientists to "learn from Soviet science." This did not mean that one could not "consider things produced by British and American scientists," he conceded. Yet he asserted that "in general all the good things from British and American science have already been distilled by the Soviet Union and therefore the quickest and best way is to learn from the Soviet Union." Finally, Chen made planning, a key element of Sovietization, the centerpiece of his proposal for the future direction of the Academy and Chinese science in general, to be carried out in consultation with the production sectors. Science planning would ensure that individual interests and interests of individual research units, including the Academy, would be aligned with practical national needs (Chen 1952).

Thus, even though the phrases "basic research" and "applied research" were rarely explicitly invoked in the Chinese discourse on science policy in the early years of the P.R.C., an emphasis on "revolutionary utilitarianism" was clearly at the heart of the party-state's efforts at ideological transformation of the scientists into party loyalists and at getting them to contribute to economic development, especially industrialization. In fact, it often appeared that political loyalty was more important than practical contributions, and that the emphasis on the latter was to serve the purpose of the former. Part of the reason for this was the party leadership's political distrust of the scientific elite, and part of it was the widespread belief that China could and should rely on Soviet technical assistance for its critical developmental and defense

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objectives. "Rely on the workers for production and on Soviet experts for technology," as the popular saying went (Zhou [1956] 1984; see also Z. Wang 2015: 183). In 1953, when the nuclear physicist Qian Sanqiang, who was also general secretary of the CAS and director of its Institute of Physics, returned from a visit to the Soviet Union as head of a CAS delegation, he went to talk to a deputy minister of machinery-building about the emphasis on semiconductors in the Soviet Union ("working like crazy") and about the need to take action in this area in China. The minister, as Qian recalled later, replied, "There is no rush on this subject. We still have not mastered the technology of vacuum tubes. When we do, then they [Soviets] would have turned their semiconductor research into industrial production. Then we can just 'request' it from them."[13]

Consistent with this widespread reliance on Soviet assistance, it was perhaps not surprising that national security was often conspicuously absent or mentioned only in a nominal way in the party-state's mandates to the Academy and other domestic scientific and technological institutions. The decision in early 1955 to launch China's own nuclear weapons project, and even more the Sino-Soviet tension in this area in 1959, would, however, force the Chinese party-state to reevaluate its attitudes toward Chinese science, scientists, and the proper balance between basic and applied research.

## Theory, Practice, and the Bomb Decision

On 20 August 1954, Qian Sanqiang was invited by Marshal Peng Dehuai, the minister of defense, to give a briefing in Zhongnanhai, seat of the Chinese party-state in Beijing, to a group of high-ranking military leaders on the topic of atomic and hydrogen bombs. Peng was soon to head a Chinese delegation to the Soviet Union to observe a Soviet nuclear test and wanted to explore with the Soviet side the possibility of their providing nuclear assistance to China. "If China wants to build an atomic bomb, how should we go about it? What are the most critical technologies and equipment?" Peng asked. Qian responded that the most important items initially were experimental reactors and cyclotron accelerators, which could be used to train technical personnel, gather talents, and prepare for the construction of both a nuclear industry and nuclear weapons (Ge 2013: 246). This marked one of the earliest direct involvements of Chinese scientists in China's incipient nuclear weapons program. Such involvement would soon expand and have profound impacts on Chinese science, science policy in general, and the debate over basic and applied research in particular.

The Chinese party-state made its decision to launch the nuclear weapons program in late 1954 and early 1955 against a complex international geopolit-

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ical background, including renewed American nuclear threats in the Taiwan Strait crisis in the summer of 1954 and the possibility of the Soviets launching its counterpart to the United States' Atoms for Peace program for nuclear assistance to its allies (Lewis and Xue 1991; Z. Wang 2010). It was during late 1954 that the Science Division of the party's Propaganda Department sent investigators to interview Qian and others about a possible bomb project before drafting a report for the top leadership. Once again Qian suggested that the key was to promote nuclear scientific research, train new talents, and build a nuclear industry as preconditions to the making of the bomb (Ge 2013: 247).

The most pivotal meeting in the atomic decision-making process took place on 15 January 1955 in Zhongnanhai, chaired by Mao Zedong himself and attended by Zhou Enlai and other top party, state, and military leaders. The day before, Zhou Enlai asked Qian and Li Siguang, a geologist who was both minister of geology and a vice president of the CAS, to come to his office for some preparatory mutual briefing. According to a biography of Qian, Zhou told the scientists about the American nuclear threats China had perceived: it started during the Korean War with Harry Truman and then continued with Dwight Eisenhower. The latter's secretary of state, John Foster Dulles, Zhou revealed to the scientists, had tried to pass this message to the Chinese government through Indian Prime Minister Jawaharlal Nehru in 1953: "If a cease fire in the [Korean] War could not be arranged, then the U.S. would no longer be obligated to a commitment of not using nuclear weapons." When Nehru refused to pass the message, Zhou said, the United States tried to spread the message publicly at the negotiation tables in Panmunjom. He also added that the United States made repeated nuclear threats in 1954, from the Dien Bien Phu battle, when the French forces faced defeat by North Vietnamese forces in April/May; to September, when the Chinese People's Liberation Army bombarded Jinmen (Quemoy), an island occupied by the Chinese Nationalist forces; to November, when China sentenced thirteen captured U.S. pilots accused of espionage (Ge 2013: 249).

For his part, Qian briefed Zhou on the status of nuclear science in the West and in the Soviet Union as well as at home, including China's technical training program in this field. He emphasized that all the scientists were full of confidence and wanted to push ahead with the nuclear program. Zhou finally told Qian and Li to come back the next day to give briefings for Mao and others, together with some demonstrations with uranium samples and Geiger counters (Ge 2013: 249).

For our purpose here, what is remarkable about the well-known 15 January 1955 Zhongnanhai meeting is not only the decision on the formal (though still secret) launching of China's atomic bomb project, but also how the deci-

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sion helped to elevate the status of Chinese scientists. At the very beginning of the meeting, Mao announced to Qian and Li that "today, we are like elementary school students getting a lesson from you on issues related to atomic energy," according to an account given by Qian himself some years later. When Li passed around a yellow uranium sample at the meeting, "all the leaders handed it one to another, full of curiosity about its enormous, legendary power." When Qian, with the uranium sample in his pocket, walked by a Geiger counter he had made and triggered it off with loud noise, "everyone in the room broke into happy laughter" (Ge 2013: 250).

The famous statement by Mao announcing the decision to launch the bomb project, as recalled by Qian Sanqiang, also contained subtle hints about the improved fortune of Chinese science in the eyes of the political leadership:

> We now know that our country has uranium mines. After further exploration, we will certainly find even more uranium mines. In addition, we have trained a number of people, laid some foundation in scientific research, and created some favorable conditions [for the nuclear program]. In the past several years, preoccupation with many other things has led to a neglect of this matter. But it has to be taken seriously. Now it's time to go at it. As soon as we put it on our agenda, focusing on it steadily, we will definitely achieve our goal . . . . Now with the Soviet assistance, we should make it work. [Even if] we have to do it on our own, we can also definitely make it work. As long as we have the people and the resources we can create miracles at will. (Ge 2013: 250; see also Z. Wang 2010: 259)

Here, when Mao spoke of "scientific research," its significance clearly went beyond the routine production problems that the CAS was asked to solve earlier; it was now connected with the highest priority of the party-state. Likewise, the criteria of judging the scientific community was no longer primarily its political standing or class identity, but whether it can "make it work." It is true that, at this stage, Chinese scientists were still expected to play second fiddle to Soviet experts in the bomb project and that Mao's overall attitude toward scientists and other intellectuals would not always remain so positive. But it was clear that the bomb project brought some protective effects to the scientists in China, as it did elsewhere.

What was implicit in Mao's statement about the importance of scientists and scientific research in January 1955 was made explicit in Zhou Enlai's influential address at the Conference on the Issue of Intellectuals a year later. Zhou enumerated the enormous progress China had made in science and education. However, at the same time he stated starkly that "overall the state of our science and technology is still quite backward." Specifically he pointed to China's backwardness in "technological sciences," which he believed was linked to "a weakness in theoretical scientific foundations." "And it's precisely

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in the area of scientific research that we have invested the least," said Zhou ([1956] 1984: 166).[14]

Without mentioning the Chinese nuclear weapons program then secretly underway, Zhou invoked atomic energy as the "highest peak of new developments in scientific and technological development" and declared that "we must catch up with the advanced scientific state of the art" (Zhou [1956] 1984: 181–182). In order to do so, some corrective actions in regard to scientists and science policy must be undertaken, Zhou ([1956] 1984: 183–184) argued:

> In the past few years, various kinds of work have just begun, so it is inevitable and understandable that we have needed to invest more in technological work and paid less attention to long-term needs and theoretical work. But now, if we still do not increase our attention to long-term needs and theoretical work, we would be making a big mistake. Without certain theoretical scientific research as the foundation, it is impossible to make essential technological progress or innovations. But the effects of theoretical work are usually indirect, not easily visible immediately. It is precisely because of this that many comrades tend to be short-sighted at the present, unwilling to spend necessary efforts on scientific research and often requiring scientists to solve relatively simple problems of technical applications and production operations.

Here, in contrast to the rhetoric of "revolutionary utilitarianism" from the 1952 Thought Remolding campaign, including Chen Boda's speech, Zhou's address articulated what might be called the Chinese linear model of "theoretical research," which was very close to "basic research" in the West, leading to technological progress. This formulation was familiar to the Western-trained Chinese scientific elite, and, even further, Zhou pushed for more emphasis on long-term scientific research in comparison to short-term "technological work." Admittedly, "science for science's sake" was still not acceptable, but the balance was clearly moved upstream in the linear model in Zhou's address: "Of course theory should never be detached from practice; we are opposed to any 'theoretical studies' detached from practice. But at the present the main tendency has been the neglect of theoretical research. This situation exists not only in the natural sciences, but also in the social sciences" (Zhou [1956] 1984: 184).

To Qian and other leaders of the CAS, what Zhou announced next came as an opportunity to integrate this enlightened view into actual policy. He called for the formulation of a comprehensive long-term national science and technology plan for 1956–1967 as a key step in what he called "A March on Modern Science":

> In making this long-term plan, it is imperative to introduce the most advanced achievements of modern science into our scientific, national defense, produc-

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> tion, and educational sectors as soon as possible and based on feasibility and needs, and to fill in the gaps in fields where our science is most lacking but where the demands from our national construction are most urgent, so that our scientific and technological levels in these fields would, in twelve years' time, approach those of the Soviet Union and other world powers. (Zhou [1956] 1984: 184)

Among other measures, Zhou specifically called for "greatly strengthening the Chinese Academy of Sciences, making it into the locomotive that would lead the nation in lifting scientific standards and training new talents." Likewise he called for rapid improvement of scientific research and training in universities and the production ministries, including defense, under the new science plan and in coordination with the CAS (Zhou [1956] 1984: 185).

## "Exploratory Research" and the Bomb Project

With this auspicious beginning, the year 1956, which witnessed the making of the science plan, the launch of the March on Science, and Mao's issuing of the liberal double-hundred policy ("let one hundred schools contend and let one hundred flowers bloom"), turned into a golden year of relative political relaxation and professional enhancement for scientists and other intellectuals. Emboldened, some scientific leaders now sought to make the most out of Zhou Enlai's declaration on the importance of basic theoretical science.

Qian Sanqiang, for example, used his election (actually, appointment) as a delegate to the Communist Party's Eighth National Congress in September 1956 to elaborate on the importance of basic research (he called it "exploratory research"), especially for long-term technological innovation, when it was his turn to speak. Perhaps the leading nuclear physicists in China at the time, Qian and He Zehui, a husband and wife team, had worked under Frédéric and Irène Joliot-Curie in France in the 1930s and 1940s and made the discovery that the uranium nucleus could fission into more than two fragments. Recounting the history from scientific studies of radioactivity and the nucleus to nuclear weapons, Qian commented at the party congress that

> such exploratory researches were aimed at understanding the internal laws of the structure of matter. There were not many connections with the production practices at the time, neither did they foretell directly such important discoveries [as atomic energy]. But after forty years' exploration following the intrinsic laws of scientific development, the principles governing the applications of atomic energy were finally discovered and the speed of development has quickened greatly thereafter. (Ge 2013: 269)

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Here Qian was speaking of the importance of basic or theoretical research not only as a rhetorical exercise, but also as a matter of actual policy debate within his own Institute of Physics. In the early 1950s, Qian had established a "Theoretical Group" in the institute (called the Institute of Modern Physics then) and "it was not without controversy" amid the Thought Remolding campaign, as he later recalled. Qian later felt vindicated when members of the theoretical group all became leaders of the atomic and hydrogen bomb projects.[15]

But, perhaps reflecting in part his transnational experiences, broad responsibilities at the Academy, and intimate involvement in the concurrent making of the twelve-year science and technology plan, Qian was aware of the complex and reciprocal relationships between science and technology beyond the common perception that the former would lead to the latter (as articulated, for example, by Zhou Enlai above). As he explained at the party congress,

> It should be pointed out here that exploratory research in basic science would be impossible without modern industrial and technological conditions. . . . The example [of particle accelerators] demonstrates that under conditions provided by modern industry, exploratory scientific research has produced new technologies and helped advance industry and technology in general. Science in turn has taken advantage of these new technologies to gain new conditions for rapid development, preparing the stage well for the next phase of exploratory research. Such is the relationship between science and production and between theory and practice. (Ge 2013: 269–270)

Qian further complicated the conventional view with his proposal for "theoretical research in technological sciences," which addressed common problems in industrial technologies and which "could also lead to leap-like developments" in the latter (Ge 2013: 270). Thus to Qian, there were four elements or stages in scientific and technological development: "basic scientific research [jichu kexue yanjiu], technological scientific research [jishu kexue yanjiu], engineering design [gongcheng sheji], and industrial production [gongye shengchan]" (Ge 2013: 270).

For Qian, the key point of this discourse on science and technology was the idea that if China was to move from dependence on Soviet assistance to "establishing our own independent science and technology," it must pay more attention to the first three elements. Otherwise, Qian warned, "we would forever lag behind industrially advanced countries and manufacture by copying, producing no new designs, much less radical progress or innovations." Obviously, he had in mind the vice minister's comments about asking the Soviets for semiconductor technology. Quoting Zhou's address, Qian argued further that while it was possible to import technologies within a short period of time,

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it was impossible to rush scientific research. Therefore, there was an urgent need to strengthen the first two elements in his formulation: exploratory research in the basic sciences (*jichu kexue zhong de tansuoxing de yanjiu gongzuo*) and theoretical research in the technological sciences (*jishu kexue zhong de lilun gongzuo*) (Ge 2013: 270–271).

How to strike the right balance between basic research and practical applications, as advocated by Qian, became a critical point of contention in the making of the twelve-year science plan, involving tension not only between the scientists and administrators but also between the scientists and the Soviet advisors. On 7 March 1956, at an executive session discussing the making of the plan, several leading Chinese physicists, mostly U.S.-trained, "expressed their dissatisfaction" that the framework of fifty projects to be included in the plan, as proposed by Boris R. Lazarenko, chief Soviet advisor to the president of the Academy, "did not mention basic scientific development," according to Zhu Kezhen's diary.[16] Two weeks later, Lazarenko struck back, expressing his own unhappiness with the status of the plan, complaining that it "did not grasp the key problems but only made arrangements for the various disciplines."[17]

Meanwhile, the administrators had come up with a slogan that tended to side with Lazarenko—"tasks leading disciplines" (*renwu dai xueke*)—as the organizing principle of plan-making and came up with fifty-five such applied tasks. They also settled on the slogan "select important developments and catch up from behind" (*zhongdian fazhan, yingtou ganshang*), overruling the alternative, preferred by many of the scientists, of "select important developments, plan comprehensively, lay a solid foundation, and catch up from behind" (*zhongdian fazhan, quanmian jihua, tashi jichu, yingtou ganshang*) as too diffused (Nie 1986: 779; see also Z. Wang 2015).

However, when the administrators briefed Zhou Enlai on "tasks leading disciplines" and the fifty-five actual tasks, Zhou raised objections. According to Wu Heng, a geologist-administrator present at the briefing, "When we got to the slogan of 'tasks leading disciplines,' Premier Zhou paused for a moment, and then asked, what happened to those disciplines that could not be led by tasks? Shouldn't there be a plan for disciplines aiming at scientific development? This was a must for any long-term plan" (Wu 1992: 164).[18] It is not clear whether Zhou's intervention was connected with the physicists' own objections described above, but it was evidently consistent with his earlier address on intellectuals and would become legendary among scientists as an indication of Zhou's concern for basic research. In the end, the compromise provided that the plan would remain dominated by fifty-five practically-oriented tasks, but a fifty-sixth task on "Investigations into Some Basic Theoretical Problems in the Modern Natural Sciences" would be added, which

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listed about a dozen specific topics in physics, chemistry, biology, astronomy, mechanics, and mathematics. Literally, "theory" was now "attached to practice." Later, a fifty-seventh, on scientific and technological information, was also added to the plan.[19] The CAS made its own separate long-term plan for all major scientific fields that was more comprehensive than task no. 56 above (Wu 1992: 164).

The debate over theory and practice or basic and applied research would, of course, not end with the balance reached in the 1956 science and technology plan. During Mao's 1957 Anti-Rightist campaign against intellectuals, including scientists, who had voiced criticism of the party-state (initially at Mao's own urging), advocacy for theoretical, basic research once again, as in Thought Remolding, became a political liability. On 5 July 1957, in a highly-publicized speech drafted by the party's Propaganda Department, CAS president Guo Moruo denounced "some scientists" for believing that "scientific work can be conducted without planning or leadership, [or that] scientific research does not have to be integrated into various forms of national construction work; they want to have absolute personal freedom in scientific research, science for science's sake" (Guo 1957). As a result of the campaign, about 550 thousand intellectual "rightists" nationwide were purged, often with tragic consequences for themselves and their families. Under its party secretary Zhang Jinfu, the CAS leadership made a bold but ultimately successful appeal to Mao on the utilitarian basis of talent scarcity and protected some of its senior scientists, but that still left 167 people in the Academy's institutions in Beijing who were persecuted as rightists, including eleven senior researchers at the levels of professor or associate professor. With less protection, scientists in universities fared even worse (Qian and Gu 1994, vol. 1: 86–89).

Mao's next campaign, the Great Leap Forward movement in 1958–1962, would continue to tighten control and pressure toward the practical, and even ideological correctness in scientific research. Basic research was often denounced as theory detached from practice or even worse—"being expert without being red." Among the various component campaigns during the Leap, there was one called "Planting Red Flags and Pulling White Flags," whose main targets were scientists and other intellectuals accused of being "white experts" by pursuing theoretical research detached from practice or without the guidance of Mao and the party.[20] Once again, the Academy's leadership tried its best to protect basic research. With the right combination of factors, including an appeal to national prestige in the Sputnik era, to socialist coordination, and to writings of Friedrich Engels, a co-founder of Marxism, such efforts actually led to the 1958 launch of the basic research project to

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artificially synthesize bovine insulin by the Academy and several universities, which remarkably continued into the Cultural Revolution (Xiong and Wang 2005: 14–21). The Soviet decision in 1959 to withdraw its nuclear assistance to China also played a critical role in forcing the Chinese party-state to rely on Chinese scientists and engineers for building the atomic bomb, which in turn improved their professional and political status.[21]

Ultimately, it was the devastating Leap-induced famine that caused the pendulum to swing back to a point of relative political relaxation and a more moderate science policy by the early 1960s. It was in this environment of "adjustment" that Marshal Nie Rongzhen, who was in direct charge of national science and technology policy on both the civilian and defense sides, supervised the making of the famous Fourteen Points liberal science policy directive in 1961. Point no. 3, "Correctly Implement the Principle of Theory Attached to Practice," for example, stipulated the following:

> Socialist construction has many different needs, and the way for theory to be attached to practice is very broad. A comprehensive and long-term point of view should be taken in this regard, and a narrow or short-sighted understanding should be avoided. Besides pushing hard for those kinds of research work that directly serves current economic and defense construction, it must be arranged to carry out those kinds of research work that will only be applicable indirectly or in the long-term. Research topics can be raised from production and construction; they can also be raised from the development of various disciplines. There should not be biased preferences in this regard. Some exploratory topics and some disciplinary branches, even though without foreseeable applied values at the present, should not be neglected if they could help humans gain deeper understanding of the objective world—there must be people to carry out such work.[22]

It was concern over the damaging effects of the Leap Forward movement on what was called "advanced technologies"—code words for the nuclear and missile programs—that led Nie to formulate, and the party-state leadership to approve, the policy statement. It was also in this context that the CAS explicitly articulated its positioning, which had started with the 1956 science plan, as the provider of basic, applied basic, and applied research, especially to the nuclear and missile programs as mentioned at the beginning of this chapter. And finally, the liberal political implications of the Fourteen Points were spelled out in February 1962 during what became known as the "Guangzhou Conference" on science and technology when Premier Zhou Enlai and Vice Premier Chen Yi successively pronounced that scientists and other intellectuals were no longer bourgeois but part of the politically reliable laboring and working class (see illustration 8.1).[23]

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**Figure 8.1.** *Left to right:* Chinese Premier Zhou Enlai with Chinese physicists Qian Sanqiang and Zhou Peiyuan in February 1962 in Guangzhou at a reception during the famous Guangzhou Conference on National Scientific and Technological Work. Source: Qian Sanqiang, *Qian Sanqiang wenxuan* [Selected papers of Qian Sanqiang] (Hangzhou: Zhejiang Science and Technology Press, 1994), xxi.

## Conclusion

The successful Chinese atomic bomb test on 16 October 1964 marked a milestone in the history of modern China, whose political, social, and cultural significance still awaits fuller and deeper analysis. For Chinese scientists and science policy participants who were still engaged in ideological battles for the importance of basic scientific research, including those in the CAS, it came as a welcome vindication of their past advocacy and as justification for future requests of support. In a 1965 report to Mao and the party leadership on science and technology policy for the next five years, Nie cited Mao's recently revived Leap-era call for "catching up and surpassing advanced world standards" to justify increased investment in "basic research" (*jichu yanjiu*)—the term that now appeared for the first time in Nie's papers. Without mentioning the bomb directly, Nie claimed that "now the phase of imitation is gradually passing, which requires that we need to create, to carry out independent research." "Therefore," he continued, "the key at the present is to greatly strengthen basic research work."[24]

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Alas, less than two years after the Academy articulated its strategic positions on the upper stream of the linear model and less than one year after Nie made his case for basic research, Mao, whose position at home and internationally was now buoyed by the success of the atomic bomb, launched the Great Proletariat Cultural Revolution in mid-1966, resetting the delicate balance between theory and practice, or basic and applied research, that had been reached during the making of the bomb. Once again, many scientists and intellectuals were attacked as "reactionary bourgeois," as in the Remolding or Anti-Rightist days, and suffered brutal "labor re-education" or worse. "Theory detached from practice" once again became a label carrying deadly consequences for scientists. Qian Sanqiang, who had always been distrusted by the party leaders in his institute and also in the second ministry of machinery-building, where he served as a vice minister during the bomb project, spent years in labor camps ("cadre schools"); the CAS party secretary Zhang Jinfu was persecuted; and Nie Rongzhen was attacked for putting too much emphasis on modernization and not enough on revolutionization.[25] Only the death of Mao in 1976 brought an end to the Cultural Revolution, and the beginning of the reform era that reversed the radical politicization of science. It also introduced a new dynamics of science policy that recognized the value of basic research but still gave more weight to applications and development.

What this survey of the Chinese debates over basic and applied research, or theory and practice, in the early Mao years has demonstrated is that such issues were, compared to developments in the West, but also compared to other socialist countries, political and ideological in a much more radical sense. At the core of the problem lay the political distrust by the Communist party-state, especially Mao himself, of the mostly Western-trained Chinese scientific elite. The issue was thus a matter of party control of science and scientists, of policing the possibility of any individual freedom outside of the authority of the party-state in this period. Thus the fortunes of basic research and its advocates rose and fell with a rhythm that resonated with the general tenor of Chinese politics under Mao, largely confirming Suttmeier's "regularization/mobilization" model. Only secondarily was the issue of basic research a matter of effective science policy that was amenable to reasoned debates or discussion during times of relative political liberalization. Nevertheless, in this limited sense, it should be noted that the bomb did intervene and provide increased maneuverability by those who carried out basic, applied basic, or applied research. "Science for science's sake" was never acceptable under Mao, even during periods of moderation, but the relative continuity of the nuclear weapons projects and apparent survival of the artificial insulin project during the Cultural Revolution indicate that there were both change

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and continuity in the debates over basic and applied research during the Mao years that deserve further exploration.

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## Notes

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1. "Zhongguo kexueyuan gongzuo tiaoli (ziran kexue bufen, chugao)" [Chinese Academy of Science Work Regulations (for natural science, draft)], drafted in late 1963 and issued within the Academy on 4 April 1964 (here as quoted in Lou and Zhang 2008: 273).
2. On the change in American rhetoric, see, e.g., Hollinger 1990. On the historical claim that Chinese scientists did bring pure science ideals from the United States to China, see Buck 1980.
3. See Zhu Kezhen diary for 1951–1952 (Zhu 2004–2013, vol. 12). Zhu Kezhen's diaries related to the Thought Remolding have been collected and edited for easy access by Wang Yangzong (Zhu 2013).
4. Zhu Kezhen diary entry for 8 November 1951 (Zhu 2004–2013, vol. 12: 467).
5. Zhu Kezhen diary entry for 10 January 1952 (Zhu 2004–2013, vol. 12: 537).
6. Zhu Kezhen diary entry for 17 February 1952 (Zhu 2004–2013, vol. 12: 561–562).
7. Zhu Kezhen diary entries for 26 February and 10 April 1952 (Zhu 2004–2013, vol. 12: 567, 596).
8. Zhu Kezhen diary entry for 21 May 1952 (Zhu 2004–2013, vol. 12: 622).
9. Le, among other misdeeds, had driven the U.S.-trained prominent geneticist Li Jingjun (Ching Chun Li) to flee China when Le was the dean of the Beijing Agricultural University in 1950 (see Schneider 2003: 123).
10. Zhu Kezhen diary entry for 31 May 1952 (Zhu 2004–2013, vol. 12: 628).
11. "Wei jianchi shengwu kexue de miqiulin fangxiang er nuli" [Struggle in defending the Michurinian direction in the biological sciences], *Renmin ribao,* 29 June 1952, 3.

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12. On Chen's speech, see also Suttmeier 1974: 36–42.
13. Qian Sanqiang. Talk at the meeting of the Committee on the Collection of Artifacts and Materials Related to the History of the Chinese Academy of Sciences (Qian was chairman), 27 December 1990, as reprinted in Ge Nengquan, *Qian Sanqiang nianpu changbian* [A detailed chronicle of Qian Sanqiang] (Beijing: Science Press, 2013), 729–735, quote 733, see also 220.
14. On Zhou's speech, also see Z. Wang 2015.
15. "Shiwunian de huigu" [A look back at the fifteen years], an interview with Qian Sanqiang by Wang Gantang in May 1989, printed in Ge Nengquan, *Qian Sanqiang nianpu changbian* [A detailed chronicle of Qian Sanqiang] (Beijing: Science Press, 2013), 701–707, quote 704. On the founding of the theoretical group in 1951, see 187; on opposition from a party member in the institute at the height of the Remolding campaign in October 1952, see 197.
16. Zhu Kezhen diary entry for 7 March 1956 (Zhu 2004–2013, vol. 14: 301).
17. Zhu Kezhen diary entry for 23 March 1956 (Zhu 2004–2013, vol. 14: 308).
18. Wu Heng did not give the exact date for this briefing, but in Li and Ma (1997: 556) there is an entry for 12 March 1956, when Zhou met with the science plan organizers.
19. "Yijiuwuliu-yijiuliuqi nian kexue fazhan yuanjing guihua gangyao (xiuzheng caoan)" [The revised draft of the outline of the long-term plan for the development of science and technology for 1956–1967], in Zhonggong zhongyang wenxian yanjiushi (1992–1995), vol. 9: 436–540, on 503.
20. See, for example, a remarkable collection of self-criticisms by scientists in Shanghai in this campaign, published by Shanghai People's Press in 1958: *Cha hongqi ba baiqi: Shanghai bufen gaoji zhishi fenzi de sixiang jiancha* [Planting red flags, pulling white flags: Ideological confessions by some senior intellectuals in Shanghai].
21. See, for example, an interview with Qian Sanqiang by Wang Gantang in May 1989, printed in Ge Nengquan, *Qian Sanqiang nianpu changbian* [A detailed chronicle of Qian Sanqiang] (Beijing: Science Press, 2013), 701–707, on 703.
22. "Guojia kexue jishu weiyuanhui dangzu, zhongguo kexueyuan dangzu guanyu ziran kexue yanjiu jigou dangqian gongzuo de shisitiao yijian (caoan)" [Fourteen points of suggestions on the current work of natural science research institutions by the State Science and Technology Commission and Chinese Academy of Sciences party groups (draft)], June 1961, in Zhonggong zhongyang wenxian yanjiushi (1992–1995), vol. 14: 546–570, on 550–551.
23. On the Guangzhou conference, see Qian and Gu 1994, vol. 1, 109–111.
24. Nie Rongzhen, "Youguan disange wunian jihua jishu zhengce he shixian ganchao mubiao de ruogan jianyi" [Some suggestions related to technology policy during the third five-year plan and to achieving the objectives of catching up and surpassing], a report to Mao Zedong and the party Central Committee, 23 August 1965, in *Nie Rongzhen keji wenxuan* [Selected papers on science and technology by Nie Rongzhen] (Beijing: National Defense Industry Press, 1999), 580–588, on 585–586.

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25. On Qian shortly before and during the Cultural Revolution, see Ge 2006: 318–335; on Zhang Jinfu, see Lu 2009. The book was based on oral history interviews with Zhang by Liu Zhenkun. On Nie during the Cultural Revolution, see Wang 2010.

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# Part III
# Outlook

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Chapter 9

# The Language of Science Policy in the Twenty-First Century

## What Comes after Basic and Applied Research?

*Tim Flink and David Kaldewey*

The turn of this century was a peculiar period. It was accompanied by intense retrospections as well as imaginations and millennium proclamations. A further coalescing world society has tried to find answers to its own immediacies, and seems to be in need of a new language that would both frame exigent problems and bring together actors that are willing to tackle them. In this respect, old concepts live on vis-à-vis new ones, and they may peacefully coexist in some cases while causing interferences in others. Such reflections are not restricted to the sphere of high politics, the world economy, and global tensions and conflicts. It is also in the prosaic sphere of science policy that various actors try to make sense of what's happening in the present age and what has changed. Against this background, the questions we follow in this chapter are whether twentieth-century science policy concepts—such as "basic research" and "applied research"—have been overwritten by new ones, or whether old and new concepts can coexist. More specifically, we ask which new concepts bear potential to structure twenty-first-century expectations about the relationship of science and society.

Looking back at the history of basic and applied research, we have to keep in mind that this distinction never stood on its own feet, but needed symbolic and institutional backing. First, the basic/applied distinction was

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embedded in those master narratives that have determined science policy discourses in the second half of the twentieth century. For example, reviewing the post–World War II decades, science policy scholars in the 1990s came to the common understanding that science and society had—more or less explicitly—established a "social contract" (Guston and Keniston 1994; Sarewitz 1996; Gibbons 1999). By this quasi-contractual relationship, scientists were granted high degrees of autonomy as long as society was convinced that scientific knowledge would be translatable into technological innovations and ultimately contribute to economic welfare.

Similarly, at the organizational level, models and narratives of "technology transfer" seized prominence in the postwar period. While investments into warfare-related R&D had proved useful, the continuing and even intensifying federal R&D expenditure after the war needed extra justification, which resorted to concepts of technology transfer for civilian use. On university campuses and in research laboratories, calls for technology transfer were thus expounded by various terms, such as "fall out," "spillover," and, most notably, "spin offs" (Mowery and Sampat 2001; Mowery et al. 2004; Shane 2004: 45–48). For the scientists' autonomy, these early concepts of technology transfer were functional: they did not question the relevance of scientific knowledge production as such, but argued for auxiliaries that would make scientific knowledge transferable. In this respect, they helped sustain a "protected space" for scientists and universities (Rip 2011).

And yet, the most successful master narrative was the "linear model of innovation." Introduced and disseminated in the 1950s and 1960s mainly by economists (Godin 2006, 2017; Lax 2015), this model built on the idea that innovation could be rationally scheduled via consecutive sequences of action: basic research would be followed by applied research, the latter by development, and this last step would ultimately lead to technological innovations and profitable market products. Although the linear model was never codified or generally accepted as valid in terms of its explanatory power (Edgerton 2004), it was tremendously influential insofar as it provided a simple way of conceiving and communicating the utility of science—not least the utility of basic research (Kaldewey 2013: 371–383). In the scholarly communities, however, the linear model was increasingly challenged from the 1980s onward (Stokes 1997; Pielke and Byerly 1998; Fagerberg 2005). Nathan Rosenberg (1991: 335) put this critique in a nutshell: "Everyone knows that the linear model of innovation is dead." In the following years, such declarations became commonplace, and, as some scholars observed, turned into a cheap and polemic ritual (Freeman 1996: 27; Balconi, Brusoni, and Orsenigo 2010; Mirowski 2011: 47). The consensus in the 1990s was that the linear model of innovation could never fully grasp the complexity of innovation processes.

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The rise and decline of the linear model as a master narrative can be illustrated by quantitative semantic analysis (Chumtong and Kaldewey 2017). Figure 9.1 traces the frequency of the key terms "basic research," "applied research," and "technological innovation" in the Google Ngram Viewer corpus. The figure makes visible how the trajectories of the three terms are

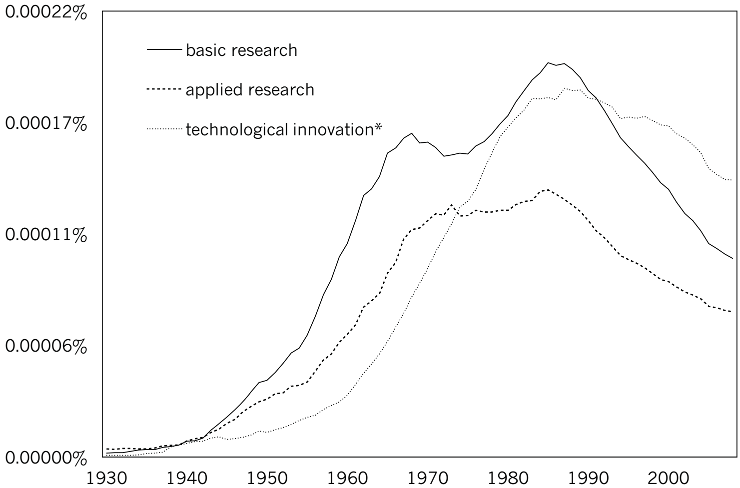


**Figure 9.1.** The linear model as seen through the Google Ngram Viewer, 1930–2008 (relative frequencies of the respective terms, English corpus, case-insensitive; data retrieved 8 November 2016; smoothing=3)

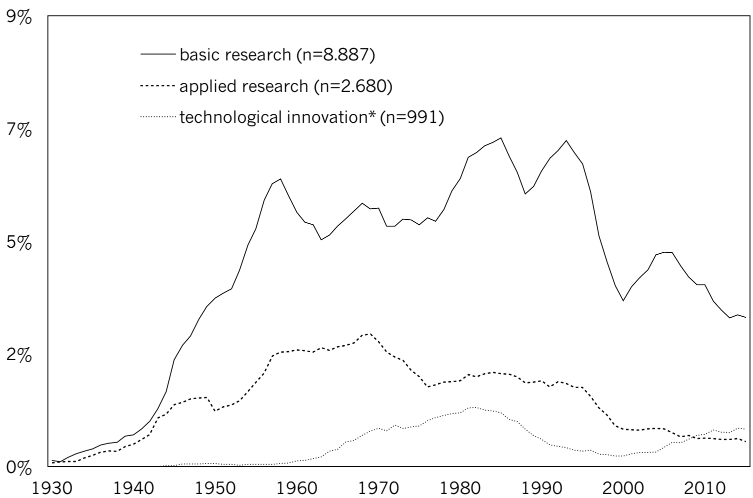


**Figure 9.2.** The linear model as seen through the *Science* archive, 1930–2015 (relative numbers of articles that contain the respective terms; data retrieved 8 November 2016; smoothing=3)

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interrelated, how they all disseminate into everyday language in the 1950s and 1960s, and then decline in the 1990s. This data impressively corroborates the "death hypothesis" put forward by Rosenberg and others. The Google Ngram Viewer corpus, however, does not represent scientific communication in the narrow sense and cannot be used to infer anything about how relevant the respective concepts have been in more specific scientific and science policy contexts. Figure 9.2, therefore, shows how many articles in the archives of the journal *Science* contain the respective terms, relative to the total number of articles in a given year. Since *Science* is dedicated to academic scientists as its primary audience, the concept of basic research is—unsurprisingly—more prominently represented in this context than the concept of applied research. Nevertheless, both terms follow a pattern quite similar to that displayed in the Google Ngram Viewer: they had their heyday between the 1950s and the 1980s and became less commonly used at the end of the century.

Based on the data in figures 9.1 and 9.2 and the critiques mentioned above, one could conclude that the linear model did not make it into the twenty-first century. Further quantitative inquiries with a specific focus on the more recent past, however, challenge this interpretation. Figure 9.3 contains data extracted from more than thirty-five million publications indexed in the *Web of Science* core collection[1] and indicates how many of these contain the respective terms in title, abstract, or keywords. Surprisingly, in this corpus, the use of all three terms has increased constantly between 1991 and 2015. If one

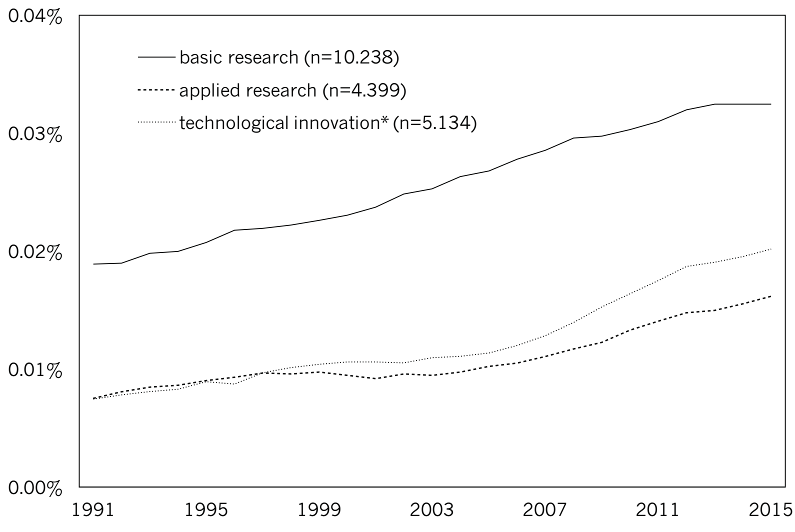


**Figure 9.3.** The linear model as seen through the *Web of Science* core collection, 1991–2015 (relative numbers of publications that contain the respective terms in title, abstract, or keywords; data retrieved 11 August 2016; smoothing=3)

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compares the three figures, the overall picture becomes blurry. On the one hand, figures 9.1 and 9.2 indicate that the linear model of innovation lost its status as a master narrative; on the other hand, figure 9.3 indicates that the key terms are firmly established in "normal" scientific communication.

Why then did the linear model, and the concepts nested within it, persist despite the scholarly critique? One reason is that the linear model of innovation and the idea of a social contract of science were, for several decades, coupled and integrated in the same policy rationale: that the state—amid decreasing competitive market advantages for companies—needs to provide public goods. Basic scientific knowledge was assumed to be an important public good that everyone could take advantage of (Schauz 2014: 299). This rationale has sustained its importance despite the fact that economists and other scholars disagree.[2] Another reason for the persistence of the linear model is, simply, that so far there seems to be no alternative master narrative available. Ulrich Wengenroth (2000: 28) thus concisely summarized the debate at the end of the century by admitting that the linear model "is now dead," while at the same time stating that "it has not yet been successfully replaced by a new orthodoxy." Roger Pielke (2012: 341) recently put forward a similar argument asking if and to what extent "a new political consensus" may emerge that can "replace 'basic research' as a central, organizing symbol." In our view, the same question should be asked with respect to all concepts nested in the linear model ("basic research," "applied research," "research and development," "technological innovation").

Against this background, the questions of whether basic and applied research denote distinct activities within the logic of the linear model, and whether the model is more or less appropriate in describing or planning reality, do not seem instructive for us. Instead, we contend that concepts are powerful not necessarily due to their analytical accuracy, but rather due to their *symbolic* function in science policymaking. Because concepts—old and new ones—are embedded in a narrative structure, they represent more than strategic language games: they open avenues to unfold alternative identities, for individual actors, organizations, and science in general.[3]

In view of these considerations, we propose to take a closer look at those science policy concepts that have gained prominence in the last two decades, and to reflect on how these new concepts relate to the old ones. In contrast to those science policy scholars who aim to show how one conception of the science/society relationship follows the other ("mode 1" versus "mode 2"), we assume that new concepts are related to the old narrative in a more complex way. We focus on the reuptake of two concepts in particular, both of which have become particularly prominent in the transnational research policy of the European Union (EU), while at the same time being rooted in ideas

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originally stemming from the United States of America.[4] First, we reconstruct the history of the "frontier"-metaphor in science policy contexts, particularly the European Research Council's strategy of reframing "basic research" as "frontier research." Second, we trace the idea of "grand challenges" in science policy discourses in the United States and Europe, as it has, though mostly implicitly, come to replace older notions of "applied research." Figures 9.4 and 9.5 illustrate that and how these concepts have gained traction in scientific communication—and how the trajectories (though not their absolute frequency) outperform the old concepts of basic and applied research (as seen in figure 9.3). Such quantitative analysis, however, is of heuristic value only, and must be handled with care whenever the goal is to explain actual historical transformations. Therefore, the following analysis of contemporary discourses employs a more qualitatively oriented methodological strategy, which, similarly to the other chapters of this volume, builds on insights from historical semantics.

By selecting these two cases, we do not propose that the "frontier" concept finally replaces basic research, or that the concept of "grand challenges" supplants older notions of applied research. Many more concepts would be worth further exploration—for example, "excellence," "interdisciplinarity," and "breakthrough research" as circumscriptions of basic research, or "impact," "transdisciplinarity," and "translational research" as new formulations of what used to be called applied research. Studying the whole semantic field

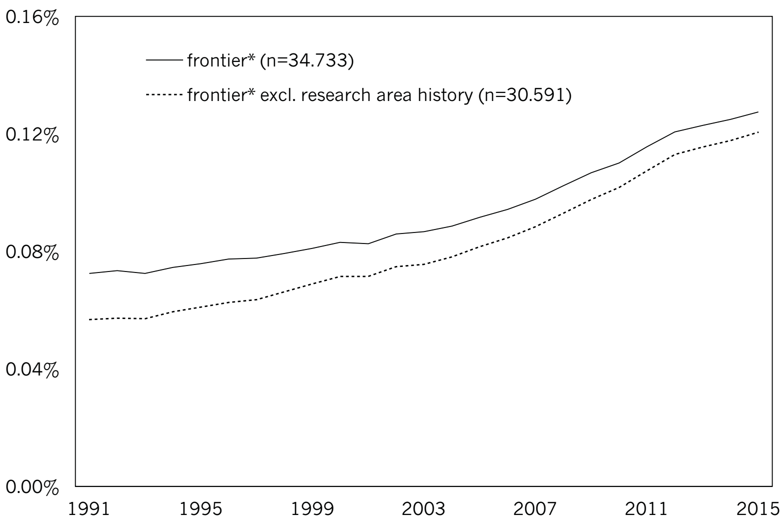


**Figure 9.4.** The frontier metaphor as seen through the *Web of Science* core collection, 1991–2015 (relative numbers of publications that contain the term in title, abstract, or keywords; data retrieved 13 July 2017; smoothing=3)

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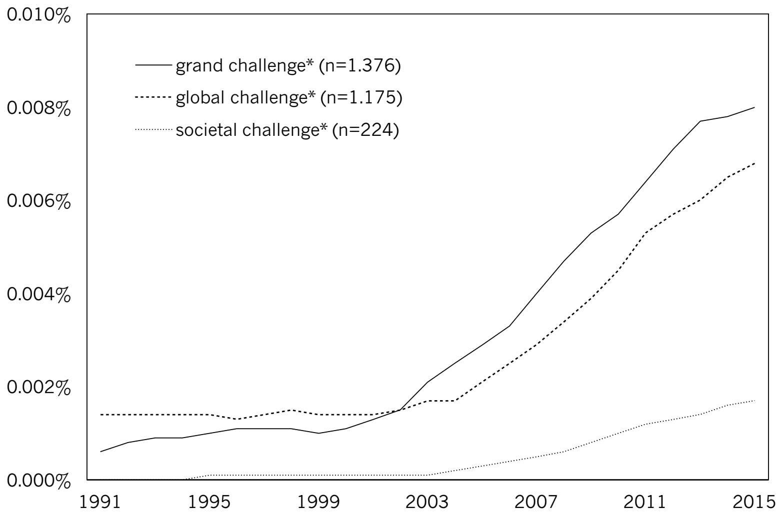


**Figure 9.5.** The grand challenges discourse as seen through the *Web of Science* core collection, 1991–2015 (relative numbers of publications that contain the respective terms in title, abstract, or keywords; data retrieved 14 February 2017; smoothing=3)

that builds on and transforms the twentieth-century basic/applied distinction would go beyond the scope of this chapter. Furthermore, in view of the linear model, we do not inquire into what happened to the final link of the chain, "technological innovation." Here, again, a wide field of new conceptual variations has developed in the last decades—concepts such as "social innovation," "open innovation," "sustainable innovation," or, most recently, "responsible innovation."[5] Nevertheless, the two case studies presented in this chapter point out that conceptual history and metaphorical analysis are helpful not only in understanding the history of old concepts, but also in reflecting on quite recent developments and transformations in the language of science policy.

## Frontier Research

In April 2005, the European Research Council (ERC) was established by the European Commission as part of a legal proposal for the Seventh Framework Programme for Research and Technological Development (FP7) and finally enacted in December 2006 by the European Parliament and the Council of the EU. The new thing about the ERC was that for the first time in the history of the European Communities (EC), its central institutions allowed for investigator-driven and strictly peer-reviewed basic research funding. While calls

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for such a funding mechanism are as old as the EC themselves,[6] these initiatives never gained momentum, a reality best reflected in the unfortunate development of the European Science Foundation (ESF) since its establishment in 1974 (Darmon 1997). Part of the reason is that after the first big European crisis in the 1970s, research policy on the supranational EC level had been (re) established in view of one primary goal: the revitalization of European integration via the internal market project (Garrett 1992; Guzzetti 1995; Peterson and Sharp 1998). Among other policy fields, scientific research was chosen, and thus subordinated, to serve the competitiveness of European business enterprises, in particular against U.S. and Japanese firms. Thus, the initial Framework Programmes (FPs)—BRITE, EURAM, ESPRIT—channeled political expectations toward the ideal of economic utility. As a consequence, the Commission's directorate general pursued a strictly utility-oriented research policy (Banchoff 2002). As early as with the Third Framework Programme (1991–1994), however, that path-dependent development no longer matched reality: public research institutions drew close with private entities as regards their extraction of funding from the FPs (Peschke 2001). Thus, there was a mismatch of expectations, which ran toward a dichotomy between the proffered applied research funding (aiming at economic utility) and a self-determination of actors from public research institutions (being sympathetic with basic research).

Against this backdrop, proposals to finance basic research on a pan-European level were prompted in the mid-1990s—for example, from the European Molecular Biology Organization (EMBO), the Royal Society, and from policymakers of smaller European countries (Flink 2016: 105). The Commission was relentlessly criticized for keeping the FPs oriented toward application-oriented, cross-national collaborative R&D, which were not only regarded as bulky and red-taped, but also inadequate to serve the free play of knowledge production.[7] Still, the fact the ERC project was actually going to walk the walk, with the idea of inducing purely scientific peer-review principles of the highest standards and allowing individual researchers of all scientific fields to apply for funding, did cause a sensational bang in and far beyond the limbo of Brussels' office corridors.

The Commission initially ignored, then argued against, the idea of investigator-driven basic research funding (Nature Editorial 1997: 661; Schulz-Forberg and Stråth 2010: 149–150), as the organization was bound legally and politically to other funding rationales (Flink 2016). First, the *principle of subsidiarity* required that actions should be regulated, if possible, on a lower level of European governance (e.g., communal, regional, and national). Second, required to deliver *European-added value,* the Commission needed to itemize its potential actions as specifically beneficial for actors of cross-

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national range and scope. Third, the consortia profiting from the FPs were well rehearsed in lobbying for every euro. Therefore, the ERC initiative and most certainly the Commission were likely to face the strong counter-argument that basic research would not lead to profitable, innovative products serving the European market. From early on, most advocates of the ERC initiative might have strategically anticipated such forthcoming counter-arguments and therefore continued to stress the economic utility of basic research. Yet the term itself was problematic as it conveyed notions of pure curiosity and uselessness. Against this background, the European Commission (2005: 3, 6, 36, passim) in her legal proposal of FP7 replaced "basic research" with a new term, "frontier research." This swift semantic shift—which was legitimized by a body of experts who had delivered a report titled *Frontier Research: The European Challenge* (Harris et al. 2005) only two months before the legal proposal—is remarkable not only because "basic research" was obliterated, but because the experts and the Commission chose the specific alternative of "frontier research." Thus, this kind of conceptual politics is an intriguing case to study in order to answer the questions put forward in this chapter: (how) are old concepts overwritten by new ones, and is it possible that old and new concepts coexist?

"Frontier" is not a common term in Europe, and was rarely used in the continent's national science policy discourses during the twentieth century. Rather, it draws on a longstanding historical legacy of the United States, where it finds use both as a popular, conventional daily-life metaphor and as a term employed in science policy discourse.[8] The concept became most famous in the nineteenth century's "Wild West" era and portrays a movement of exploring and exploiting the Americas from east to west and further south (Ceccarelli 2013). Thereby, the frontier does not primarily denote a border but rather a transitional contact zone for adventurous "frontiersmen" penetrating the unknown territory. While moving the frontier as a literal process terminated with the pioneers reaching the natural borders of the Americas, the late nineteenth century turned the frontier into a powerful metaphor that was no longer bound to an actual process of exploring new lands.

In 1893, historian Frederick Jackson Turner presented the first version of his famous treatise on *The Significance of the Frontier in American History* to the American Historical Association in Chicago (Turner 1893; see also Coleman 1966; Rushing 1986).[9] Though he revised his presentation numerous times until the 1920s, his basic idea remained stable: extending the actual frontier with all its implied boldness and risk had forged a "special American character . . . marked by fierce individualism, pragmatism, and egalitarianism" (Cronon 1987: 157). Amid the cessation of the literal process of expansion, the frontier would have been perpetuated in a spiritual manner

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that vindicated Americans for being "savagely exploitative, and firm in [their] conviction that opportunity was boundless," no matter in what field they were active (O'Donnell 2000: 83; see also Ceccarelli 2013: 35).

In the early twentieth century, historians scathingly criticized Turner for his post-Darwinian truism of an existing American exceptionalism.[10] It is noteworthy that, despite the academic origin and immediate contestation, Turner's metaphorical concept fell on fertile ground in the general public and became a highly praised portrait of an American "pragmatism." Turner was fascinated that the American settlers, most of them European immigrants, "were separated from their past and forced to assume a new physical and spiritual appearance" (Coleman 1966: 36). As an implication, the miraculous frontier could not allow the individual or society to be limited by tradition-based intellectual—that is, European pondering. In other words, the frontier concept can be viewed as a mundane and aggressive version of liberalist thinking. Accordingly, frontier-inspired liberalism attached little value to governmental oversight, alleging that it would hamper both the individual in thriving on its freedom, and society in forming a collective identity vested with the values of the pioneering frontiersman (Turner 1920: 271–272). Still a breach was left to be filled: as not all men were pioneers during the actual process of expanding the frontier, so could not everyone in society become a frontiersman in the metaphorical sense. Given that problem, Turner (1920: 284) was quite explicit in choosing "university men" as the new pioneers: "Scientific experiment and construction must be applied to all of nature's forces in our complex modern society. The test tube and the microscope are needed rather than the ax and rifle in this new ideal of conquest."[11]

Turner's translation of the literal frontier into a metaphor describing the modern research enterprise has been extremely influential for decades. In 1922, later U.S. President Herbert Hoover (1922: 64) published his book on *American Individualism* and conjectured that "the days of the pioneer are not over . . . . The great continent of science is as yet explored only on its borders, and it is only the pioneer who will penetrate the frontier in the quest for new worlds to conquer."[12] After 1945, following Vannevar Bush's report *Science—The Endless Frontier,*[13] such references became convention in science policy-making whenever new programs were to be justified. The frontier concept was prominently employed, for example, in John F. Kennedy's (1960) advocacy of the Apollo program: "Beyond that frontier are uncharted areas of science and space." Another two decades later, Jimmy Carter (1979) stood up for his plans to spend federal money on R&D amid decreasing tax revenues: "We are pushing back the frontiers in basic research for energy, defense and other critical national needs." As for its semantic dissemination, the frontier had already been incorporated into dictionaries in the 1950s, which

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defined science as a way to explore the unknown territories of knowledge. On the one hand, this popularization is an indication of a "dying metaphor." On the other hand, the fact that "it does not appear in all dictionaries suggests that the frontier of science is a fairly recent locution and that its historic resonances are not lost on the interpretive communities that encounter it" (Ceccarelli 2013: 33).

Against the background of the specific meaning of the frontier in the American context, the question that needs answering is why EU research policy makers, as well as the body of experts who advised the Commission (Harris et al. 2005), adapted the metaphor. To recall, the Commission resorted to the frontier concept in its legal proposal for establishing the ERC under FP7, although everyone knew that the ERC was all about funding "basic research." So why exactly was it that "frontier" rather than any other qualifying term was used to pinpoint the ERC's mission, given that EU policy-makers could resort to concepts more conversant in Europe? To answer this question, a functional perspective on language is helpful: the frontier helped European policy-makers circumvent the traditional distinction of basic and applied research—with its potential antagonistic meaning. This evasive strategy was even made explicit on the ERC's website: "Today the distinction between 'basic' and 'applied' research has become blurred. . . . As a result, the term 'frontier research' was coined for ERC activities since they will be directed to fundamental advances at and beyond the 'frontier' of knowledge."[14] At first sight, frontier research seems to get reduced to a fill-in concept, as the ERC and the Commission would endanger their own legitimacy by touching upon the social assumptions pertaining to "basic" vis-à-vis "applied" research and embracing the former. At the same time, the ERC attempted to clarify the term "frontier research" by stressing that it reflects a new understanding of basic research: "On the one hand it denotes that basic research in science and technology is of critical importance to economic and social welfare, and on the other that research at and beyond the frontiers of understanding is an intrinsically risky venture, progressing on new and most exciting research areas and is characterized by an absence of disciplinary boundaries."[15] Read sequentially, the ERC and the Commission substitute "basic research" with "frontier research." Additionally, the description of frontier research first promises economic growth, and only second promises social welfare, before it actually points to the original meaning of the term—that is, glorifying "an intrinsically risky venture." That the EU discourse can resort to these fairly old interpretive schemas points to both the impressive capabilities of the Commission as a policy entrepreneur and, more importantly, to how the historical baggage of concepts survives even if these concepts travel through time and space.

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The rhetorical move of the Commission seems to have been carefully prepared in the course of policymaking. As mentioned above, before releasing its legal proposal for FP7, the Commission ordered a "high-level expert group report," which was delivered in February 2005 (Harris et al. 2005) and confirmed that the distinction between basic and applied research was obsolete for modern research undertakings. The Commission resorted to a specific mixture of expertise, as it put together scientists; science, technology, and innovation (STI) policy scholars; high-ranking industrial representatives; and members of the Commission. It is noteworthy, however, that the same line of argumentation had been presented by the Commission one year before in the official communication, *Europe and Basic Research* (European Commission 2004), while neither this nor any other earlier document had mentioned the term "frontier research." Given the close interaction of policy experts from the commission and academic experts, it is difficult to assess who was responsible for the decision to introduce the new term. Nevertheless, the point is that these actors in the end came to a common understanding.

The functionality of the new concept for EU science policy was not restricted to the problem of obtaining competencies in basic research funding surreptitiously. Another crucial point emphasized in all reports and legal documents using the frontier metaphor was the geostrategic importance of science in an alleged "war for talents" fought against the United States, Japan, and other new global powers (e.g., European Commission 2004: 7–10). While this geostrategic demarcation discourse shielded both European science and the European Union's internal market from outsiders in a very blunt way, as in the ratified document of FP7, some of this preparatory work needed to be delivered with a subtle package. The best example is given by the title of the aforementioned report, *Frontier Research: The European Challenge,* obviously a combination of two famous book titles, but with some decisive modifications. The first component, *Frontier Research,* evidently referring to Vannevar Bush's report *Science—The Endless Frontier* (1945), implies the necessity of establishing a new European organization functionally similar to the U.S. National Science Foundation (NSF). The second component of the title, *The European Challenge,* alludes to Jean-Jacques Servan-Schreiber's international bestselling book, *The American Challenge* (1968), which presented the United States and Europe in the state of an economic and innovation war,[16] also known as the "technology gap."

The European Union's adoption of the "frontiers of science" metaphor has some notable aspects. Both the Commission and the ERC employed "frontier research" in order to cover tracks that could lead back to the distinction between basic and applied research. While other concepts could have been employed (for example, "strategic research" or "use-inspired basic re-

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search"), one cannot dismiss that the NSF was viewed as a role model for the establishment of the ERC. Some would even concede that this remodeling bears traces of the forlorn leitmotif of Europe as being fully integrated in a federalist nation—that is, the "United States of Europe" (Majone 2006: 610). Furthermore, paying tribute to Herbert Hoover's and especially Vannevar Bush's ideas of the frontiers of science alludes to the positive sides of risk-taking that go hand in hand with pioneering research activities. The Commission and the ERC presented all kinds of benefits resulting from frontier research, be it the vast number of U.S. Nobel Prize laureates, the immediate technology transfer from this allegedly breakthrough-type of research into successful market products, or the high citation impact of U.S.-based scientists. Again, the United States were presented as both a role model and a competitor in the same breath, which has empowered a specific European interpretation of the frontier. Beyond that, one can conclude that implementing the frontier metaphor in the European science policy context was not only aimed at establishing a new research funding agency but also a new and bold political approach of the Commission vis-à-vis national European science policy actors, including science funding agencies. Research undertakings funded by the ERC were meant to expand the frontiers of knowledge, but at the same time the Commission had *actually* expanded the political frontiers by seizing new competences hitherto claimed nationally.

## Grand Challenges

The establishment of the ERC as a European funding agency dedicated to basic research was the most conspicuous novelty in the Seventh Framework Programme (2007–2013). The subsequent Eighth Framework Programme (2013–2020) came with a new name, "Horizon 2020," and complemented the goals of "excellent science" and "industrial leadership" with a new rationale that built on the notion of "societal challenges" (European Commission 2011a, b).[17] In concrete terms, this meant that funding within the new Framework Programme would focus on six priority areas broadly circumscribed by the issues of health and demographic change, food supply and agriculture, energy security, transport, climate action and sustainability, and security.[18] In the parlance of the Commission, this "reflects the policy priorities of the Europe 2020 strategy and addresses major concerns shared by citizens in Europe and elsewhere" (European Commission 2011a: 5). The term "societal challenges" in these documents is used mostly synonymously with "grand challenges," a term officially introduced in EU policy in 2007 and 2008 to stipulate a new rationale for an all-encompassing coordination within the promised European Research Area (European Commission 2007,

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2008). Shortly after, in the so-called Lund Declaration (Swedish EU Presidency Conference 2009: 40) a large coalition of stakeholders and representatives from science policy, industry, and research organizations proposed that "European research must focus on the Grand Challenges of our time moving beyond current rigid thematic approaches."

As in the case of frontier research, the idea of grand challenges is not European by origin. Rather, the concept first appeared in U.S. science policy contexts of the late 1980s, indicating a need for federal funding in the field of computational sciences, especially with regard to the building of supercomputers (Hicks 2016). The first explicit definition can be found in a 1987 report by the U.S. Federal Coordinating Council for Science, Engineering, and Technology (FCCSET), outlining the High Performance Computing & Communications (HPCC) program: "A grand challenge is a fundamental problem in science or engineering, with broad applications, whose solution would be enabled by the application of the high performance computing resources that could become available in the near future" (OSTP 1987: 3). The definition illustrates that the new term is employed to mediate between basic and applied research, if not to revisit their distinction, and to clarify what is meant by basic/fundamental (problems of great concern) and by application (the promise of broad usability). Evidently, problems and solutions are conceived here in technical terms—that is, the idea of grand challenges is still close to what in the former terminology could have been labeled, for example, as application-oriented basic research. Following this stance, institutions such as the National Research Council (NRC 1988a, b, 1995), and star scientists (Reddy 1988; Wilson 1988, 1989) used the grand challenges concept to articulate their research agendas in the fields of computational sciences and artificial intelligence. Strikingly, throughout the 1990s, the concept was nearly exclusively used in these computing-related communities.[19]

After the millennium, however, definitions became broader. The NRC introduced the concept in new disciplinary contexts, such as environmental sciences (NRC 2001a), physics (NRC 2001b), earthquake engineering (NRC 2004), and biology (NRC 2009). The following statement, for example, shows more flexibility in regard to what kind of disciplinary knowledge is necessary to tackle grand challenges, which are now circumscribed as "major scientific tasks that are compelling for both intellectual and practical reasons, that offer potential for major breakthroughs on the basis of recent developments in science and technology, and that are feasible given current capabilities and a serious infusion of resources" (NRC 2001a: 2). By being translated into ever more different contexts, the notion of grand challenges became increasingly associated not only with technical but also with societal problems. This is no surprise, as one important aspect of the grand challenges discourse is its ap-

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peal to a broader public beyond academia. As early as in the 1980s, the first protagonists claimed that grand challenges "capture the imagination of the public" (Reddy 1988: 17). Scientists, policy makers, and other stakeholders in the United States (e.g., NRC 2001b: 147; Kalil 2012) and in Europe (e.g., European Commission 2008: 8; Royal Society 2011: 76) have reiterated these arguments ever since. The appeal to a broader public is particularly visible in the *Grand Challenges in Global Health* initiative, announced by Bill Gates in 2003 (Varmus et al. 2003; Varmus 2009).[20]

In 2009, quite simultaneously with the developments in Europe sketched above, the concept became a key element in the Obama administration's *Strategy for American Innovation,* which proposed to "harness science and technology to address the 'grand challenges' of the 21st century" (White House 2009: 22).[21] In 2012, the Office of Science and Technology Policy (OSTP) convened a conference on Grand Challenges (Dorgelo and Kalil 2012), and until the end of the Obama administration in January 2017, the OSTP displayed on its website a definition and a set of "grand challenges" that were funded by diverse U.S. government institutions, such as the National Institutes of Health (NIH), the Defense Advanced Research Projects Agency (DARPA), the National Science Foundation (NSF), the Department of Education (DOE), the National Aeronautics and Space Administration (NASA), and the U.S. Agency for International Development (USAID).[22] Those funding programs aimed at transforming the identity work of scientists and engineers, particularly in regard to collaborative research: the White House asked for "all hands on deck." The rhetoric employed here is an example for how the grand challenges discourse highlights the necessity of multilateral cooperation and collaboration (Keenan et al. 2012; Kallerud et al. 2013; Hoareau McGrath et al. 2014)—in this respect, the discourse moved clearly beyond older notions of applied research.

Discussing the increasingly prominent role of the grand challenges discourse in U.S. science policy, Diana Hicks (2016: 37) suggests that the new concept may be "a new way of seeing research." At the same time, Hicks stresses that the modernist basic/applied schema always lurks in the background. As a consequence, many actors "see the new language as another way of talking about applied research." Following Hicks, this is particularly the case when "grand challenges" are reworded as "societal challenges," which can then be juxtaposed with "scientific challenges" (Hicks 2016: 38). Indeed, the idea that science can and should be harnessed to address societal goals and problems does not seem new at first glance. As early as in the nineteenth century, the concept of "applied science" was associated not only with technological developments, but also with more general ideas of societal progress (see chapter 1). Furthermore, critical debates in the 1960s and 1970s have

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resulted in ever newer categories pertaining to knowledge production, such as "mission-oriented research" and "strategic research" (see chapters 2 and 3), as well as in the ideals of "interdisciplinarity" and, later, "transdisciplinarity." A common denominator of these new categories was the alignment of science toward real world problems, not defined by disciplinary developments but by societal demands.

Against this background, several science policy scholars have considered the grand challenges discourse as a reformulation of mission-oriented R&D programs (Gassler, Polt, and Rammer 2008; Cagnin, Amanatidou, and Keenan 2012; Foray, Mowery, and Nelson 2012; Mowery 2012; Amanatidou et al. 2014). In contrast, Hicks (2016) and some other observers avoid equating the grand challenges concept with traditional research categories and instead investigate whether and in what sense grand challenges are more than "old wine in new bottles" (Kallerud et al. 2013: 2; Calvert 2013: 475; Ulnicane 2016; Kaldewey 2017). This perspective is important because if we subsume the new discourse under an established concept, we miss the chance to elicit whether and how the new semantics transform the very categories we used before.

What, then, distinguishes the grand challenges concept from older categories of applied research and from those concepts that mediated between basic and applied research, as, for example, Stokes's (1997) notion of "use-inspired basic research"? The main difference is that grand challenges are no research category in the narrow sense, but rather represent a discourse about the role and future mission of the scientific community. In most definitions, grand challenges are conceptualized as long-term and large-scale research goals, determined by heterogeneous societal stakeholders. Thus, they enable actors to construct alternative objectives of science and science policy vis-à-vis the twentieth-century ideal of "technological innovation." Many observers therefore associate the grand challenges idea with new conceptions of innovation, such as "social innovation," "green innovation," or "responsible innovation."[23] Ideally, this means a democratization of priority-setting that would make science more independent of economic interests.[24] Furthermore, the idea that grand and global challenges have to be addressed by close cooperation between different actors and beyond national borders is obviously related to the idea of a social contract between science and society[25]—yet quite a different one, if compared to the contract idea we know from twentieth-century science policy. Jane Calvert (2013: 475), for example, suggests that introducing grand challenges indicates a "political renegotiation of the value of science." If grand challenges are understood as "publicly stated priorities," then they "could be seen as part of an attempt to establish a new contract for the public funding of science."

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However, such analytical definitions of grand challenges do not explain the relevance of the discourse for present-day transformations of the identity work in science and science policy. To assess whether the grand challenges concept really is able to replace the "tired categories" (Hicks 2016: 39) of twentieth-century science policy, one has to take a closer look at the history and performativity of the concept, its tacit presuppositions, and the deep structure of the discourse in which it is embedded (Kaldewey 2017). Historical semantics teach us that language in general and contested concepts in particular are both "indicators" and "factors" of social and political change (Koselleck 2004: 251; see also Olsen 2012: 171). To understand whether and how the grand challenges discourse can actually be interpreted as a driving force in the history of science policy, it is necessary to trace its origin, its changing meaning, and how it has diffused and proliferated in various contexts within and beyond academia.

In plain English, both the noun "challenge" and the adjective "grand" convey a meaning that evolved independently of any scientific or science policy context. The term "challenge" has its origin in Middle English, where it was used in the sense of "accusation" (as a noun) or "to accuse" (as a verb). Being confronted with a challenge implied a demand to stand up against an accusation. The most salient form of such a challenge was the duel as an arranged combat between two individuals, traditionally noblemen. Following etymological dictionaries, this accusatory connotation died out in the seventeenth century. What remains today is the notion of someone participating "in a competitive situation or fight to decide who is superior in terms of ability or strength."[26] Since the nineteenth century, the term "challenge" has been particularly associated with the sphere of sports. In various disciplines, "challenge cups" have been institutionalized as specific forms of competition, and the event of a "world title challenge" evokes the older meaning of challenging an individual to a duel. There is also evidence that the phrase "grand challenge," which has never been common in everyday speech, has its origin in sports. In 1839, the "Grand Challenge Cup," a men's eight-crew rowing competition, was initiated and institutionalized as the most prestigious event of the annual Henley Royal Regatta on the River Thames; with few exceptions, that competition has continued through to the present under the same name.

It is not until the 1980s that "grand challenges" disseminate beyond the sphere of sports and appear in those peculiar U.S. computational science and science policy contexts described above. Unsurprisingly, during the formative phase of the grand challenges discourse from the late 1980s to the early 2000s, several examples point to the coupling of science and technology developments with the logic of sports. A first example for using sports-like challenges

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in the sphere of science and technology is the annual international RoboCup competition, which took place for the first time in 1997 in Nagoya, Japan. Some years before, the main figure behind the RoboCup, computer scientist Hiroaki Kitano, had copublished a paper with scientists from the United States, Japan, and Germany, outlining "Grand Challenge AI Applications" and referring explicitly to the grand challenges definition of the American HPCC program (Kitano et al. 1993). Two years later, Kitano and his Japanese colleagues presented the idea of using a "Robot World Cup" as a new "standard problem" for AI and robotics research, from which "a lot of interesting research issues will arise" (Kitano et al. 1995: 1). In short, they considered it a "grand-challenge project" (Kitano et al. 1997: 73).

A second example brings us back to U.S. science policy contexts. In 2003, DARPA announced the plan of a "Grand Challenge for autonomous robotic ground vehicles," intended to spur technological development for military applications.[27] Teams of professionals and amateurs were invited to develop autonomous vehicles able to navigate an off-road course in the desert between Los Angeles and Las Vegas, with the winner being promised prize money of $1.0 million. While the inaugural challenge in March 2004, in which no team completed the course, was reported by *Popular Science* magazine as "DARPA's debacle in the desert" (Hooper 2004),[28] the agency itself was enthusiastic:

> All across the nation, from garages to high schools, from universities to corporate laboratories, hundreds—perhaps thousands—of people worked on solving a problem important to the DoD. We had hoped that the Grand Challenge would excite many people, but it grew into something much, much bigger than anyone had imagined. The Congressionally authorized prize authority inspired many smart people who would not ordinarily work on a problem important to DoD, dedicating long days, nights and weekends toward finding a solution. (Tether 2005: 8)

Immediately after the event, DARPA's director, Tony Tether, stated in a press release that "we learned a tremendous amount today about autonomous ground vehicle technology" and that even those vehicles that did not come very far "made it to the Challenge."[29] In other words, not only did Tether highlight the scientific relevance of the Challenge, but also its character as a sports-like event, which was held a value in itself, as in the Olympic Games, in which "the important thing is not winning but taking part."[30] Consequently, the DARPA Grand Challenge was repeated in October 2005, with the prize doubled to $2.0 million, and this time five of twenty-three vehicles completed the course. The winning team was led by Sebastian Thrun, then the head of Stanford's Artificial Intelligence Laboratory (SAIL) and later responsible for Google's driverless-car program. The event, the team, and the winning car,

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which was named Stanley, received media coverage (e.g., Davis 2006) and were successful in terms of scientific reputation. The *Journal of Field Robotics* published a highly cited paper with the title "Stanley: The Robot that Won the DARPA Grand Challenge" (Thrun et al. 2006).

The two examples indicate that the meaning enshrined in the grand challenges discourse tacitly introduces aspects of the logic of sports into the science system (Kaldewey 2017). This is particularly noteworthy when we compare the grand challenges discourse to more traditional conceptions of research (as shown in the other chapters of this volume): "pure science" had, at least in the U.S. context, a religious and moral connotation; "applied research" mostly refers to industry and business; and "problem-oriented research," "interdisciplinarity," and "transdisciplinarity" have a decidedly political tone. All of these concepts translate the logic of specific societal spheres (religion, the economy, politics) to make sense of the practices and goals of scientific research. The grand challenges discourse now discloses a new reference point for how we talk and think about science, technology, and their social embeddedness: the logic of sports and competition, leading to self-mobilization and, ultimately, to self-optimization of the participating scientists and engineers.

The structural potency of a concept—that is, its capacity to become a factor of historical change—depends on how successfully it is institutionalized both in discourses and social structures. For example, one reason for the success of the concepts of basic and applied research was their embeddedness as categories in international R&D statistics (Godin 2005). As a result, they were the key to formulating science policies around the world until today. As for the grand challenges concept, it is probably too early to assess whether it will become stabilized as a "social fact" in a way comparable to the linear model of innovation (Godin 2006). There are, however, several indicators pointing to successful processes of institutionalization.

First, as discussed above, the grand challenges concept has become a key rationale for science policy in various contexts, most prominently in the United States and in Europe, but partly also in other cultural contexts, such as India or China (Hoareau McGrath et al. 2014). Second, the concept was picked up in a variety of contexts beyond science policy in the narrow sense. It has been included in the Organisation for Economic Co-operation and Development's (OECD 2010, 2012) innovation strategies, influenced the practices of new philanthropic organizations (Brooks et al. 2009), and was taken up by private think tanks (e.g., Pamlin and Armstrong 2015). Third, the concept is increasingly translated into specific transdisciplinary curricula at renowned universities. For example, in 2007, Princeton University established a "Grand Challenges Program" focusing on issues of climate and energy, development, and health. A glossy pamphlet asks the reader to "imag-

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ine a world in which the brightest minds work together to solve humanity's most pressing environmental problems, a transformative world that expands classroom learning beyond traditional academic and national boundaries."[31] More radically, Michael Crow, president of Arizona State University, aimed to achieve a comprehensive reconceptualization of research and teaching, "to seek solutions to the grand challenges associated with sustainability," the result of which would be the "New American University" (Crow 2010). Finally, and this is perhaps the most interesting point, the grand challenges discourse also impacts scientific communication and research practices. There is some evidence that normal scientific publications increasingly refer to grand challenges (as indicated, for example, in figure 9.5). Furthermore, prominent new journals are no longer organized around specific research fields or disciplines; instead, they focus on challenges such as climate change (e.g., *Nature Climate Change,* since October 2010) or energy security (e.g., *Nature Energy,* since January 2016). As publishing is crucial for every scientist, we may assume that a journal expecting its authors to address grand challenges in their research may significantly influence their choice of problem and research trajectories.

## Beyond the Dichotomy: From Boundary Work to Identity Work

Science policy discourses in Europe and elsewhere have seen the arrival and adoption of new concepts that leave behind the twentieth-century basic/applied distinction and challenge the linear model of innovation, as well as the ideas of differentiation, orderliness, and contractual relations associated with it. The two concepts discussed in this chapter—frontier research and grand challenges—make visible different trajectories and ideologies of science policy and research planning in the twenty-first century. At the same time, they are comparable in their character as "travelling concepts" (Bal 2002),[32] not so much in the sense of the original epistemological call for letting "concepts" travel between disciplines for the sake of seizing common understandings (Bal 2002: 24) but in their traceable journey across time, regions, and different social contexts (Hyvärinen 2013: 17)—most noteworthy from the United States to Europe, and from popularized to professional contexts of science policy expertise.

The "frontier" was made prominent in North America as a metaphor conveying liberalist ideas—that is, portraying bold individuals who were unleashed from Europeanist intellectualism and national political regulation to venture out into the unknown and to ultimately bring about prosperity for society. In defiance of its initial intra-academic contestation—Turner's hypothesis of an American exceptionalism triggered a *Historikerstreit*—the frontier became very popular among the public. For decades, U.S. presidents

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and other prominent decision-makers used the concept to justify spending on risk-taking endeavors in the field of science and technology. The appeal of this U.S. concept for EU science policymaking was not so much due to an allusion to the glorious achievements of U.S. science and its allegedly stupendous funding, but to circumnavigate the distinction between basic and applied research and thus be able to establish a European Research Council. Before, European research policy had been more strictly differentiated between national, transnational, and supranational policies, with the latter being restricted to financing near-market R&D activities. Frontier research, again, was a rhetorical substitute for basic research, but more than this: the ERC's mission is to stimulate scientific individualism and a risk-oriented attitude as opposed to the hitherto integrative—that is, collaborative—research. Thereby, the EU's geostrategy, in which the ERC is embedded, is to battle the alleged preeminence of the U.S. knowledge-based economy. Borrowing a science policy concept from another country to fuel its underlying rivalry is not actually ironic, but reflects the ambivalent position of EU political actors toward the United States, with the latter being a rival as well as a role model (Majone 2006).

In a similar vein, the grand challenges narrative traveled a long journey from its origin in the sphere of sport in the nineteenth century, to the problems of U.S. federal funding of computer sciences in the 1980s, to grand challenge competitions in artificial intelligence and engineering that addressed a broader public, before finally being standardized as a new rationale for science policy around the globe. While "grand challenges" thus journeyed back and forth with regard to the national und supranational political contexts of the United States and Europe, they are at the same time conceived as "global challenges," referring to pressing issues that lie beyond political borders. They call not for national policies, but for the global community and various stakeholders from politics, science, and the economy to take action. Moreover, similar to the properties of the frontier, the concept of grand challenges instantaneously intertwines failure and success in the face of an immediate or foreseeable struggle; it is not by accident that its semantics are related to and rooted in sports challenges. Recalling the notion of a social contract, the concept offers the bridging of a gap between science and society by embracing potentially all actors, public and private (sometimes philanthropic) ones, to invest great efforts and to coordinate with each other in solving a concrete problem.

What the two concepts avoid are the kind of dichotomies we know from former science policy discourses that built, not least, on the basic/applied distinction. Particularly if seen from the perspective of science policy, these new concepts are not supposed to be negated: funding agencies do usually not support scientists working on problems that are interpreted as petty.

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In a similar vein, science policy is increasingly oriented at ultimate solutions, innovations, and impact for society. Hence, there can neither be "small challenges" nor alternatives to the frontier, which is by definition "endless." As a consequence, however, these new concepts are all-encompassing and sticky *because* they have no clear ambit.

This is, after all, how the new science policy discourses axiomatically diverge from older descriptions pertaining to the linear model of innovation. Building on the basic/applied dichotomy, science policy in the twentieth century oscillated between strategies of *boundary work* and strategies of *identity work*: on the one hand, scientists and research organizations could demarcate their work as distinct and autonomous activities; on the other hand, they could also conceive of themselves as being part of a more comprehensive whole. They could creatively "cross the distinction" (Spencer Brown 1969), moving from one side to the other and assigning positive and negative values, depending on context-dependent necessities. In contrast, science policy in the twenty-first century seems to opt out of these oscillations. From now on, *boundary work* is no longer politically correct. Instead, the *identity work* of scientists and research organizations is aligned toward those goals that are associated with the frontiers of knowledge and the grand challenges of world society. In contrast to conflict-laden boundary disputes, the new narratives proclaim the existence of general values and goals toward which the whole research enterprise has to be oriented. The open question is whether it has become more difficult in the twenty-first century for the scientific community to step back—that is, to suspend solving the big problems of humanity in order to conduct "normal science."

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In his book *Wahrheit und Nützlichkeit* (2013), he explored discourses on the goals and values of science in a long-durée perspective. Recent publications in *Minerva* (2017), *Research Policy* (2018, with Tim Flink), and *Science and Public Policy* (2017, with Désirée Schauz) deal with the changing relationship of science and politics, particularly with the contemporary pluralization of science policy discourses and how they transform the identity work of scholars, scientists, and policy makers.

## Notes

1. The *Web of Science* core collection includes the Science Citation Index, the Social Science Citation Index, and the Arts & Humanities Citation Index.
2. Quite similar to the linear model of innovation, it seems not to matter that economic theories of public goods are contested (Coase 1974; Callon 1994; Nelson and Romer 1996; Bozeman 2000; Foray 2000). There is a mutually stabilizing correspondence between the linear model and public good theory in how they tell a story of functional differentiation: while the glory days of a caretaking welfare state were not deemed over yet (Esping-Andersen 1990), neither were those whose enterprises had been doing basic research themselves, now that there was market competition that would hardly allow for these activities, while returns on R&D investments dwindled down. Against this background, the state was appealed to provide scientific knowledge as a public good (Scharpf 1999: 36), particularly via basic research, but also through applied research. Enterprises within a nation-state (or within other political realms, such as the European Union) could then take advantage of this public supply, especially in order to remain competitive on international markets. This "market-failure paradigm" (Bozeman 2000) also extends to state regulations—for example, legal frameworks for technology transfer activities, patent law, standardizations, and public procurement.
3. As regards the relationship of narrative and identity, see Somers 1994. This approach, however, is very much focused on individuals and groups. We propose to broaden the perspective and conceive of "identity work" in regard to epistemic communities, and, ultimately, in regard to science in general. To do so, we combine methods from conceptual history and cognitive metaphorical analysis, as outlined in Flink and Kaldewey 2018 and Flink and Peter 2018.
4. After World War II, the United States had become an international role model for science policy. The concept of "basic research," in particular, can be regarded as an extremely successful science policy export product (see the introduction to and chapter 3 of this volume). What we show in this chapter is that the influence of the United States regarding the global language of science policy goes far beyond this single concept.
5. As regards these concepts, see Benoît Godin, "X-innovation: A Story of Appropriation and Contestation" (unpublished manuscript). For an analysis of "responsible research and innovation" (RRI) see Flink and Kaldewey 2018.

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6. Though rarely documented (e.g., Nature News 1968; King 1968), there were various plans or de facto realizations of what could count as today's ERC—e.g., the Royal Society's European Scientific Exchange Program, plans to establish a European Medical Research Council, the Cooperation for Science and Technology (COST, founded in 1971), a European Scientific Research Council (planned in 1972), and the creation of the actual European Science Foundation (ESF) in 1974.
7. The Royal Society first assailed the Commission (Nature Editorial 1995), followed by molecular biologists, about the prestigious European Molecular Biology Organization (EMBO) (Abbott 1995; Gannon 2000, 2001, 2002; Breithaupt 2003), with the latter teaming up with the Federation of European Biochemistry (FEBS) to streamline and widen their efforts of what would become the *Initiative for Science in Europe.* Other national and transnational R&D actors joined the cause of this epistemic community more or less overtly. This movement was reinforced by prestigious scientific journals, such as *Nature* and *Science* (Flink 2016). The ERC became a salient issue for EU research policy and, at the latest stage of agenda setting, it lobbied for prominent conferences organized under the auspices of the Swedish, Danish, and Irish EU presidencies from 2001 to 2004.
8. There are Frontier cafés and cinemas, Frontier Airlines headquartered in Denver, and various other business enterprises carrying the name, such as the primary Frontier School of Innovation in Kansas City, as well as the Energy Frontier Research Centers, established by the Office of Basic Energy Sciences in the U.S. Department of Energy's Office of Science, to provide just a few examples.
9. Turner—until his presentation a nobody among historians—became immediately famous due to his presentation in Chicago, as the Historical Association convened openly during the World's Columbian Exposition, and its speeches, especially those celebrating U.S. achievements since 1492, attracted large extramural audiences.
10. While Turner could not present any clear definition of the frontier whatsoever, his thinking and metaphorical writings borrowed greatly from evolutionary human geography and biology. It conjectured that people entering the American society would turn into "new men." These were called "germs" that become part of a "social organism," thriving and prospering even and especially in the face of adverse conditions (Coleman 1966: 24–26).
11. Again, Turner distinguished his ideal brave and experimental scientists from the image of a traditional and conservative scholar restrained by deep contemplation.
12. Hoover took up Turner's idea of a frontier of science in his book *American Individualism,* which he wrote in 1922 as secretary of commerce. In Herbert Hoover's presidency (1929–1933), the Great Depression gave him an incredibly hard time, and he still adhered to the same metaphor in trying to deliver messages of hope for a suffering society (Hart 2010).
13. Notably, most science policy-related references to this report circumvent discussing the concept of the frontier, while focusing on its relevance pertaining to the linear model of innovation, whereby basic research is usually emphasized. This lack of scholarly attention is criticized by Ceccarelli (2013: 43–45).

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14. "Mission." *About ERC.* European Research Council website. European Commission. Retrieved 27 May 2016 from https://erc.europa.eu/about-erc/mission.
15. "Frontier Research." *Glossary.* European Research Council website. European Commission. Retrieved 27 May 2016 from https://erc.europa.eu/glossary/term/267.
16. According to Servan-Schreiber, Europe was outclassed by the United States in management and technology development and suffered from heavy brain-drain of top-talented workforce.
17. This alleged newness should not hide the fact that the political predefinition of thematic priorities concerning the tackling of societal issues has been a strong rationale as of the Third Framework Programme, most notably with respect to health, energy, and environment (Kuhlmann and Reger 1995; Abels 2003). However, the Framework Programmes have been set up primarily to boost the technology and innovative capacities of business enterprises, and, therefore, societal issues were hitherto subordinated.
18. From the estimated total budget of 77 billion euros, 29.7 billion euros—that is, on average, 4.2 billion euros per year—have been dedicated to this new rationale (European Commission 2013). The distribution of the money explains something about real priorities: (1.) "Health, demographic change and wellbeing"—7.4 billion euros; (2.) "Food security, sustainable agriculture and forestry, marine, maritime and inland water research and the Bioeconomy"—3.9 billion euros; (3.) "Secure, clean and efficient energy"—5.9 billion euros; (4.) "Smart, green and integrated transport"—6.3 billion euros; (5.) "Climate action, environment resource efficiency and raw materials"—3.1 billion euros; (6.) "Europe in a changing world—Inclusive, innovative and reflective societies"—1.3 billion euros; (7.) "Secure societies—Protecting freedom and security of Europe and its citizens"—1.7 billion euros.
19. There are, however, some early references to grand challenges in the field of environmental research (Brown 1994), in ocean and polar sciences (Hempel 1996; Prandle 1997), in geophysical research (Lyons 1998; Raeder et al. 1998), and in regard to educational problems (Ehrmann 1999).
20. Notably, Nobel Prize winner Harold Varmus, who chaired the international board of scientists that was responsible for the program, himself became world famous for his earnest attempts to make research, especially in medicine, accessible for scientists and practitioners of developing countries, which, among other initiatives, led to the creation of the Public Library of Science (PLoS) and boosted the open access movement.
21. In the following years, two updated versions of this report were published (White House 2011, 2015). During that time, the strategy evolved from simple to-do lists to a new and ambitious policy tool (Hicks 2016: 31–34).
22. See "21st Century Grand Challenges" on the archived Office of Science and Technology Policy website, retrieved 27 July 2017 from https://obamawhitehouse.archives.gov/administration/eop/ostp/grand-challenges.
23. The "Responsible Research and Innovation" framework of the European Com-

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mission, for example, is intimately connected to the goal of tackling societal challenges. See, for example, the leaflet entitled “Responsible Research and Innovation: Europe’s ability to respond to societal challenges,” retrieved 3 August 2017 from https://ec.europa.eu/research/swafs/pdf/pub_public_engagement/responsible-research-and-innovation-leaflet_en.pdf.

24. Several authors formulated this hope, however, at the same time they criticized the idea of grand challenges for remaining stuck within the logic of capitalism or neoliberalism (Brooks et al. 2009; Vostal, Silvaggi, and Vasilaki 2011; Cech 2012; Calvert 2013).
25. An example for a close coupling between the reference to grand challenges and the idea of a new contract is the 2011 report from the German Advisory Council on Global Change (WBGU 2011).
26. “Challenge.” *Oxford Dictionary of English,* current online version, retrieved 27 July 2017 from https://en.oxforddictionaries.com/definition/challenge.
27. “DARPA Plans Grand Challenge for Robotic Ground Vehicles,” press release, 2 January 2003, retrieved 15 June 2016 from http://archive.darpa.mil/grandchallenge04/media/announcement.pdf; “DARPA Outlines Plans for Grand Challenge at Competitors’ Conference,” press release, 22 February 2003, retrieved 15 June 2016 from http://archive.darpa.mil/grandchallenge04/media/comp_conf_rel.pdf.
28. The playing field was set with 142 miles and a time limit of ten hours. None of the fifteen vehicles actually making it over the scratch line reached the goal, and the four most successful teams only managed five to seven miles before their vehicles dropped out.
29. “American Innovators Take Robotic Technology into the Field during Saturday’s Inaugural DARPA Grand Challenge,” press release, 13 March 2004, retrieved 15 June 2016 from http://archive.darpa.mil/grandchallenge04/media/innovators.pdf.
30. This famous phrase was part of a speech given in London in 1908 by Pierre de Coubertin, founder of the International Olympic Committee.
31. “Grand Challenges Program,” Princeton University website, retrieved 19 August 2015 from http://www.princeton.edu/grandchallenges/about/progress-report/gc_pamphlet.pdf.
32. The term “traveling” is metaphorical by itself and certainly does not suggest that concepts desert their semantic provenance. The concept might be still meaningful within the social world it arose from, while it now also appears somewhere far away from its very origination to convey cognate ideas.

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