Chapter 6

Beyond the Basic/ Applied Distinction?

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

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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 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 institutes 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 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

(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 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-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 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 physicist 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 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 university 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 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 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).

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.

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).

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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