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Biochemical Research Institutes in the Max Planck Society to ca. 1990

Preprint 21

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Preface

The following study examines developments in the principal biochemical research institutes of the Max Planck Society for the Advancement of Science (MPG) from the war, when it was still the Kaiser Wilhelm Society (KWG) to around 1990. For background, it has been useful to include a less detailed account of the prewar development of the KWG's biochemical institutes, focusing on two in particular, the Kaiser Wilhelm Institute (KWI) for Biochemistry in Dahlem and the KWI for Medical Research in Heidelberg. Two key players emerged from the prewar experience in these institutes, Adolf Butenandt (1903–1995), who became the second president of the MPG in 1960, and Richard Kuhn (1900–1967), who was the opposing candidate for the presidency in 1960. Kuhn died of cancer in 1967, but Butenandt's twelve-year presidency effectively transformed the MPG from the small-institute format that had been carried over from the KWG and had characterized the Dahlem community to a series of much larger institutes, in effect a kind of »big science«. An early exception to the small-institute policy had been Kuhn's Heidelberg institute, which was planned as an interdisciplinary community of four smaller institutes within a single building, in effect a kind of mini-Dahlem on the Neckar. At the end of the war, Dahlem was temporarily shut down as a scientific community, a particular disappointment for Butenandt who had engaged collaboratively with the other research institutes, most notably by establishing with the KWI for Biology a joint research group for virus research, but less positively by enabling one of his associates to collaborate with the KWI for Anthropology in a study of blood chemistry using samples from Auschwitz victims.[1] Along with many other institutes, in 1943–1944 the KWI for Biochemistry had moved out to Tübingen, which (along with Göttingen) became a kind of »new Dahlem«.[2] Butenandt ultimately could not be satisfied with the situation in Tübingen, however, and, during the early 1950s, he initiated a move to Munich, with the construction of a new MPI there in somewhat close proximity to two other biochemistry-related MPIs. This in turn also failed to satisfy him, and after he assumed the presidency, he began the process that would culminate a decade later in the consolidation of all three institutes into a new, much larger MPI for Biochemistry out in Martinsried, southwest of Munich. The Martinsried biochemical center, as it was initially known, was consciously planned as a »little Dahlem« or »new Dahlem«, but in the form of a single institute with a dozen or so semi-autonomous sections organized in a collegial structure modelled on American departments. It also to some extent resembled the original version of Kuhn's Heidelberg KWI, but on a much larger scale. The new approach reflected both the influence of »Americanization« in science, driven by what John Krige has called American hegemony in postwar

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1 Achim Trunk: *Zweihundert Blutproben aus Auschwitz. Ein Forschungsvorhaben zwischen Anthropologie und Biochemie (1943–1945).* Vol. 12. Berlin: Vorabdrucke des Forschungsprogramms »Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus« 2003; Achim Trunk: Biochemistry in Wartime. The Life and Lessons of Adolf Butenandt, 1936–1946. *Minerva* 44/3 (2006), 285–306. doi:10.1007/s11024-006-9002-2.

2 Paul J. Weindling: Verdacht, Kontrolle, Aussöhnung. Adolf Butenands Platz in der Wissenschaftspolitik der Westalliierten (1942–1969). In: Wolfgang Schieder and Achim Trunk (eds.): *Adolf Butenandt und die Kaiser-Wilhelm-Gesellschaft. Wissenschaft, Industrie und Politik im ›Dritten Reich‹.* Göttingen: Wallstein Verlag 2004, 320–345, 321 giving the impression that this was a wartime observation but unfortunately citing no source for this phrase.

(Western) Europe,[3] as well as a (West) German effort to challenge that hegemony and reclaim some of Germany's lost prewar scientific glory in the field of biochemistry (and the new postwar field of molecular biology). This study provides a case-study of these developments, while also considering the Martinsried project as a new phase in development from the original Dahlem project of the KWG.

3 John Krige: *American Hegemony and the Postwar Reconstruction of Science in Europe*. Cambridge, MA: MIT Press 2006, esp. chapter 9. Martinsried will be the subject of a doctoral dissertation currently in progress, but not yet available. Magnus Altschäfl: *Globale Konkurrenz – Lokale Kooperation. Die Entstehung des lebenswissenschaftlichen Forschungsstandort München-Martinsried, 1960–1995*, forthcoming. The author was kind enough to send me unpublished and incomplete drafts of his introduction and of an accompanying paper, Magnus Altschäfl: Who's Laughing Now? The *Tertius Gaudens* in Multi-Tier Competitions, in progress.

1 Prologue: The Interaction of Chemistry and Biology in the Kaiser Wilhelm Institutes

1.1 Emil Fischer's dream for Dahlem (1915): creating a »synthetic-chemical biology«

> Fortunately there are [. . .] between the two great scientific disciplines [chemistry and biology] numerous points of contact, which invite work in common, and one may hope that this work will draw many advantages from the close proximity of the Dahlem institutes. [. . .] And so I see, half in a dream, the emergence of a synthetic-chemical biology that will transform the living world as fundamentally as chemistry, physics, and industry have done for so long with non-living nature.[4]

In October 1915 the Kaiser Wilhelm Society for the Advancement of the Sciences (KWG) was less than five years old. Its first two research institutes, for chemistry and physical chemistry, had opened just three years earlier in Dahlem, a suburb in southwest Berlin. The most influential scientist in the KWG's leadership (indeed the only practicing natural scientist on its administrative committee) was the organic chemist Emil Fischer (1852–1919), ordinarius professor at the University of Berlin and director of Germany's largest teaching institute for chemistry, which also served as the research laboratory of the Berlin Academy of Sciences. As an organic chemist, Fischer had won the 1902 Nobel Prize for his pioneering work on the systematic analysis and synthesis of moderately complex biological molecules, beginning with carbohydrates. His ambition was to transform biology into an experimental science based on structural chemistry, to which end he had unsuccessfully sought to synthesize proteins and nucleic acids, with the ultimate prospect of modifying genes, whose chemical nature was as yet almost wholly unknown.[5] Fischer recognized that despite the global dominance of German organic chemists, German universities were not especially suitable locations for pursuing his goal. Hence, in effect, what Fischer was calling for was the creation of a new discipline, and he believed that this would have to emerge from research collaboration between chemists (or biochemists) and biologists in a context other than the universities. That context would be, he hoped, the scientific community in Dahlem.

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4 Emil Fischer: Die Kaiser-Wilhelm-Institute und der Zusammenhang von organischer Chemie und Biologie (1915). In: Max Bergmann (ed.): *Untersuchungen aus verschiedenen Gebieten. Vorträge und Abhandlungen allgemeinen Inhalts*. Berlin: Springer 1924, 796–809, 797–798, 808. Unless otherwise indicated in the notes, all translations from the original German in this paper will be my own.

5 On Fischer's goal of a chemistry-based experimental biology, see Fischer, Die Kaiser-Wilhelm-Institute, 1924, 796–809, 808; Jeffrey Allan Johnson: From Bio-Organic Chemistry to Molecular and Synthetic Biology: Fulfilling Emil Fischer's Dream. In: Masanori Kaji et al. (eds.): *Transformation of Chemistry from the 1920s to the 1960s. Proceedings of the International Workshop on the History of Chemistry (March 2–4, 2015)*. Tokyo: Japanese Society for the History of Chemistry 2016, 1–13; Joseph S. Fruton: *Proteins, Enzymes, Genes: The Interplay of Chemistry and Biology*. New Haven, CT: Yale University Press 1999, 186–190 and passim. On the gene as chemically unknown in 1915, cf. Evelyn Fox Keller: *The Century of the Gene*. Cambridge, MA: Harvard University Press 2000, 1–3.

Biology did not then exist as a university discipline in Germany; the fields that could be said to constitute biology were broken up into separate faculties and institutes: anatomy and physiology in the medical faculties, zoology and botany in the philosophical or natural-science faculties. The situation was not much better for the fields of chemistry most closely related to biology. A century earlier, most university chemists had been based in medical faculties, but over the course of the century, there had been a general migration to the philosophical or natural science faculties, where structural organic chemists had come to hold a dominant position in the university chemistry institutes. Within the medical faculties, there remained a few chairs specifically for medical chemistry or what had originally been called animal chemistry (*Thierchemie*), a name that implied inexact methods incapable of precisely analyzing impure samples, and thus disdained by the structural organic chemists (»Thierchemie ist Schmierchemie« – animal chemistry is sloppy chemistry).[6] Thus, by 1915, the disciplines of physiological chemistry and pathological chemistry had replaced animal chemistry in the medical faculties, and since 1877 there had been a *Zeitschrift für physiologische Chemie*, founded and edited by the renowned Felix Hoppe-Seyler (1825–1895). It was not until this journal was published in English that the title changed from physiological to biological chemistry (1985). Indeed, there were two similar, but competing disciplines differentiated largely by the faculty in which they were based. Despite the success of Hoppe-Seyler's journal, on the medical side there were as yet only a few independent institutes and Ordinarius professors for physiological chemistry in 1915. In most cases physiological chemists were appointed as section heads within larger institutes for physiology. In the case of the philosophical or natural science faculties, there was the more recently developed discipline of biochemistry, organized around the *Biochemische Zeitschrift* founded in 1906 by Carl Neuberg (1877–1956). Initially, there was a considerable overlap in the topics, audiences, and contributors to each of these journals, but during the decades after Fischer's talk in 1915 they tended to become separated along faculty lines, with the first journal appealing more to those in the medical faculties, the second more to natural scientists.[7]

Fischer's influence on the early development of the KWG (until his death in 1919) ensured that its early institutes featured chemists with interests in biological questions as well as biologists working on topics that might offer themselves to chemical analysis. The KWI for Chemistry, one of the first two institutes in Dahlem and the result of a project Fischer had been involved with since 1905,[8] had as its first scientific member Richard Willstätter (1872–1942), who had worked on the structure of alkaloids such as cocaine as well as the structure of chlorophyll, the essential chemical for photosynthesis in plants. He left Dahlem in 1915 (the year of his Nobel

6 A cliché among the »pure« organic chemists of the later nineteenth century, found in many sources, including Henry E. Armstrong: Chemistry in the Making. *Nature* 104/2610 (1919), 219–221, 220. doi:https://doi.org/10.1038/104219a0. Armstrong attributes it to Emil Fischer, but I strongly suspect an earlier origin for the phrase.

7 For a systematic institutional analysis of the situation of biochemistry in the German universities, see Robert E. Kohler: *From Medical Chemistry to Biochemistry. The Making of a Biomedical Discipline*. Cambridge: Cambridge University Press 1982, chapter 2.

8 Details in Jeffrey Allan Johnson: *The Kaiser's Chemists. Science and Modernization in Imperial Germany*. Chapel Hill, NC: University of North Carolina Press 1990.

Prize in Chemistry) to become the successor to Fischer's mentor (and his own), the organic chemist Adolf von Baeyer (1835–1917) at the University of Munich. The pioneering biochemist Neuberg, who had done his doctoral work in Fischer's institute, came to Dahlem in 1913, at the time of one of his greatest achievements, the clarification of the chemical processes involved in alcoholic fermentation. He became head of the chemistry section of the newly opened KWI for Experimental Therapy directed by the bacteriologist August von Wassermann (1866–1925), inventor of the Wassermann blood test for syphilis. In 1917 Neuberg's section would become a KWI for Biochemistry within the same building as Wassermann's institute, which merged into the biochemical institute following Wassermann's death.

In the KWI for Biology (possibly the first German academic institute designated for »biology« as a discipline), whose building was begun just before the war but whose opening and full operation were subjected to wartime delays, one of the first two directors was the botanist and geneticist Carl Correns (1864–1933). Another member specializing in genetics was the zoologist Richard Goldschmidt (1878–1958), then interned in the United States. Genetics was one of the biological fields that in his 1915 lecture Fischer argued could be understood and potentially subjected to scientific control with the help of chemistry, though this was as yet far from realization, and thus something he could see only »half in a dream«. Fischer's former student Otto Warburg (1883–1970), then in military service, was to head a section for physiology, in which he would examine the processes of respiration and tumor development from a chemical perspective. Warburg's section would become independent as the KWI for Cell Physiology in 1930, shortly before he was awarded the Nobel Prize for Physiology and Medicine in 1931 for his »discovery of the nature and mode of action of the respiratory enzyme.«[9]

The figures below show first, the Dahlem institutes around 1918 (Fig. 1), and then in the early 1930s, following the construction of Warburg's institute (Fig. 2). It is clear that even then the research community remained in a relatively isolated, semi-rural setting. The lack of a central dining hall posed problems for the institute staffs. Fischer had rejected this idea on the grounds of economy, saying The KWG »isn't building any pubs!«[10] But he failed to see how a dining hall could foster intellectual exchange.

9 The Nobel Prize: Otto Warburg. Biographical. https://www.nobelprize.org/prizes/medicine/1931/warburg/biographical/. Last accessed 1/26/2020.

10 Richard Willstätter: *From My Life: The Memoirs of Richard Willstätter*. Translated by Lilli S. Hornig. New York, NY: W. A. Benjamin 1965, 222.

Fig. 1. An aerial photograph of the nascent Dahlem research community around 1918. The first two Institutes (opened 1912) are in the center foreground, with the KWI for Chemistry on the left and the KWI for Physical Chemistry and Electrochemistry on the right, flanked by the directors' residences. The low buildings between the two institutes are temporary structures built for wartime chemical warfare research. The villa on the extreme right had been Richard Willstätter's home until 1915, across the street from his friend Fritz Haber, director of the KWI for Physical Chemistry. On the extreme left is the KWI for Experimental Therapy (opened 1913), later the KWI for Biochemistry. The KWI for Biology is at top right-center.
Source: Archives of the Max Planck Society.

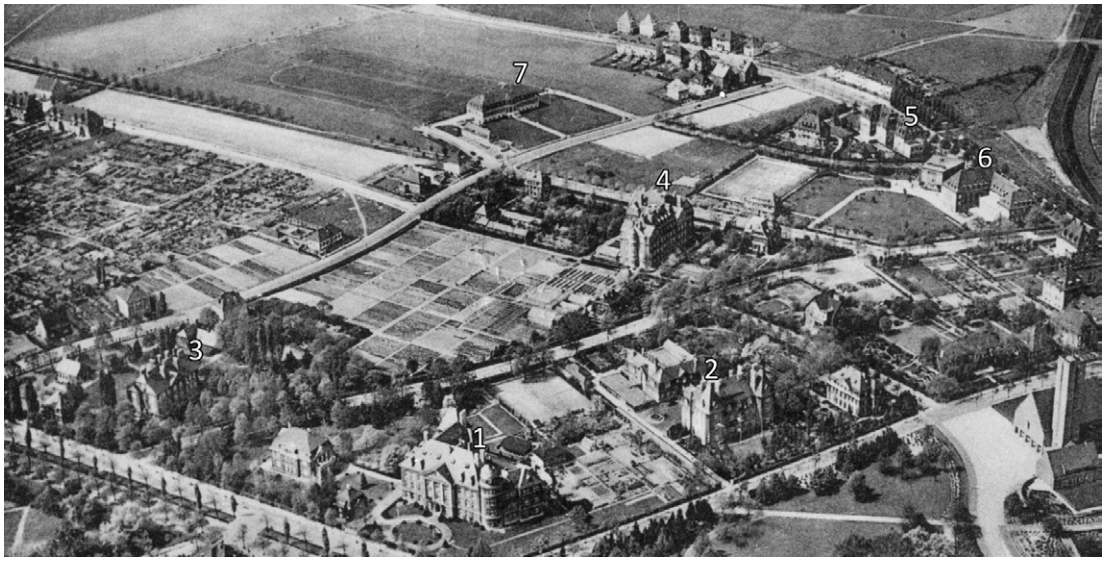

Fig. 2. An aerial photograph of the Dahlem research community after the construction of Warburg's institute for cell physiology (top center, no. 7); compare Fig. 1. Other institutes, numbered in chronological order include Chemistry (1), Physical Chemistry (2), Biochemistry (3), Biology (4), and Anthropology (5). The first joint building in Dahlem, the Harnack House (far right, 6), stood near the U-Bahn line to central Berlin, just visible on the right edge of the photo. It was opened in May 1929 as a conference center and guest house for visiting international scientists, with dining facilities for the entire community. In its Goethe-Saal, audiences of 500 people could attend lectures.[11]
Source: Archives of the Max Planck Society.

11 Bernhard vom Brocke: Die Kaiser-Wilhelm-Gesellschaft in der Weimarer Republik. In: Rudolf Vierhaus and Bernhard vom Brocke (eds.): *Forschung im Spannungsfeld von Politik und Gesellschaft. Geschichte und Struktur der Kaiser-Wilhelm-Max-Planck-Gesellschaft*. Stuttgart: Deutsche Verlags-Anstalt 1990, 197–355, 324–329.

1.2 Dahlem in the 1920s–1930s. An Ideal Site for Chemical-Biological Collaboration?

Following the war, work on the boundaries of chemistry and biology in Dahlem continued to develop along some of the lines foreseen by Emil Fischer, despite facing the well-known financial and political problems suffered by German scientific institutions in general. In 1924, following the end of hyperinflation, at Warburg's recommendation the KWG recruited the physiologist Otto Meyerhof (1884–1951), who was awarded the Nobel Prize for Physiology or Medicine in 1922 »for his discovery of the fixed relationship between the consumption of oxygen and the metabolism of lactic acid in the muscle.«[12] Meyerhof was appointed head of a section for general physiology within the KWI for Biology, and he became part of the community of scientists in Dahlem working on chemical and biological problems until leaving to head an institute in a new KWI in Heidelberg. The only lecture hall among the Dahlem institutes during the 1920s was in Haber's KWI for Physical Chemistry, where Haber hosted a weekly interdisciplinary seminar, the »Haber Colloquium«, that became a center of intellectual exchange. An important feature were the discussions following each presentation, at which (unusually for the hierarchical world of German academe) even lower-ranking assistants were encouraged to participate and question the speakers.[13]

The Great Depression followed by Hitler's coming to power in 1933 brought significant changes to Dahlem, with its disproportionate number of »non-Aryan« scientists, who were nearly all dismissed (with the exception of Warburg, who was apparently tolerated for his work on cancer). I will not discuss this in detail, as there is now a considerable amount of literature available.[14] After 1933, Neuberg, one of those of Jewish descent, lost his position and was ultimately forced into exile in the United States. His successor was Butenandt (previously at the Technische Hochschule in Danzig), who was appointed in 1936, simultaneously with his becoming a member of the NSDAP. Butenandt became one of the leading biochemists of his generation in Germany, best known for his pioneering work on sex hormones (which, in 1939, led to his being designated with Leopold Ruzicka (1887–1976) of Zurich as a Chemistry Nobel laureate – an

12 The Nobel Prize: Otto Meyerhof. Facts. https://www.nobelprize.org/prizes/medicine/1922/meyerhof/facts/. Last accessed 12/30/2021.

13 Fondly remembered by many younger scholars who were in Dahlem at the time, including Meyerhof's assistant David Nachmansohn: *German-Jewish Pioneers in Science 1900–1933. Highlights in Atomic Physics, Chemistry, and Biochemistry.* New York, NY: Springer 1979.

14 See in particular the works of Ute Deichmann: *Biologists under Hitler.* Cambridge, MA: Harvard University Press 1996, 206–250; Ute Deichmann: *Flüchten, Mitmachen, Vergessen. Chemiker und Biochemiker in der NS-Zeit.* Weinheim: Wiley-VCH 2001. There is also the series *Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus* (Göttingen: Wallstein Verlag), under the general editorship of Reinhard Rürup and Wolfgang Schieder. Of special interest for this paper are Wolfgang Schieder and Achim Trunk (eds.): *Adolf Butenandt und die Kaiser-Wilhelm-Gesellschaft. Wissenschaft, Industrie und Politik im ›Dritten Reich‹.* Göttingen: Wallstein Verlag 2004; Florian Schmaltz: *Kampfstoff-Forschung im Nationalsozialismus. Zur Kooperation von Kaiser-Wilhelm-Instituten, Militär und Industrie.* 2nd edition. Göttingen: Wallstein Verlag 2017; Michael Schüring: *Minervas verstoßene Kinder. Vertriebene Wissenschaftler und die Vergangenheitspolitik der Max-Planck-Gesellschaft.* Göttingen: Wallstein Verlag 2006. On the issue of cancer in Nazi culture see Robert N. Proctor: *The Nazi War on Cancer.* Princeton, NJ: Princeton University Press 1999.

honor he was forced to decline) and insect pheromones. In the process he developed an excellent relationship with industry, receiving support from 1927 on (when Butenandt was still the assistant of the bio-organic chemist Adolf Windaus (1876–1959) in Göttingen) from the Berlin pharmaceutical corporation Schering AG, which developed an extremely profitable business in sex hormones (and to a smaller extent in vitamins) as a direct outcome of its collaborative research with Butenandt's institute.[15] Butenandt's name (occasionally with those of his associates) was on at least thirty-eight patents signed over to Schering through 1945, and until the German defeat he received an annual royalty income from Schering of tens of thousands of Reichsmarks.[16] In Dahlem, which by the mid-1930s was still a relatively isolated scientific community with a handful of research institutes (Fig. 2), Butenandt developed a fruitful collaborative relationship between his institute and the KWI for Biology (not, however, with Warburg's KWI for Cell Physiology – Warburg was designated a »Halbjude« and remained largely isolated in the Dahlem of the late 1930s).[17] On the personal level, Butenandt collaborated with the zoologist Alfred Kühn (1885–1968), one of the directors of the KWI for Biology, on genetics and racial biology – a topic that integrated well with NS ideology, although Kühn did not become a party member.[18] On the institutional level, in 1937 the two institutes established an interdisciplinary research group for virus research, which was formalized as the Arbeitsstätte für Virusforschung der Kaiser-Wilhelm-Institute für Biochemie und Biologie. During the period before 1945, this probably came the closest to exemplifying Emil Fischer's 1915 vision of the interdisciplinary possibilities for Dahlem. In Butenandt's case this process was facilitated by a thorough renovation of his institute, which was a condition for his accepting the position and took about a year to complete. It should also be mentioned that both institutes benefited from significant increases in funding during the latter part of the NS period, which helped to enable this kind of research. Butenandt's annual budget included 100,000 RM from the KWG, supplemented to a great extent in Butenandt's case by support from industry, both Schering (as noted above) as well as the IG Farben concern, which supported both institutes. In fact, it appears that the impetus for the research group for virus research came from Heinrich Hörlein of the IG Farben Elberfeld pharmaceutical division (Bayer), which provided its initial funding.[19] During the war, addi-

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15 On Butenandt's collaboration with Schering see in particular Jean-Paul Gaudillière: Better Prepared than Synthesized. Adolf Butenandt, Schering AG and the Transformation of Sex Steroids into Drugs (1930–1946). *Studies in History and Philosophy of Biological and Biomedical Sciences* 36/4 (2005), 612–644; Jean-Paul Gaudillière: Biochemie und Industrie. Der »Arbeitskreis Butenandt-Schering« im Nationalsozialismus. In: Schieder and Trunk, *Butenandt*, 2004, 198–246. The archive of the Schering AG is now part of the larger corporate Bayer Archives. In this paper I will cite the Schering Archives in Berlin as SchA, and the Bayer Archives in Leverkusen as BAL; both contain extensive documentation and correspondence with Butenandt, supplementing the Butenandt Nachlass in the Archives of the Max-Planck-Gesellschaft, Berlin-Dahlem (AMPG), III. Abt., Rep. 84/1 and Rep. 84/2.

16 Gaudilière, Better prepared, 2005, 623; SchA–B1–264–001 Personalia-Butenandt: Patent Album.

17 Cf. Ute Deichmann: *Proteinforschung an Kaiser-Wilhelm-Instituten von 1930 bis 1950 im internationalen Vergleich.* Vol. 21. Berlin: Vorabdrucke des Forschungsprogramms »Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus« 2004, 30–31.

18 Cf. Hans-Jörg Rheinberger: Die Zusammenarbeit zwischen Adolf Butenandt und Alfred Kühn. In: Schieder and Trunk, *Butenandt*, 2004, 169–197; curiously enough, their collaboration did not produce any jointly authored papers.

19 See Butenandt-Hörlein correspondence (1937–1943) in AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 170–243.

tional support for the virus research group came from the German Industrial Bank (Deutsche Industriebank), as well as from the Reich Ministry of Education and the German Research Foundation (DFG). Including all sources, Butenandt's total budget amounted to at least 250,000 RM by 1944.[20]

1.3 The KWI for Medical Research in Heidelberg. A mini-Dahlem on the Neckar?

Many of the KWIs established outside Berlin before 1933 were focused on various branches of research related to industrial materials (iron, light metals, coal, leather, etc.), and thus primarily funded by industrial interests and located near the relevant industrial regions. The principal exception was the KWI for Medical Research, formally opened in Heidelberg in 1930. Its principal organizer was the physician Ludolf von Krehl (1861–1937), in whose Heidelberg university laboratory Warburg and Meyerhof had met before the war. Krehl had long been involved with the KWG, whose leaders including Fischer he knew well, and he may have been inspired by what he saw in Dahlem to create a kind of mini-Dahlem in Heidelberg: an interdisciplinary community of several institutes within a single building, to include physiology, chemistry, physics, and pathology. But in the absence of an independent funding source that might have promoted the institute, it was not until 1927 that the KWG agreed to proceed with the project. Krehl himself was to head an institute for pathology; for physiology he wanted Warburg, who, after reflection (and the prospect of his own KWI in Dahlem), recommended Meyerhof instead. Physics would be headed by Karl Wilhelm Hausser (1887–1933), who had also received a doctorate in Heidelberg and by 1927 led the medical physics laboratory of the Siemens & Halske electric manufacturing corporation in Berlin, where he studied questions such as the effect of light radiation on the skin.[21] For chemistry, Warburg proposed appointing a much younger but very promising bio-organic chemist, Willstätter's former student Richard Kuhn, then at the ETH in Zurich. As he wrote to Kuhn, »I have thought for several years that you would be the right man for a research institute. Willstätter said once that it would be too early for you, but that was a while ago, and perhaps now is exactly the right time.« He added that »if the institute goes as it is planned, the [directorial] positions will, in my view, be far better than a university professorship.«[22] This was certainly true in that the KWG guaranteed a substantial supplement in income to compensate the directors for the course fees they would otherwise earn as professors.

All the prospective directors met together in Heidelberg in July 1927 to discuss preliminary plans for the institute; Kuhn, Meyerhof, and Hausser had not previously been acquainted, but it appears that they got along very well. They continued to meet at times and to provide input to the architect (Hans Freese of Dresden) throughout the construction process in order to »max-

20 Deichmann, *Biologists*, 1996, 173, 210–211; H. H. Weber (Dekan, Med. Fak. Tübingen) to Hörlein, 8 Oct. 1948, BAL–363–274.

21 Cf. Richard Kuhn: Hausser, Karl Wilhelm. *Neue Deutsche Biographie*, 1969, 128. https://www.deutsche-biographie.de/pnd1018853561.html. Last accessed 2/27/2023.

22 Warburg to Kuhn, 11 July 1927, AMPG, III. Abt., Rep. 25, Nr. 38.

imize the scientific productivity of the laboratories«, so that the KWI became a joint creation that also satisfied the individual scientific needs of each prospective director. It was said that Kuhn even insisted on specifying the location of light switches.[23] The resulting building was in the shape of an H, with the crossbar containing the central library (an impressive internal open space with three banks of bookshelves), while each of the four wings housed one of the disciplinary institutes: chemistry and physiology on the north side, physics and pathology on the south side facing the Neckar River, near the bridge crossing over to the old city (Figs. 3 and 4 below). As one of the biochemists who worked there in the first year recalled, it was »one of the most beautiful research centers I have ever seen.«[24] The land (formerly an athletic field surrounded by orchards) was a gift from the city of Heidelberg, while the basic construction costs (780,000 RM, then equivalent to about $180,000) came from Baden's share of the so-called »Borderland Funds« allocated by the Reichstag, which apparently also covered the annual operating costs of 300,000 RM. The costs of internal furnishings came from the Social Insurance Agencies (Landesversicherungsanstalten) of Baden, supplemented by the KWG, the Notgemeinschaft (later DFG) and the state of Prussia. These also covered a substantial part of the apparatus. According to Kuhn, the total cost to build and equip the KWI was 1.68 million RM, a sum comparable to one of the largest university science institutes (e. g., Fischer's chemistry institute completed in Berlin in 1900). Krehl had successfully solicited additional support from the Rockefeller Foundation and from the IG Farben concern, whose CEO, Carl Bosch, had a villa in the area. IG Farben probably donated or supplied at a discount a considerable part of the chemicals used in the KWI. Even this much support was initially insufficient to completely furnish Krehl's wing (it was his choice to prioritize the facilities of the other directors, as he could use an office and work in the university).[25]

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23 David M. States: A History of the Kaiser Wilhelm Institute for Medical Research: 1929–1939. *The Nobel Prize*. https://www.nobelprize.org/prizes/themes/a-history-of-the-kaiser-wilhelm-institute-for-medical-research-1929-1939. Last accessed 4/10/2023. This is part of a detailed history of the institute under its initial directors; unfortunately, no sources are cited, so that it must be used with caution. Note that the embedded links in this webpage did not work when last accessed, but see the following: The Nobel Prize [David M. States]: The Foundation of the Kaiser Wilhelm Institute for Medical Research. https://www.nobelprize.org/prizes/themes/the-foundation-of-the-kaiser-wilhelm-institute-for-medical-research. Last accessed 4/10/2023.

24 Fritz A. Lipmann (1899–1986, Nobel Prize in Medicine or Physiology 1953), one of Meyerhof's co-workers in 1929–30, cited in Richard Kuhn: 25 Jahre Max-Planck-Institut für Medizinische Forschung in Heidelberg. *Mitteilungen aus der Max-Planck-Gesellschaft zur Förderung der Wissenschaften* Heft 2 (1955), 69–99, 99.

25 Cf. [States], Foundation; also Kuhn, 25 Jahre, 1955, 69–99, 76.

Fig. 3. Aerial view of the KWI for Medical Research in Heidelberg (undated photograph, c. 1931). The institutes for physics and pathology are at the east and west ends of the south wing, respectively, facing the Neckar River; chemistry and physiology are in corresponding ends of the north wing. The connecting wing houses the library, seminar rooms, a lecture hall, a dining hall (opening onto the terrace facing the river), and central administrative offices. The buildings to the left (west) include the stall building for experimental animals and an apartment building with accommodations for four institute officials and four married assistants, and guest rooms for visiting scientists (the directors were given housing subsidies for homes built nearby). Construction of the façades was primarily red brick to match the red sandstone prominent in the city of Heidelberg.[26]
Source: Archives of the Max Planck Society.

Fig. 4. The library of the KWI for Medical Research, 1930. It remains much the same today.
Source: Archives of the Max Planck Society.

26 Hans Freese: Das Kaiser-Wilhelm-Institut für medizinische Forschung Heidelberg. *Der Baumeister* 29/8 (Aug. 1931), 301–312.

Although the formal opening of the KWI was not until May 1930, the three younger directors all began work in Heidelberg in late 1929, bringing with them assistants and co-workers from their previous positions. To all appearances, the synergies in the new institute were extraordinary, and over the next decade they produced a tremendous amount of creative, interdisciplinary research. Kuhn's work on carotenoids and vitamins with several able collaborators, which also involved developing organic-analytical methodologies such as chromatography, led to his designation in 1939 as the sole winner of the 1938 Nobel Prize for Chemistry, an honor he (like Butenandt in the same year) was forced to decline. But he developed a highly profitable relationship with industry, particularly with the IG Farben (formerly Bayer) pharmaceutical division in Elberfeld. This was not unlike the case of Butenandt, and, by 1945, Kuhn's name (usually with collaborators) was on some sixty patents, most signed over to IG Farben and other firms.[27] Hausser and Kuhn also collaborated on several papers before Hausser's untimely death in 1933. Hausser's successor Walther Bothe (1891–1957) focused on nuclear physics (leading to a Nobel Prize in 1954) with little opportunity for collaborative research with the other institutes. But Hausser's widow Isolde (1889–1951), also trained in physics, subsequently carried on some of her husband's work in a small biophysics laboratory within the KWI.[28] Following up on work begun in Dahlem, Meyerhof's Physiology Institute was especially successful in working out the details of the multi-stage, enzyme-catalyzed metabolic processes associated with glycolysis, the most common form of which came to be designated the Embden–Meyerhof–Parnas (EMP) Pathway, which allows the cell to convert glucose into other substances and energy for work. A central achievement was to recognize the central importance in these metabolic processes of the compound ATP (adenosine triphosphate), isolated by Meyerhof's biochemistry associate Karl Lohmann in Dahlem in 1929. The significance of this work was such that some have designated it as a revolution in biology. Lohmann and several other biochemists who worked with Meyerhof during this period went on to distinguished careers. Four of them were awarded Nobel Prizes (Fritz Lipmann 1953, Severo Ochoa 1959, André Lwoff 1965, and George Wald 1967).[29]

There was, of course, a dark side to this, and it was no coincidence that none of the Nobel laureates just mentioned remained in Germany when they received their awards. The advent of the National Socialist regime in 1933 put increasing political pressure on the Heidelberg KWI. Meyerhof and many of his collaborators were »non-Aryans«, as were many of Kuhn's co-workers (though not Kuhn himself). Kuhn, an Austrian citizen from Vienna and married to a Swiss woman, should have, in principle, been able to resist the pressure in the early stages, and indeed he never joined the NSDAP while continuing to honor his mentor Willstätter, a Jew who had resigned his professorship in protest against the anti-Semitic appointment decisions of his

27 I thank Thomas Steinhauser for providing me with a list from the European Patent Office database (Patente Richard Kuhn gesamt-from Th.St.25Okt17.xls).

28 Margot Fuchs: Isolde Hausser (7. 12. 1889–5. 10. 1951), Technische Physikerin und Wissenschaftlerin am Kaiser-Wilhelm-/Max-Planck-Institut für Medizinische Forschung, Heidelberg. *Berichte zur Wissenschaftsgeschichte* 17/3 (1994), 201–215. doi:10.1002/bewi.19940170309.

29 [States], Foundation; Deichmann, *Flüchten*, 2001, 278–281.

Munich faculty. Willstätter lived in retirement in Munich until the aftermath of *Kristallnacht* in 1938 made it clear to him that he must emigrate. He managed to obtain a visa to enter Switzerland (where he had taught before coming to the KWG before the First World War) in the spring of 1939, with the help of his former assistant Arthur Stoll, then director of research for the Sandoz pharmaceutical corporation in Basel.[30]

By this time, of course, Kuhn was a German citizen as a result of the annexation of Austria, and he had also become president of the German Chemical Society in 1938. Although he maintained friendly contact with Willstätter through his wife and her family, it does not appear that he was willing to jeopardize his position by helping Willstätter directly (in particular by helping him with the often-frustrating and depressing process of emigration). In contrast, Kuhn regularly took appropriate steps to confirm his loyalty to the new rulers of the Reich. Indeed, Kuhn had dismissed his Jewish co-workers in 1933, and in 1936 he secretly denounced Meyerhof for sheltering other Jewish associates.[31] Following Krehl's death in 1937, Kuhn was entrusted indefinitely with the role of managing director of the KWI. In its original statutes the KWI's administration had been collegial with each of the directors assuming that role in rotation, but after 1933 the system broke down, as it was unacceptable to the Nazis for Meyerhof to hold such a position. Meyerhof realized that he could not continue, and he fled to France via Switzerland in 1938, only to have to flee to the United States via Spain two years later. Ultimately he took a position in the Medical College of the University of Pennsylvania, where he continued to be productive, albeit with little support. His abrupt, »illegal« departure from the Reich had forced him to leave behind all of his possessions in Heidelberg, including a priceless personal library with first editions of Newton and other scientific classics. Kuhn made no effort to keep this legacy intact by integrating it into the institute library; instead, it was auctioned off and dispersed into unknown hands. In contrast, with Stoll's help, Willstätter had successfully donated his personal library to the German affiliate of Sandoz prior to his emigration, and he would surely have been able to reclaim it had he survived the war.[32]

With Meyerhof gone, Kuhn closed the Institute for Physiology, having worked to sabotage negotiations to appoint as a successor the physiologist Hermann Rein from Göttingen, who had just become director of the Aviation Medicine Research Institute under Göring's Aviation Ministry.[33] Kuhn now designated the space as an Institute for Biology. In implicit competition

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30 See Willstätter–Stoll correspondence from 1938–1939 in: ETH-Bibliothek, Hochschularchiv, Hs 1426a, Willstätter (1872–1942) – Arthur Stoll 1887–1971. Manuskripte, Korrespondenz und biographisches Material aus dem Besitz von Werner Stoll.

31 Deichmann, *Flüchten*, 2001, 75–76; Schmaltz, *Kampfstoff-Forschung*, 2017, 368–369.

32 Willstätter-Sandoz and Stoll-Willstätter correspondence from 1938–1940, esp. Willstätter to Direktion der Sandoz AG, Basel, 30 Nov. 1940, ETH-Bibliothek, Hochschularchiv, Hs 1426a.

33 Once it became clear that Rein's intention was to expand the institute for physiology while converting it into an expensive laboratory for aviation medicine that would have absorbed resources Kuhn wanted for his own research, Kuhn himself helped to sabotage Rein's appointment, with the cooperation of the KWG's General Secretary Ernst Telschow and the Reich Education Ministry's Rudolf Mentzel. Schmaltz, *Kampfstoff-Forschung*, 2017, 416–420. For more on Rein,

with the genetics work of Butenandt and Kühn in Dahlem, Kuhn believed that he had found the ideal scientific collaborator to develop a revolutionary approach to biochemical genetics based on the analysis of microorganisms such as algae. The new man was Franz Moewus (1908–1959), who had worked on the sexuality and evolutionary development of one-celled organisms in the laboratory of Max Hartmann, one of the directors in the KWI for Biology in Dahlem. Moewus had no prospects for advancement at Dahlem, so in 1937 he contacted various industrial firms in the hope of getting support or a research position. By the fortuitous circumstance that carotenoid pigments appeared to be part of the sexual substances he was examining, the pharmaceutical department of IG Farben recommended that Moewus work with Richard Kuhn, for whom the carotenoids were a central focus of his vitamin studies. Kuhn was evidently impressed by Moewus and welcomed him as a collaborator, initially within the Institute for Chemistry. They proceeded to publish six joint papers between 1938 and 1944. Moewus never became a scientific member of the KWI, however, let alone director of the Institute for Biology, which Kuhn kept under his personal control. Kuhn provided Moewus with laboratory space and support for his work, which he conducted together with his wife Liselotte. As Otto Dann, one of Kuhn's associates in the chemistry institute recalled, Kuhn liked to inspire his associates to work on a problem, then leave them alone »to go their own way, because he took the view that if someone did a thorough job on a problem, something would always come out of it.« As a result he gave his associates a great deal of leeway in choosing their methods, approaches, and work schedules. Moewus's apparently revolutionary results and particularly their connection to the carotenoids excited Kuhn, who was »very ambitious«.[34] And with good reason. Moewus appeared to be laying the foundations of a new science, biochemical genetics, and he appeared to have demonstrated how genes controlled the development of cells through enzymes, to the extent that he proposed a one gene-one enzyme hypothesis similar to that later proposed (in 1941) and subsequently confirmed experimentally by George Beadle and Edward Tatum, for which they were awarded the Nobel Prize in 1958. Had his own earlier results been confirmed, Moewus (and perhaps his wife Liselotte) could have had a prior claim to a Nobel Prize (after the war, of course).[35] As institute director and their chief biochemical collaborator, Richard Kuhn must also have dreamed of taking at least a one-third share of such a Nobel Prize, which would have been his second.

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including his later appointment to the MPI in 1951 (which failed due to his death), see Katharina Trittel: *Hermann Rein und die Flugmedizin. Erkenntnisstreben und Entgrenzung*. Paderborn: Ferdinand Schöningh 2018, 167–168, 398–400. I thank Florian Schmaltz for calling my attention to this. Ironically, as will be discussed later, in 1945 the American military occupation temporarily accomplished Rein's original goal by converting half of the MPI into a laboratory for aviation medicine in which ex-Luftwaffe officers could complete their wartime research. Robert J. Benford: Report from Heidelberg. The Story of the Army Air Forces Aero Medical Center in Germany. 1945–1947, 1947. https://collections.nlm.nih.gov/ext/dw/14130150R/PDF/14130150R.pdf. Last accessed 12/17/2022.

34 Deichmann, *Flüchten*, 2001, 330.

35 For a sympathetic account heavily based on the recollections of Liselotte Moewus-Kobb (Franz's widow, who subsequently remarried), see Jan Sapp: *Where the Truth Lies. Franz Moewus And The Origins Of Molecular Biology*. Cambridge: Cambridge University Press 1990; for the other side see Deichmann, *Flüchten*, 2001; Eberhard Schnepf: Fälschungen – nicht nur in unserer Zeit: Lug und Trug in der Biologie. *Biologie in unserer Zeit* 32/3 (2002), 160–165.

Kuhn's ambitions for biological revolution are reflected in a humorous album of photos, caricatures, and poetic narrative in a mock-epic style given to him by his institute members on his fortieth birthday in December 1940. The album, which also reflects German optimism at the end of a year of triumphs in Western Europe, purports to reveal the top-secret research going on in Kuhn's institute. »We are working on the synthesis of a human being in a test-tube«, announces one of Kuhn's associates, Leonhard Birkofer, at the beginning of the poem. Two pages later Franz Moewus appears with a beaker in each hand, labelled male and female, from which beings of each gender are to be prepared. And so on, with each member of the institute staff playing a role. There are even memos circulated to the staff by Kuhn, forbidding them from disturbing the »synthetic *Volksgenossen*« or spending the night with »artificially constructed women«. Of course, ultimately it is revealed that everything has been simply a dream of Richard Kuhn, who has dozed off at his workbench.[36]

Kuhn's above-mentioned associate Birkofer noticed however that Moewus and his wife never seemed to have an unsuccessful experiment. They worked alone, often during weekends when no one was in the institute. Kuhn's co-workers began to wonder why nothing ever seemed to go wrong, »But Richard Kuhn was so excited that he never noticed that something wasn't quite right.« When the Heidelberg professor of botany, August Seybold, expressed some doubts about Moewus's too perfect results, a couple of other co-workers attempted but failed to reproduce Moewus's experiments. Instead of doubting Moewus, Kuhn criticized the others for not correctly following Moewus's methodology. But that would have meant something rather different from what Kuhn expected. As Birkofer recalled, »The affair was especially unpleasant for [Kuhn's co-worker] Irmentraut Löw, who had synthesized the compounds for Moewus that were supposed to be the control substances to be compared against the compounds that Moewus was isolating from his algae. He, however, had been putting those compounds into the experimental substances and then giving them to Löw for isolation.« As it turned out, no one was able to independently reproduce Moewus's results, and the controversy over them would continue into the mid-1950s before a consensus finally emerged that wrote Moewus out of the history of genetics. Kuhn had bet his reputation on a scientific fraud, but he never explicitly acknowledged his error.[37]

Based on the available evidence, it appears that Kuhn had not consciously engaged in fraud in his publications with Moewus, because he genuinely believed that Moewus was initiating a

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36 Kaiser Wilhelm Institut Heidelberg 1940 [40th Birthday Album], AMPG, III. Abt., Rep. 25, Nr. 47.

37 Deichmann, *Flüchten*, 2001, 330–331. Sapp, *Where the Truth Lies*, 1990, in effect tries to confuse the issue through a »social constructivist« approach to the question of scientific fraud, which although allegedly quite common, becomes an issue only when it is »constructed« as a product of a social-political context usually excluded from scientific discussions (his first chapter is entitled, »Is Science Fiction?«). But Sapp himself constructed an analysis that omitted critical sources while overly relying on an interested witness, Liselotte Moewus-Kobb. During his research in the 1980s, Sapp interviewed many scientists involved in the controversy, but evidently not Dann, Birkofer, or any other of Kuhn's former associates who were still alive to be interviewed by Deichmann in the 1990s.

new era in biochemical genetics, a belief he seems to have held at least until 1950.[38] Strong evidence for this belief comes from a lecture that Kuhn presented at the BASF Ludwigshafen main works in December 1948, in which his approach makes for an interesting contrast with that of Emil Fischer in 1915. Where Fischer had likened the cell to a chemical laboratory, Kuhn likened it to a factory of the chemical industry, whose principal components had been revealed in broad outline but with many details yet unknown, as a wartime factory's main components might be revealed in outline by aerial reconnaissance. Moreover, existing biochemical tools could only uncover the details of the internal reactions of a cell by destroying it, leaving many aspects of normal cell metabolism and biosynthesis just as uncertain as the details of normal production processes in a bombed-out factory whose production plans had been seriously damaged. Here Kuhn clearly knew his audience, the staff of one of the most heavily bombed chemical works in Germany, which was now subject to inspection by the French occupation authorities who were, of course, engaged in industrial espionage. And in regard to his central problem of genetics, Kuhn, like Fischer before him, addressed the question of mutations as chemical changes in a gene whose chemical nature was still unknown. But for Kuhn, the analogy of bombing a factory also applied to chemically induced genetic mutations. If one bombed a factory in a specific part of the plant and shut down the manufacture of a specific product, leaving a growing mass of unused intermediates, this would reveal something about the otherwise unknown apparatus and processes in that location, just as damaging a particular location in a chromosome (whose chemical nature was still largely unknown) would cause a specific mutation and an abnormal excess of certain substances, from which one could infer the reactions regulated by a specific gene at that location. In short, each gene controlled one stage in the production process of a cellular factory. Finally, just as Fischer had emphasized the necessity of an interdisciplinary approach, Kuhn did the same, but now including physics, which provided new tools for analysis such as radioactive isotopes, along with biology and chemistry. Among his examples were the collaboration of the British organic chemist Robert Robinson with botanists in the study of the genetics of plant pigments, the anthocyanins; and the collaboration of the biochemist Adolf Butenandt with the biologist Alfred Kühn (both of whose institutes had been evacuated to Tübingen at the end of the war) in the study of the genetics of insect eye pigments. A next step, which Kuhn left undiscussed probably because of its resonance with the defeated ideology of National Socialism, could be to clarify the genetic expression of pigmentation in human races. Instead, Kuhn moved in the opposite direction of magnification, from insects to algae, discussing in considerable detail the work of Moewus on the biochemical genetics of sexuality in *chlamydomonas* and highlighting the determining role of two groups of bio-organic

38 In his »cealing« [sic=ceiling, i. e. maximum] request for research staff to be connected with his professorship at the University of Pennsylvania in 1951, Kuhn proposed adding sections for protein chemistry and biochemical genetics, the latter to be headed by an unnamed professor who could only have been Moewus. Undated list [internal evidence showing it predated 1951] in Kuhn's handwriting, AMPG, III. Abt., Rep. 25, Nr. 321. Cf. undated letter (ca. summer 1949) of Moewus to Kuhn, expressing frustration that he was unable to improve his position at the MPI or to obtain a professorship in Heidelberg, AMPG, III. Abt., Rep. 25, Nr. 237. In 1950–1951 Moewus continued to defend himself to Kuhn and to others in the Heidelberg faculty for natural sciences against Seybold's criticism. Moewus apparently still believed that research by foreign scientists, particularly Americans, would vindicate him: Franz Moewus to Prof. Ludwig, Dekan der Naturw.-math. Fakultät, Heidelberg (9 Feb. 1951), UAH, K-VIII/3-17/9, fol. 284–285 (cc of copy), citing letter from Moewus to Kuhn, 24 April 1950 (not in the file).

chemicals, the carotenoids and the flavanols. Kuhn's associate Irmentraut Löw had been able to isolate these in beautifully crystallized form from Moewus's mutated strains of the algae, in an apparent demonstration of the chemistry of the factors determining sexuality. In conclusion, Kuhn emphasized the potential industrial applications of the new biochemical genetics for the production of organic fertilizers, pesticides, and pharmaceuticals.[39] Like Fischer a generation earlier, in the late 1940s Kuhn foresaw the emergence of a kind of genetic engineering, but with Moewus's (and his own and Löw's) work as its foundation.

As the evidence against Moewus's alleged discoveries and the implications of fraud grew in the 1950s, however, threatening to undermine Kuhn's credibility as a scientific leader, he distanced himself from Moewus and his research. Following the latter's death in 1959, Kuhn published a brief paper with Löw that took no position on Moewus's strictly biological arguments while acknowledging that some of the chemical substances that Moewus had claimed to have found in his algae strains could not in fact be isolated from algae cultivated in other laboratories. Nevertheless, they continued to assert the validity of the purely chemical parts of the research, i. e., the processes of isolation, crystallization, and structural analysis of the various chemical products, processes that in themselves were not particularly pathbreaking. It would seem that Kuhn was tacitly admitting fraud on the biological side of the case, but disclaiming responsibility by restricting his and Löw's roles to pure chemistry, a position that could hardly have made a favorable impression on his disciplinary colleagues.[40] In the aftermath of the war, this was only one of many examples of German scientists dodging responsibility.

1.4 Dahlem and Heidelberg in the War. Mobilization of Scientists and Impact of Secret Research

The advent of war in 1939 affected both Dahlem and Heidelberg, where Butenandt and Kuhn each undertook secret research projects. Afterwards they would justify these in part on the grounds that it allowed them to free their younger colleagues from military service and thus keep them alive to revive German science in the postwar era. While this is certainly true as far as it goes, it was also part of a deliberately constructed myth that avoided the question of how far they actually believed in and actively supported the German war effort at the time. A related question was the extent to which they as German scientists also accepted the ideology of National Socialism, as reflected in some of their rhetoric or active participation on ceremonial occasions (Kuhn, for example, marched in May Day parades led by Storm Troopers in uniform, he signed his letters with »Heil Hitler«, and he used Nazi-style rhetoric in his speeches as president of the German Chemical Society). Both Butenandt and Kuhn went to considerable lengths to conceal or play down what they had actually done, including the destruction of possibly

39 Richard Kuhn: Über einige Probleme der biochemischen Genetik. *Angewandte Chemie* 61/1 (1949), 1–5.

40 Deichmann, *Flüchten*, 2001, 335–336; Sapp, *Where the Truth Lies*, 1990, 284–285.

incriminating documents. Butenandt specified that his papers would not be open to researchers until 2025, but the MPG afterwards decided to override this restriction for scholars involved in MPG-sponsored research groups.[41]

The wartime work of Butenandt's institute and its potential connections to NS atrocities subsequently received critical scholarly analysis, at times speculative due to the disappearance of potentially incriminating secret wartime documents, in part destroyed at Butenandt's orders, others possibly confiscated by the French occupation authorities after the war.[42] Of particular interest were aeromedical research on blood chemistry supported by the Luftwaffe, including effects of cold and low pressure on test animals. This suggests that despite his protests to the contrary in his postwar testimony during the Nuremberg trial of Heinrich Hörlein of Bayer,[43] Butenandt was probably aware of if not directly involved with the similar, and lethal, experiments by SS doctors on human prisoners at the Dachau concentration camp. Butenandt's predoctoral assistant Günther Hillmann, whom he left in charge of the Dahlem institute after nearly everyone else had moved to Tübingen in late 1944, evidently stayed in Berlin because of a collaborative project with the KWI for Anthropology. Hillmann was analyzing blood samples of Auschwitz victims sent to Otmar von Verschuer, the institute's director, by his former student, the infamous SS doctor Josef Mengele.

Florian Schmaltz has presented the most complete discussion we are likely to have of the origins of nerve agents in wartime Nazi Germany, at least until the recovery of documents presumably confiscated by the Red Army in 1945. Of particular interest is his critical analysis of the origins of soman, as recalled immediately after the war by Richard Kuhn and four decades later by his associate Günter Quadbeck.[44] This is worth discussing in greater detail here, because it illustrates Kuhn's creative approach even to weapons research. According to Quadbeck, in early 1944, Richard Kuhn was looking for an alternate structure to sarin (Fig. 5 [see below for

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41 Including, of course, the group on the KWG under National Socialism and the Research Group on the History of the MPG (to which the current paper is a contribution). During my research for this paper, the Nachlass Richard Kuhn in the Archives of the Max Planck Society remained partially restricted by the Kuhn family, represented by Kuhn's son Professor Hans-Jürg Kuhn, who said that he was editing the correspondence of Richard and his wife Daisy and therefore could not release them for scholarly use, which is significant because Richard Kuhn was a person who was otherwise extremely cautious in expressing himself in letters. Professor Kuhn was kind enough to allow me to see Kuhn's postwar letters to his wife. There is now an extensive literature on this general topic; of particular interest for this paper are Deichmann, *Flüchten*, 2001 (and several other works by her); Schieder and Trunk, *Butenandt*, 2004; Schmaltz, *Kampfstoff-Forschung*, 2017.

42 Overview in Bernd Gausemeier: An der Heimatfront. »Kriegswichtige« Forschungen am Kaiser-Wilhelm-Institut für Biochemie. In: Schieder and Trunk, *Butenandt*, 2004, 134–168, 134–136. French archival documents on German scientific and educational institutions in the French zone of occupation were being reorganized and newly indexed when I visited the Centre des Archives diplomatiques (CAD) at La Courneuve outside Paris in 2019. As a result, many of the documents relevant to Butenandt's institute in Tübingen were probably not yet available; in any case, in two visits I found very little that even mentioned Butenandt, and certainly nothing related to secret wartime research. Confiscated documents might well be found elsewhere, e. g. in the military archives, which I did not visit. The finding aid listed two potentially relevant reports on Butenandt's institute, in CAD 1AAA-27-OMS: Rapports scientifiques, Nr. 00438 (by Fauche, 31 March 1950) and Nr. 01125 (by Nguyen Dang Tan, 12 Sept. 1950). Neither report was in the indicated location.

43 Gausemeier, Heimatfront, 2004, 134–168, 166–167.

44 Schmaltz, *Kampfstoff-Forschung*, 2017, 486–590.

Figs. 5–8]), which would contain the same deadly phosphate-fluorine complex but otherwise would be more homologous to choline (Fig. 6). After seeing a presentation that used space-filling models (Kalottenmodelle), Kuhn was struck by the similarity of the nitrogen-based tri-methyl complex in choline (Fig. 6) to the carbon-based tri-methyl complex in pinacoline [pinacolyl] alcohol (3,3-dimethyl-2-butanol) [also known as: *tert*-butyl methyl carbinol] (Fig. 8). Substituting the pinacoline complex for the isopropyl complex in sarin would produce soman (Fig. 7). But how did Kuhn get the idea to do this, and when? Until now, the absence of contemporary documents has made it impossible to date the discovery more precisely than »early spring of 1944«, which has been interpreted as March.[45] But I believe that Kuhn's insight came in late January 1944, for the following reasons.

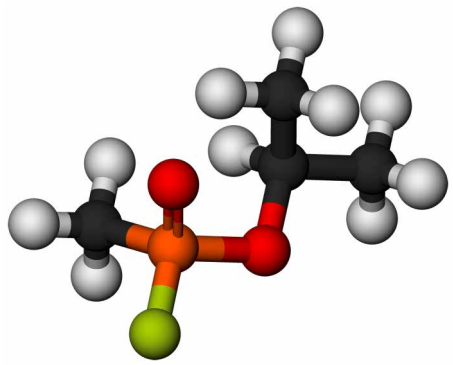


Fig. 5: Sarin chemical structure

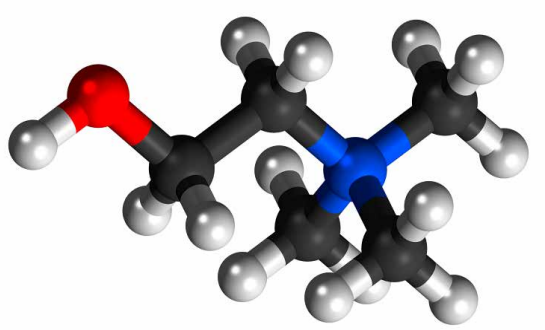


Fig. 6: Choline chemical structure

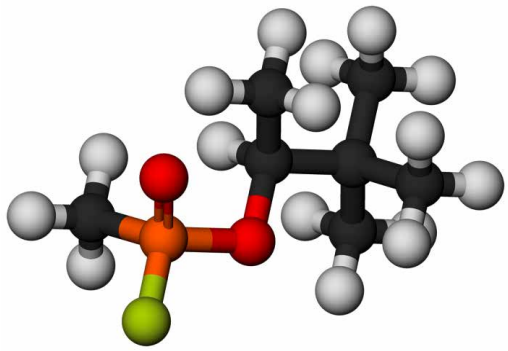


Fig. 7: Soman chemical structure

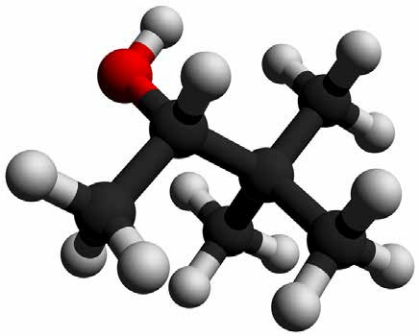
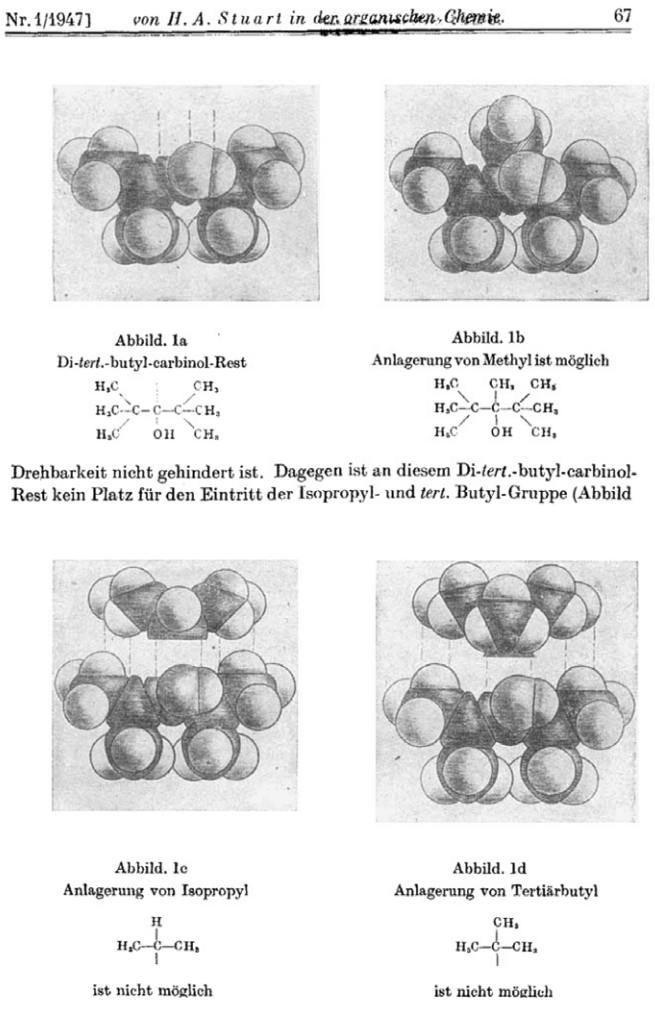


Fig. 8: Pinacoline alcohol structure[46]

45 Schmaltz, *Kampfstoff-Forschung*, 2017, 488, 579.

46 Sources for Figs. 5–8: illustrations in Wikimedia Commons entries for each compound, as shown below. All CC0 1.0 (public domain). Orientation of the models has been changed in some cases for better comparisons: https://commons.wikimedia.org/wiki/File:Sarin-3D-balls-by-AHRLS-2012.png (5), https://commons.wikimedia.org/wiki/File:Choline-cation-3D-balls.png (6), https://commons.wikimedia.org/wiki/File:Soman-3D-balls-by-AHRLS-2011.png (7), https://commons.wikimedia.org/wiki/File:3,3-dimethyl-2-butanol-3D-balls-by-AHRLS-2012.png (8). All last accessed 30 Dec. 2022).

Quadbeck recalled that Kuhn had been inspired by a presentation to the Heidelberg Chemical Society by a »female associate of W. Reppe, then the head of the BASF Main Laboratory in Ludwigshafen, on branched alcohols synthesized via acetylene chemistry. In this talk she also demonstrated the structure of pinacoline (pinacolyl) alcohol in a space-filling model.«[47] After four decades, Quadbeck understandably may have misidentified the speaker. On Jan. 20, 1944, Dr. Emma Wolffhardt of the IG Farben (from 1952: BASF) Ammonia Laboratory in Oppau presented a talk on space-filling models in organic synthesis, which was submitted to the *Berichte* in November 1944 but not published until 1947.[48] She included an illustration of a space-filling molecular model for a di-tert.-butyl-carbinol complex, to which one could easily add a methyl group, producing structurally a doubled form of pinacoline alcohol (*tert*-butyl methyl carbinol), but to which one could not add larger structures (such as an isopropyl group) (Fig. 9 below).


Nr. 1/1947] von H. A. Stuart in der organischen Chemie. 67

Abbild. 1a
Di-*tert.*-butyl-carbinol-Rest

Abbild. 1b
Anlagerung von Methyl ist möglich

Drehbarkeit nicht gehindert ist. Dagegen ist an diesem Di-*tert.*-butyl-carbinol-Rest kein Platz für den Eintritt der Isopropyl- und *tert.* Butyl-Gruppe (Abbild

Abbild. 1c
Anlagerung von Isopropyl
ist nicht möglich

Abbild. 1d
Anlagerung von Tertiärbutyl
ist nicht möglich

Fig. 9. Wolffhardt's illustrations of di-*tert.*-butyl-carbinol structures, demonstrating the difference between the classical two-dimensional structural formulas and modern three-dimensional structural models.
Source: E. Wolffhardt: Beiträge zur Verwendung der Atommodelle von H. A. Stuart in der organischen Chemie. *Chemische Berichte* 80 (1947), 64–76, 67. Copyright Wiley-VCH GmbH. Reproduced with permission.

47 Günter Quadbeck: Richard Kuhn (1900–1967). In: Wilhelm Doerr (ed.): *Semper Apertus. Sechshundert Jahre Ruprecht-Karls-Universität 1386–1986. Festschrift in sechs Bänden. Vol. 3: Das zwanzigste Jahrhundert, 1918–1985*. Berlin: Springer 1985, 55–77, 68; cited by Schmaltz, *Kampfstoff-Forschung*, 2017, 489.

48 Emma Wolffhardt: Beiträge zur Verwendung der Atommodelle von H. A. Stuart in der organischen Chemie. *Chemische Berichte* 80/1 (1947), 64–76. doi:https://doi.org/10.1002/cber.19470800113.

Quadbeck emphasizes that Kuhn was struck by the »large spatial similarity« between the structures of choline and pinacoline alcohol, each with three methyl groups around a central atom (nitrogen and carbon, respectively). The illustrations above also suggest that Wolffhardt's models brought out to Kuhn the contrast between the pinacoline alcohol and isopropyl structures. These structural insights in turn led Kuhn »on the next morning« (which, if Wolffhardt's talk was the inspiration, would have been on January 21, 1944) to assign to his assistant Konrad Henkel the task of substituting a pinacoline alcohol complex for the isopropyl alcohol complex in sarin.[49] Thus Henkel first synthesized soman (Fig. 7), but Quadbeck oddly fails to mention that name, or to point out that it was the deadliest form of nerve agent developed in Germany during the war. Instead, Quadbeck describes Henkel's results in technical terms that a lay reader would be highly unlikely to understand: »Thus was found the strongest competitive inhibitor for acetylcholinesterase.« As if to distract a more informed reader from thinking about military implications, Quadbeck immediately changes the subject to the long-term scientific value of Kuhn's »spatial vision,« which »was a decisive factor in Richard Kuhn's great successes in his later work on the chemistry of the oligosaccharides.«[50] This is an interesting point, because Wolffhardt herself believed that »one had to move away from paper [i. e. classical two-dimensional models] and start thinking in space,« which was a problem for too many organic chemists who relied too much on »paper tools« in the form of traditional two-dimensional structural formulas of classical organic chemistry.[51] I doubt that she ever suspected that her lecture had led directly to the invention of a deadly nerve agent.

Could there have been another woman chemist in Reppe's laboratory who also gave a talk in the spring of 1944 using space-filling models to illustrate the branch structures of alcohols? Perhaps, but this is unlikely. Working solely in the Ammonia Laboratory, Wolffhardt can be credited as the principal innovator in the use of space-filling molecular models for industrial synthetic research, and her laboratory chiefs accordingly tasked her with lecturing on this topic both in the Heidelberg Chemical Society, and at the IG Farben Oppau/Ludwigshafen and Leuna Works.[52] She was careful in her Heidelberg paper not to mention that her own use of space-filling models had arisen out of military-oriented research on the synthesis of aviation fuel, which was evidently a secret project. Hence, it may have been convenient for her not to be too explicit about what she had originally been doing when she first used the molecular models, and a reference to Reppe might thus have served as a cover. Wolffhardt had certainly discussed her methods with Reppe prior to her talk in January 1944, and recalled that he had initially been

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49 Quadbeck, Kuhn, 1985, 55–77, 68; Schmaltz, *Kampfstoff-Forschung*, 2017, 489–490.

50 Quadbeck, Kuhn, 1985, 55–77, 68.

51 Jeffrey A. Johnson: Notes to interview with Dr. Emma Wolffhardt (Ludwigshafen, 25 June 1993) (complete transcript not available). Cf. Ursula Klein: *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century*. Stanford, CA: Stanford University Press 2003.

52 Jeffrey Allan Johnson: The Case of the Missing German Quantum Chemists: On Molecular Models, Mobilization, and the Paradoxes of Modernizing Chemistry in Nazi Germany. *Historical Studies in the Natural Sciences* 43/4 (2013), 391–452, 400. doi:10.1525/hsns.2013.43.4.391; Johnson, Wolffhardt interview, 1993.

very skeptical. But she recalled that he was present for at least one of her talks, which would suggest that he was at the Heidelberg lecture. She was, moreover, surprised that after this lecture, Reppe defended her use of space-filling models when criticized by a physical chemist,[53] which could account for Quadbeck's incorrect attribution forty years later. More important is the fact that Quadbeck recalled Kuhn being inspired by spatial modelling, which was quite unusual among German organic chemists at the time. Indeed, this was the whole point of Wolffhardt's lectures: to encourage German organic chemists to think in three dimensions,[54] which Kuhn grasped and immediately applied to his own research, first on nerve agents and later in his postwar projects.

53 Johnson, Wolffhardt interview, 1993.

54 Emma Wolffhardt: Entwurf für den Vortrag in Leuna am 11. Juli [hand-corrected from 13. Juni] 1944, »Zusammenhänge zwischen Molekülbau und dem Verlauf einiger organischer Reaktionen«, 1–2, 18. Incomplete copy in BASF UA PB W.1.3./207: Unterlagen von Dr. Emma Wolffhardt; Johnson, Wolffhardt interview, 1993.

2 From KWG to MPG in the Aftermath of War

2.1 Another New Dahlem on the Neckar? Butenandt in Tübingen and Kuhn in Heidelberg under Allied occupation

As the war came to an end in 1945, Adolf Butenandt and Richard Kuhn were the most influential biochemists in the KWG. Along with the physicist Werner Heisenberg and the nuclear chemist Otto Hahn (who became the first president of the successor organization, the Max Planck Society, the MPG), they played a critical role in the transition from the KWG to its continuation as MPG (carrying over from the Kaiser-Wilhelm-Gesellschaft (KWG)). Butenandt continued to lead the former KWI for Biochemistry in Dahlem, which moved to Tübingen during the war (where it subsequently became an MPI) and then to Munich in 1955–1956. Aside from some extended research visits to the USA, Richard Kuhn remained at the former KWI (now MPI) for Medical Research in Heidelberg, which he led until his death in 1967. Both institutes had to go through difficult periods in the immediate postwar era under Allied occupation, during which both directors seriously considered accepting positions in foreign institutions. Butenandt's decision not to accept a position in Basel in 1949 will be discussed later. Although Kuhn spent several semesters in the early 1950s as a visiting research professor in the School of Medicine at the University of Pennsylvania (henceforth: Penn) in Philadelphia, as will be discussed below, he ultimately chose to decline Penn's offer of a tenured professorship and remained in Heidelberg to lead his institute into the greater prosperity of the mid-1960s.

The occupation and Allied science policy affected both Butenandt and Kuhn's institutes, albeit not to the same extent. At the end of the war, Heidelberg was in the American zone and Tübingen in the French zone. The Americans confiscated half of Kuhn's institute, which had survived essentially intact as a result of the lack of bombing in Heidelberg.[55] Tübingen was similarly untouched, but Butenandt too lost facilities and apparatus to confiscation by the French (mainly equipment in other university institutes, such as an electron microscope from the hygiene institute). Butenandt was able to limit confiscations in his own institute by appealing to the American liaison officer in Heidelberg, pointing out that before the war he had purchased much of his prewar equipment with the help of grants received from the Rockefeller Foundation. He thereby persuaded the American officer to declare the institute to be within the American sphere of influence and to facilitate the issuance of a written notice from the French commandant that there would be no interference in the institute without the personal consent of the commandant and the American liaison officer.[56]

Kuhn and Butenandt both recognized the need to adapt to the new situation by cooperating with the occupation authorities. At the same time, they each felt the need to conceal aspects of

55 Correspondence and documents in AMPG, III. Abt., Rep. 25, Nr. 396.

56 Butenandt to Telschow, 4 June 1945, AMPG, II. Abt., Rep. 66, Nr. 619, fol. 493–495 verso.

their past before 1945. As mentioned above, Kuhn had not been a Nazi party member, but he had done top-secret war work, including the development of soman, the most lethal nerve agent that the Germans produced during the war. As Schmaltz has shown, Kuhn initially (in April 1945, before the German surrender) tried to conceal his involvement in chemical warfare work, claiming that he knew nothing about the German program.[57] Kuhn asserted further that only second-rate Nazi scientists did war work.[58] Kuhn thereby helped to contribute to a myth that was commonly promoted by German scientists during the postwar decades, and which for a time gained credence among western scientists as well. Only several months later, when confronted with evidence of his nerve-agent research, did Kuhn more openly discuss with the Americans what he had done. That information did not become common knowledge for some time, however, as the American and British military authorities had an interest in concealing the embarrassing fact that they had not only been unable to develop any agents comparable to the German nerve agents, but were not even aware of their existence. Although the Germans had not used nerve agents in combat, they had produced significant amounts of soman, a point which Kuhn also tried to conceal.[59]

Kuhn also vehemently denied assertions that his institute had produced substances to be tested in concentration camps, and indeed no evidence of this appears in the surviving documents. In his capacity as the senior specialist in the Reich Research Council (Reichsforschungsrat) responsible for grants relating to organic chemistry, however, Kuhn did approve funding for experiments conducted by Otto Bickenbach on the effects of phosgene gas at the Natzweiler concentration camp.[60] The surviving documentation does not specify the type of experiments supported, leaving open the possibility that the grant did not formally apply to prisoners as subjects and that Kuhn was not officially aware of such experiments.[61] Nevertheless, it seems hardly likely that someone as well-connected as Kuhn, who moreover considered Bickenbach a friend and colleague, would have been unaware of the fact that the latter's experiments in 1943 and 1944 made use of prisoners, with lethal consequences for many. But even accepting his possible ignorance at the time, it is striking that Kuhn was subsequently prepared to defend Bickenbach, even though Bickenbach himself was ultimately willing to admit that his experiments transgressed the bounds of medical ethics.[62]

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57 Schmaltz, *Kampfstoff-Forschung*, 2017, 570.

58 CIOS Evaluation report 1 on Richard Kuhn, 20 April 1945, copy, AMPG, I. Abt., Rep. 29, Nr. 143.

59 Schmaltz, *Kampfstoff-Forschung*, 2017, 570–573.

60 Schmaltz, *Kampfstoff-Forschung*, 2017, 535–536, 559–560.

61 Schmaltz, *Kampfstoff-Forschung*, 2017, 573. The lack of specific documentation is emphasized by Hans-Jürg Kuhn, Richard Kuhn's son, who has sought vigorously to defend his father's reputation.

62 Florian Schmaltz: Otto Bickenbach's Human Experiments with Chemical Warfare Agents and the Concentration Camp Natzweiler. In: Wolfgang U. Eckart (ed.): *Man, Medicine, and the State. The Human Body as an Object of Government Sponsored Medical Research in the 20th Century*. Stuttgart: Franz Steiner Verlag 2006, 139–156, 147, 155–156.

Both Kuhn and Butenandt could present evidence to the Allies that they had received critical assessments by various low-level Nazi officials.[63] Such criticisms were useful for supporting claims of their anti-Nazi sentiments, but the fact that both men had continued to hold high positions and to engage in wartime research suggests that they were never seriously opposed to the Nazi regime. Indeed, Butenandt had joined the party, simultaneously with his appointment in Dahlem, but the French authorities chose not to hold this against him.[64] The fact that both men had signed similar letters rejecting their Nobel Prizes in 1939 could of course be seen as a result of political intimidation by the Nazi regime. Yet after the war, neither man took the initiative in apologizing to the Nobel Academy, which had to make it clear in response to inquiries by third parties on their behalf in 1948 – three years after the end of the Nazi regime – that formal apologies and explanations would be necessary before the Germans could receive their medals (the cash prize itself had reverted to the Nobel Foundation when unclaimed for two years, in accordance with the Nobel Prize guidelines).[65]

Kuhn's institute meanwhile was divided into two parts. In December 1945, the American military took possession of the half of the building facing the river, which housed both Bothe's physics institute (forcing him to take an interim professorship at the university) and the military hospital that had been located in Krehl's former institute for pathology. The space was initially used to accommodate an aviation medical laboratory. The U. S. Army collected former Luftwaffe scientists under the supervision of Hubert Strughold to finish wartime research projects, including some that had killed prisoners in Dachau, and for which three were put on trial by the Americans in Nuremberg, immediately after completing their research for the Americans in Heidelberg. The American report on that research referred to »alleged participation in medical war crimes«.[66] Subsequently, the American half of the building became the 4th Medical Laboratory, finally returned to the now MPI for Medical Research in 1954.

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63 Copy of letter from the Heidelberg NSDAP Kreisleiter Seiler (16 May 1944) in possession of Hans-Jürg Kuhn, who recalled his father saying that it must be kept, because it would be useful after the war; Richard Kuhn then cited this document in his own petition and another petition (apparently drafted by him, but labeled Freudenberg), both dated 15 October 1945, as evidence of his institute's »anti-Nazi attitude«, submitted to the American Military Government in order to permit him and his subordinates to continue their research. Copies in AMPG, I. Abt., Rep. 29, Nr. 142. Göttingen NSDB assessment of Butenandt in letter to Pg. Büsselmann, Kreisleitung der NSDAP Göttingen (16 Oct. 1936) in SchA–B13–402.

64 Several sources cite the party records in Berlin; Butenandt himself sought after the war to present himself only as an applicant (Anwärter), which is presumably the reason (along with his scientific ability) that the French authorities chose not to dismiss him from his post in Tübingen. Cf. Meyerhof to Lwoff, 31 Oct. 1946, in the Otto Meyerhof Papers, UPA, UPT 50 M613, Box 1, Folder 27.

65 For a detailed account, including the simultaneous forced rejection of the prize by Gerhard Domagk of IG Farben, see Alfred Neubauer: *Bittere Nobelpreise*. Norderstedt: Books on Demand 2005, 29–51; Schmaltz, *Kampfstoff-Forschung, 2017*, 376–380.

66 Benford, Report from Heidelberg, 1947, 8. The Heidelberg Dachau group included Siegfried Ruff, Konrad Schäfer, and Hermann Becker-Freyseng, accused of crimes against humanity in experiments using a low-pressure chamber to simulate high-altitude parachute jumps in which prisoners died from lack of oxygen, and others in which prisoners drank sea water to test a chemical supposed to neutralize the effects, but which proved to be toxic. Only Becker-Freyseng was sentenced to a long prison term, but he was freed in 1952. Cf. Karl Heinz Roth: Flying Bodies – Enforcing States. German Aviation Medical Research from 1925 to 1975 and the Deutsche Forschungsgemeinschaft. In: Eckart, *Man, Medicine, and the State*, 107–137, 111–112, 122–125. The two who were acquitted joined their colleagues in the United States to continue their high-altitude research as part of the Paperclip program. Contributions of all three went into the resulting

Kuhn himself ultimately cooperated fully with the Americans, providing extensive details of his wartime work over the course of repeated interrogations. This resulted in permission in July 1945 to resume his institute's activity, initially only for himself and his assistants, in the form of secret »therapeutic« chemical warfare research (on mustard gas) at a time when the Allies had expressly prohibited such research by the Germans.[67] By the end of the year, Kuhn had received permission for the rest of the institute to resume its peacetime research. Beginning in 1946, for the FIAT (Field Information Agency Technical) program, he acted as general editor for a four-volume report on non-military biochemistry research carried out in Germany during the war.[68] The FIAT work, besides supplementing his income, had the welcome side-effect of protecting his residence from confiscation by the occupation authorities, and it also simplified the task of interzonal travel (facilitating his contacts with the BASF in French-occupied Ludwigshafen) as well as opening the possibility of travel to the United States.[69] Despite several requests from the Americans, Kuhn nevertheless refused to be recruited for the Paperclip program, but he continued to have cordial interactions with the American chemical warfare officers in EUCOM, the European Command, stationed in Heidelberg. In the fall of 1948 he apparently passed along to one of them, Col. Charles Loucks, information about a new drug whose psychoactive properties had been discovered during the war in Arthur Stoll's Sandoz laboratory, which specialized in ergot derivatives. This particular one, LSD-25, had been tested with a variety of mental patients by Stoll's psychologist son Werner since 1945 and offered some potential as a non-lethal chemical weapon or perhaps a tool for the interrogation of espionage suspects.[70] Loucks investigated and sent preliminary reports to the head of the Chemical Corps and the

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publication, US Air Force of Aviation [later: Aerospace] Medicine: *German Aviation Medicine, World War II.* 2 Vols. Washingon, DC: Department of the Air Force 1950, parts IV, V, VI, and XI.

67 Schmaltz, *Kampfstoff-Forschung,* 2017, 576.

68 Richard Kuhn (ed.): *Biochemistry.* Parts I–IV. Wiesbaden: FIAT 1947. On the origins of the FIAT program see John Gimbel: *Science, Technology and Reparations. Exploitation and Plunder in Postwar Germany.* Stanford, CA: Stanford University Press 1990.

69 Andrew J. Boyle, FIAT (US) to CO, Stadtkreis Heidelberg (26 April 1946), AMPG, III. Abt., Rep. 25, Nr. 11. Declaration for the obtaining of one trip Inter-Zonal pass [to BASF-Ludwigshafen] (Heidelberg, 10 Aug. 1948), AMPG, III. Abt., Rep. 25, Nr. 12.

70 Cf. W. A. Stoll: Lysegsäure-diathylamid, ein Phantastikum aus der Mutterkorngruppe. *Schweizer Archiv für Neurologie und Psychiatrie* 60 (1947), 279–323, 314–318, 322. Stoll of course said nothing about military applications, and he reserved judgment on its therapeutic value, but he did stress the high hallucinogenic potency of the chemical in small doses, compared to such older drugs as opium, mescaline and other alkaloids, and he called for further research into its effects. It should be noted that Kuhn and Arthur Stoll, both students of Richard Willstätter, had known each other since the 1920s. Kuhn reopened contact to Stoll after the war, leading to a meeting between the two in Switzerland in August 1948 and an agreement on scientific collaboration and supplementary income for Kuhn from Sandoz. Although there was never any real collaborative research involved, aside from discussions of scientific issues, Sandoz continued to pay Kuhn an honorarium until Stoll retired in 1957 (Sandoz to Kuhn, 21 Jan. 1957, AMPG, III. Abt., Rep. 25, Nr. 325). The literature on LSD is of course immense, and much of what purports to provide historical information about the early involvement of the military and national security agencies in LSD research is poorly documented and rife with conspiracy theories. It is my intention (pending availability of appropriate source material) to prepare a more comprehensive study of these developments, particularly the recruitment of German scientists to do secret research for the U.S. military, for which I presented a preliminary report, »From Heidelberg to Edgewood: Technology transfer from Germany and American chemical and psychochemical warfare research, 1945–1971«, to the Bielefeld University International Conference on: Science and the State. Governmental Research in War and Peace during the Twentieth Century (3 March 2022).

head of Research and Engineering (the job Loucks had previously held), after which a special consultant for technical intelligence, Dr. John P. Clay, was assigned to his office in Heidelberg. In March 1949 Clay sent a secret report to the Chemical Corps head of Research and Engineering on Werner Stoll's psychological research with LSD, which was then passed on to L. Wilson Greene, Scientific Director at the chemical warfare center at Edgewood Arsenal in Maryland. Greene then cited Clay's report in his secret memorandum that initiated the U.S. Army's psychochemical warfare research program, which continued in various forms until 1974.[71]

Kuhn's positive impressions of Americans led him to visit the United States several times from December 1949 to 1954, including an invited lecture tour of several universities in May 1950, which allowed him to compare American research facilities with his own.[72] He also visited and presented at least one lecture in July 1951 at Edgewood, where one of his former co-workers, Theodor Wagner-Jauregg, was then working in the medical laboratory (presumably trying to develop effective counter-measures against nerve agents, which Kuhn's group had tried and failed to do during the war).[73] In July 1945, Kuhn had begun writing to Meyerhof, making a gesture to restore good relations and even offering Meyerhof his old position in Heidelberg, but Meyerhof could not forgive Kuhn his collaboration with the Nazis and his failure to even

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71 L. Wilson Greene: Psychochemical Warfare. A New Concept of War. A Preliminary Report. Army Chemical Center, Maryland, 17 Aug. 1949. Copy in Shulgin Archive by Erowid (https://www.erowid.org/), Cabinet 6, Drawer 3, Folder 12-GN, Subfolder GN-19, File 2017_12_22_00_36.pdf. Still being organized and thus not yet accessible to the public; copy obtained courtesy of Keeper Trout, Erowid, 18 Feb. 2022; my thanks also to David Carlson for assistance with this source. I have not yet been able to visit the National Archives and Records Administration (NARA), College Park, Maryland, to consult the relevant files from Edgewood or the files of the Chemical Corps officers to which Loucks and Clay reported. Cf. however the Charles Loucks Papers at the U.S. Army Heritage and Education Center (USAHEC) in Carlisle, PA. Loucks' desk calendar for 1948–1950 (Box 3) makes several references to his contacts with Kuhn, albeit not until 1949, and in notes for a later lecture in the 1970s he explicitly credited Kuhn with alerting him to LSD (Loucks: Lysergic Acid Compound, undated [ca. 1972], handwritten note in an untitled folder, Box 29; Loucks: typed drafts in the same location of a lecture to chapters of the Daughters and Sons of the American Revolution). Cf. Annie Jacobsen: *Operation Paperclip. The Secret Intelligence Program to Bring Nazi Scientists to America.* New York, NY: Little, Brown and Company 2014, 299–301. Jacobsen may at times misconstrue Loucks' diary entries, and her discussion includes errors of fact (e.g. an incorrect rank for Loucks), but her citations of his lecture notes appear to be accurate. On the American psychochemical testing program: Jeffrey Allan Johnson: "War Without Death"? American Military Volunteers as Psychochemical Warfare Research Subjects, 1956-1971. In: Simon Große-Wilde, Elisabeth Koelmel, and Vivian Yurdakul (eds.): Science and the State. Governmental Research in War and Peace during the Twentieth Century. Bielefeld: Bielefeld University Press, forthcoming 2024.

72 Cf. Kuhn's correspondence with his wife Daisy (1948–1950), AMPG, III. Abt., Rep. 25, Nr. 266; although his wartime and prewar letters to her are restricted by the Kuhn family, I thank Richard's son Hans-Jürg Kuhn for permission to use the postwar letters.

73 David B. Dill, Scientific Officer, Chemical Corps Medical Laboratories. Army Chemical Center, Maryland: Medical Laboratories Seminar Agenda. Date: 6 July 1951. Speaker: Dr. Richard Kuhn. Subject: Recent observations at the Max-Planck Institute for Medical Research, Heidelberg. In AMPG, III. Abt., Rep. 25, Nr. 321. Kuhn spoke on four topics, beginning with triacetylene synthesis and continuing with benzil derivatives (which had pharmaceutical applications but also potential value as anticholinergics, i.e. antidotes to anticholinesterase nerve agents in chemical warfare), antibiotics, and the »problem of thrombocytopenia« (low platelets in blood). On Wagner-Jauregg cf. Schmaltz, *Kampfstoff-Forschung*, 2017, 516–517, 583; Theodor Wagner-Jauregg: *Mein Lebensweg als bioorganischer Chemiker*. Stuttgart: Wissenschafltliche Verlagsgesellschaft 1985; and brief references in Kuhn's pocket calendar books in AMPG, III. Abt., Rep. 25, Nr. 6 (but missing pocket calendars for 1950 and 1951). Atropine was known before the war as an anticholinergic, but its effectiveness was limited for an agent as toxic as soman. It is worth noting that Loucks also met with Wagner-Jauregg while visiting Edgewood in 1949 (Loucks desk calendar 1949, entry for 10 May, in USAHEC, Charles Loucks Papers, Box 3). On the anticholinergic benzil derivatives see https://pubchem.ncbi.nlm.nih.gov/compound/Benzilic-acid#section=Use-and-Manufacturing. Last accessed 15 May 2023.

attempt to rescue Meyerhof's personal library. He did, however, expect some compensation from the KWG.[74] Meyerhof had meanwhile offered a mixed judgment of Kuhn to the Americans, praising his scientific abilities but pointing out his moral failures and cautioning against ever allowing him to teach German students.[75]

For Butenandt, Tübingen had initially appeared to be only a temporary haven to avoid the destruction wreaked on Dahlem by American bombing, but as it became clear that Germany would be occupied and divided for an extended period, he abandoned the idea of resuming work there and instead began to focus on developing his research facilities in Tübingen. Whether he ever referred to Tübingen as a »new Dahlem« is uncertain, but he certainly acted as if it might be. He maintained his ties to his colleagues from the KWI for Biology, who were at first in nearby Hechingen and later also moved to Tübingen itself. Butenandt continued to support the research group for virus research, though it was now less of an inter-institutional collaboration.[76] He also gained an important institutional base by being appointed to succeed the retiring professor of physiological chemistry, and this allowed him to concentrate the remaining staff and facilities of his KWI in the university institute (see Fig. 10 below). Additional support in 1947 enabled the construction of a new building for the virus research group led by the biochemist Gerhard Schramm and the biologist Hans Friedrich-Freksa. Unfortunately, other university priorities stood in the way of a new building for the main biochemical research institute in Tübingen, so that space and opportunities to modernize the research facilities remained limited.

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74 Kuhn to Meyerhof (20 July 1945), Kuhn to Meyerhof (17 Oct. 1945), Meyerhof to Kuhn (1 Nov. 1945) with handwritten note »nicht abgesandt«, Meyerhof to Kuhn (26 June 1946), UPA, UPT 50 M613, Box 1, Folder 27. Copy of the latter also with Meyerhof to Kuhn (6 Sept. 1946), AMPG, III. Abt., Rep. 25, Nr. 237. Discussed in Deichmann, *Flüchten*, 2001, 463–465; Schmaltz, *Kampfstoff-Forschung, 2017*, 415, latter with reference to Meyerhof's technical assistant Walter Schulz, who had at least been able to purchase Meyerhof's paintings at the 1941 auction, but not his scientific library with its irreplaceable first editions of Newton, Kant, and others. Of these Schulz could find no trace, despite his best efforts after the war. He had written to Meyerhof that the scientific books never appeared in the auction, from which he surmised that some official »had already stolen them«, a point that Meyerhof did not raise in his September 1946 letter to Kuhn. Walter Schulz to Meyerhof (9 Oct. 1945) and Schulz to Meyerhof (25 May 1946), both Otto Meyerhof Papers, UPA, UPT 50 M613, Box 1, Folder 33.

75 Meyerhof to the American Military Government Liaison and Security Office, Heidelberg (29 Jan. 1947), cited in German translation, Deichmann, *Flüchten*, 2001, 466; original in English, UPA, UPT 50 M613, Box 1, Folder 27.

76 Frictions developed between Butenandt and the biologists over the question of whether the Tübingen area KWIs should join the newly established MPG headquartered in Göttingen. The sticking point for Butenandt's colleagues was the continued role of Ernst Telschow as general secretary, supported by Otto Hahn despite Telschow's National Socialist past and his role in purging Jews from the KWG. Although Butenandt originally agreed to a united front with his colleagues against Göttingen and Telschow, at a crucial point he broke with them to support a compromise that would allow Telschow to continue in the administration of the MPG. Cf. Jeffrey Lewis: Kalter Krieg in der Max-Planck-Gesellschaft. Göttingen und Tübingen – eine Vereinigung mit Hindernissen, 1948–1949. In: Schieder and Trunk, *Butenandt*, 2004, 403–443.

Fig. 10. The Institute for Physiological Chemistry of the University of Tübingen, also home of the MPI for Biochemistry (1946–1956), photo from 1955.
Source: Archives of the Max Planck Society.

2.2 Industry and the prospect of postwar »intellectual dismantling« of German biochemical research institutions: Butenandt and Kuhn compared

Given the excellent relations that both Butenandt and Kuhn had enjoyed with the chemical industry during the 1930s, it was reasonable to expect both of them to call upon industrial support to help in restoring the scientific prestige of their institutes in the postwar era. Given the economic and political situation in the aftermath of the wartime destruction and the division of the country (and the chemical industry) under postwar occupation and in the incipient Cold War, getting such support would, of course, be a far less straightforward process than before the war. In regard to both Butenandt and Kuhn, however, a significant motivating factor for industrial support was the risk that they might take foreign positions that would be tied to consulting contracts with competing foreign firms, and indeed both men received such offers, from Switzerland and the United States respectively.

2.2.1 The Basel offer and the creation of the Triple Agreement for industrial support, 1948–1949

Butenandt's professorship in Tübingen might have ended in 1949, had he accepted an attractive offer from the University of Basel in Switzerland that came to him in mid-1948. His rejection of that call made him a hero of German science in the postwar era, someone who had put his country, his institute, his university, and the education of its scientific youth before personal gain. This was certainly the way Butenandt himself presented his decision to his colleagues and to

the MPG.[77] But a closer look shows that Butenandt was rather less selfless than his public image might suggest. Butenandt's institute had faced a major financial deficit in the funding of salaries and material following the currency reform of 1948, and because of the limited resources of the German regional authorities responsible for Tübingen in the French zone of occupation (Württemberg-Hohenzollern) until West-German unification in 1949. Under these conditions, he was strongly attracted by the offer to take a professorship at the University of Basel in Switzerland, where he would also have the opportunity to collaborate with the Swiss chemical industry. In 1949, he would also receive a call to the Ludwig Maximilian University of Munich (henceforth: LMU) to succeed Heinrich Wieland as professor of chemistry,[78] but by that time, Butenandt's situation had improved enough that he could reject this offer (which admittedly would have necessitated supervising the construction of an entirely new institute for chemistry to replace the legendary one destroyed by Allied bombing).

What kept Butenandt in Tübingen in 1949 was the support of German industry, as he told the directors of Bayer who had played the most decisive role in making this possible.[79] Bayer was one of the main successors to the IG Farben concern, which had been dissolved by the Allies for complicity in Nazi war crimes. Butenandt's industrial support came through the so-called Triple Agreement (Dreierabkommen), whereby three major chemical corporations – Schering, Bayer, and the German branch of the Swiss firm Hoffmann-LaRoche – each provided 50,000 DM annually to support Butenandt's institute for three years beginning in 1949. These subsidies were based on a »gentlemen's agreement« principally negotiated by Heinrich Hörlein, the director of Bayer's pharmaceutical division. Hörlein's acquittal in the 1947–48 IG Farben trial in Nuremberg rested to a not insignificant degree on the testimony and depositions of several German academics, including some who had been forced to emigrate, who portrayed him as motivated by science and not racial politics. This was the view, for example, of Paul György, who had come into close contact with Hörlein in 1929 regarding vitamin research, even before he began to collaborate with Richard Kuhn at the KWI for Medical Research. By 1947 György was professor of clinical pediatrics at the Penn School of Medicine in Philadelphia (he had renewed his contact but not yet his collaboration with Kuhn). György went so far as to assert that Hörlein, not his

77 Heiko Stoff: Adolf Butenandt in der Nachkriegszeit, 1945–1956. Reinigung und Assoziierung. In: Schieder and Trunk, *Butenandt*, 2004, 369–402, 400–401. Cf. also Peter Karlson: *Adolf Butenandt. Biochemiker, Hormonforscher, Wissenschaftspolitiker*. Stuttgart: Wissenschaftliche Verlagsgesellschaft 1990, 181–183.

78 In October 1949, the Faculty for Natural Sciences of the LMU Munich voted to offer the position either solely to Richard Kuhn, or (if required to include other candidates) to Kuhn and Adolf Butenandt ranked as 1a and 1b, followed by Karl Ziegler and Clemens Schöpf in the second and third ranks (Beschluss der Naturwissenschaftlichen Fakultät vom 17. Oktober 1949. In University Archives of the LMU (henceforth: UAM), OC-X-4a, Bd. 3. Information provided 22 May 2019; the file was restricted due to privacy laws, because it pertained to the professor ultimately appointed in 1952, Rolf Huisgen, 1920–2020, who was then still alive). It appears that Butenandt may have received the first call in 1949. Kuhn received an offer in January 1950 and rejected it, after which the faculty negotiated with Schöpf, and finally with Huisgen, who had only in 1949 been appointed to an associate (*ausserordentlicher*) professorship in Tübingen (Bernhard Witkop: Remembering Heinrich Wieland (1877–1957). Portrait of an Organic Chemist and Founder of Modern Biochemistry. *Medicinal Research Reviews* 12/3 (1992), 195–274, 242–244. doi:10.1002/med.2610120303).

79 Butenandt to Otto Bayer (director of research, Bayer-Leverkusen), 12 March 1949, AMPG, III. Abt., Rep. 84/1, Nr. 286 (correspondence with industry, 19[37]45–1953), fol. 31; Butenandt to Ulrich Haberland (director, Bayer-Leverkusen), 14 March 1949, ibid., fol. 121; Butenandt to Hörlein (15 Jan. 1949), ibid., fol. 257.

associate Gerhard Domagk, should have been awarded the Nobel Prize in 1939 for developing the sulfa drugs.[80] Scholars have frequently cited Butenandt's testimony, which supported the contention of Hörlein's defense counselor that the Americans had mistranslated the word »Versuche«; it should not be read as »experiments« (in which case the prisoners tested as human subjects would be subjected to unacceptable risks) but as »clinical trials«, implying a much less risky situation.[81] After his acquittal, a grateful Hörlein told Butenandt that because his testimony had »broken the ring« of the prosecution's case against Hörlein »based on the false translation of the word ›Versuch‹ ..., I will be indebted to you for the rest of my life.«[82]

Thus, immediately after Hörlein returned to his position at Bayer following his acquittal, he took up the cause of finding industrial funding for Butenandt. Even before Hörlein had come back, however, the idea of funding Butenandt's institute appears to have originated with Otto Bayer, the Bayer corporation's director of research (note that Otto Bayer was not related to the founder of the company). Otto Bayer had already played a key role in establishing a Fund for the Advancement of Chemistry, replacing the interwar organizations that had centralized industrial support for German chemistry doctoral fellowships and journals.[83] In June 1948 he visited Butenandt in Tübingen to discuss a »personally very important issue«, following up by inviting Butenandt to give a lecture at the Bayer works in Leverkusen in November. At that point Bayer had discussed the issue with the managing director, Ulrich Haberland, but not yet with Hörlein, who had just returned to his position in Elberfeld.[84]

Hörlein in the meantime had received an urgent letter from Hans H. Weber, director of the institute for physiology and Butenandt's dean at the faculty of medicine at Tübingen, describing the situation that had arisen with the call and the critical situation of Butenandt's institute. In Dahlem he had enjoyed a budget of some 250,000–300,000 RM, but the South Württemberg authorities could only afford 100,000 RM, so that Butenandt received no salary as institute director (although he did have his professorship), while his associates received only 70% of theirs;

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80 Paul György: Affidavit, 9 July 1947, BAL-271–002. Meyerhof had put Kuhn in contact with György: Meyerhof to Kuhn, 26 June 1946, UPA, UPT 50 M613, Box 1, Folder 27.

81 NARA, College Park. RG 238: National Archives Collection of World War II War Crimes Records. Series: German Transcript of the Proceedings of Case No. 6, United States v. Carl Krauch, et al. Vol. 18a–20a, NAID: 285901394. Butenandt's testimony on 2 February 1948 is in Vol. 18a, 6229–6257, with the discussion of human experiments vs. clinical trials on 6234–6236 (courtesy Florian Schmaltz). Cf. Gausemeier, An der Heimatfront, 2004, 166–167, and Stoff, Butenandt, 2004, 369–402, 379–380.

82 Hörlein to Butenandt, August 1948, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 245.

83 Otto Bayer to Blaser, Goldschmidt, Konen (Kultusminister, Bonn), Klemm, and Ziegler, 28 March 1947, copy to Butenandt, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 19 verso. In 1950 the supporting fund was reorganized as the Fonds der chemischen Industrie, with Otto Bayer as chairman (cf. documents in BAL–46–015–003 (Justus-Liebig-Ges. vom 27.9.[19]20, ab 1945 Fonds der chemischen Industrie)). On the interwar supporting organizations, the Justus Liebig and Adolf Baeyer Societies, which had been organized by a previous Bayer director, Carl Duisberg, see Jeffrey Allan Johnson: The Academic-Industrial Symbiosis in German Chemical Research, 1905–1939. In: John E. Lesch (ed.): *The German Chemical Industry in the Twentieth Century*. Dordrecht: Kluwer Academic Publishers 2000, 15–56, 32–35, 43–45.

84 O. Bayer to Butenandt, 21 June 1948, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 22; O. Bayer to Butenandt (30 Sept. 1948), ibid., fol. 24.

moreover, inflation was badly undercutting his budget for material. The only possible way out was for Butenandt to receive additional support from the German chemical industry, and the dean therefore appealed to Hörlein to organize some sort of »emergency association« (*Notgemeinschaft*).[85] This inspired Hörlein to write to Butenandt regarding the call to Basel, suggesting that he would be reluctant to participate in what Otto Hahn had called the »intellectual dismantling of Germany« and asking Butenandt point-blank what it would take to keep him in Germany.[86] The answer, 150,000 DM (following the currency reform that replaced RM with DM), appeared in an article of the *Stuttgarter Zeitung* that, like Weber, also referred to the need for an »emergency association of science and industry«, which alone could provide the necessary funds to equip Butenandt's institute with more modern apparatus, particularly an electron microscope, a quartz spectroscope, and a mass spectroscope.[87]

Following discussions between the three Bayer directors and Butenandt in Leverkusen in November, Hörlein energetically and successfully worked to persuade the directors of Schering (to whom he had given the dean's letter)[88] and Hoffmann-La Roche (the German branch in Grenzach) to join in support of Butenandt, setting the target at 50,000 DM per firm, totalling 150,000 DM per year for three years. Butenandt himself meanwhile conducted negotiations with the Swiss in Basel, with Hahn in Göttingen, with the directors of the German branch of Hoffmann-La Roche in Grenzach (whom he met in company with Hörlein), with the university administration in Tübingen, with the education ministry for Württemberg-Hohenzollern, and finally with the French occupation authorities.

Initially Rudolf Schmidt of Schering was skeptical that anything could be done to keep Butenandt in Germany, but he was willing to discuss Hörlein's proposal.[89] Schering was in a particularly difficult position at this time, in view of the uncertain future of its main works in Berlin (this was the height of the 1948–49 Soviet blockade, and the firm had moved its headquarters to Braunschweig). Based on their prewar contractual arrangements, Schering had exclusive rights to patent and develop Butenandt's research results under the so-called *»Scheringreservat«*. But Hoffmann-LaRoche made dropping the *Scheringreservat* a condition for their subsidizing Butenandt's research.[90] Although Schering accepted this in principle, their board queried Hörlein

85 H. H. Weber (Dekan, Med. Fak. Tübingen) to Hörlein, 8 Oct. 1948, BAL–363–274. Note that Hans Hermann Weber was a former postdoctoral assistant of Otto Meyerhof. In 1954 he would become the first real successor to Meyerhof as director of the revived institute for physiology in the MPI for Medical Research.

86 Hörlein to Butenandt, 13 Oct. 1948, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 246; copy of this letter sent by Hörlein to Weber, 13 Oct. 1948, BAL–363–274.

87 M: Geht Professor Butenandt nach Basel? *Stuttgarter Zeitung* Nr. 118 (11.12.48), clipping BAL–363–274. Note that the DM had replaced the inflated RM in June 1948 at the rate of 1 to 10, thus wiping out any previous endowments that might have helped Butenandt's institute and making his financial difficulties even more acute.

88 Undated note from Schering-Braunschweig to Hörlein returning the letter from Weber to Hörlein, 8 Oct. 1948, BAL–363–274.

89 Schmidt to Fritz Mietzsch (director, Bayer),9 Nov. 1948, BAL–363–274.

90 Butenandt to Hörlein, 20 Nov. 1948, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 248; Hörlein to Schmidt, 27 Nov. 1948,

whether he had spoken to Butenandt about their royalty payments to him. Hörlein told his counterpart at Schering, Rudolf Schmidt, that he had indeed suggested to Butenandt that the company could deduct its subsidies »in whole or in part« from the patent royalties it would otherwise pay to Butenandt.[91] Schmidt thereupon confirmed this suggestion to Butenandt, with the apology that their direct contribution could only be »very modest«, in view of the firm's difficult postwar situation in Berlin. The compensation to Butenandt would be that in this form the payments would be tax-free.[92] As a result, it appears that Schering was able to take its entire subsidy to Butenandt's research out of royalty payments, at least in the early years.[93]

Another negotiating issue arose when the German branch of Hoffmann-LaRoche wanted the right to transfer to its Swiss parent company any rights it might claim to Butenandt's results. Such a transfer would, however, require the permission of the French military occupational authorities.[94] But when Butenandt approached the French, who had thus far been quite favorable to him, he encountered unexpected opposition to the agreement, in part because they apparently expected his scientific work to be affected by his financial dependence upon industry. Despite Butenandt's efforts to reassure the French, by early December 1948 they had not yet approved the proposed agreement. Butenandt therefore asked Hörlein to help him work out an alternative approach that would satisfy Hoffmann-LaRoche while making »superfluous« any further negotiations with the French.[95] The solution they found in a meeting with the Hoffmann-LaRoche directors in Grenzach in mid-December was not to put the agreement among the firms into a formal contract, but rather to keep it as an unwritten »gentleman's agreement«.[96] As Butenandt assured Hörlein, this created the basis that would allow him to stay in Tübingen, but he was delaying an official decision until January, pending some final negotiations with the government.[97]

BAL–363–274 (copy also sent to Butenandt). Butenandt gave Schering exclusive worldwide rights to patent results of his research, which were to be kept secret from third parties until Schering authorized their publication; cf. Schering-Butenandt contract of November 1944, copy in SchA–B5–333.

91 Hörlein to Schmidt, 30 Nov. 1948, BAL–363–274 (copy also sent to Butenandt).

92 Schmidt to Butenandt, 10 Dec. 1948, partial copy, SchA–B5–074. A recent history of Schering to 1950 does not mention their postwar subsidy to Butenandt but notes that the company was actually in a decent financial position, »turning a handsome profit« by 1948. Christopher Kobrak: *National Cultures and International Competition. The Experience of Schering AG, 1851–1950*. Cambridge: Cambridge University Press 2002, 357.

93 In 1949 Schering had to add nearly 21,000 DM to Butenandt's royalty payments to make up its 50,000 DM research subsidy; but in the next four years (1950–1953), the royalty payments from his share in the profits of five products during this period exceeded 50,000 DM, allowing the company to cover its entire research subsidy for Butenandt from this source. See handwritten table headed »Prof. Butenandt«, 30 April 1954, SchA– B5–074.

94 Butenandt to Hörlein, 20 Nov. 1948, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 248.

95 Butenandt to Hörlein, 4 Dec. 1948, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 254.

96 Hörlein to Röttger, Rechstabteilung Leverkusen, 4 March 1949, BAL–363–274.

97 Butenandt to Hörlein, 28 Dec. 1948, handwritten letter, BAL–363–274.

Thus, as late as the beginning of January 1949, it was not yet clear to the Bayer directors whether Butenandt had definitely decided to stay in Tübingen. Hörlein urged Butenandt not to make any final decision before talking to him, and Otto Bayer wrote to Butenandt to confirm that they continued to hold open to him the third possibility of coming to the Bayer pharmaceutical laboratories in Elberfeld, which they believed would offer »considerably greater« resources and possibilities for his research than he might find in Switzerland.[98]

Before Butenandt could respond to the Bayer directors, he needed the university and ministerial authorities to find a means of accepting the Triple Agreement while not making its details public. Butenandt could not hide the fact that his institute's research budget was receiving funds from outside sources, which might well have led to embarrassing questions from the French. Here, however, Dean Erich Kamke of the faculty for mathematics and natural sciences in Tübingen came to his rescue. Kamke had written to Hörlein in November to assure him that the faculty was strongly in favor of retaining Butenandt, whose dual appointment with the medical faculty made possible a collaborative research group on the border of biology and chemistry that was »currently unique in Germany«, and whose loss to Switzerland might trigger additional losses of top German scientists. Thus the faculty welcomed Hörlein's support, recognizing that »industrial circles« would be the only possible source for the support Butenandt needed.[99] By late November Hörlein could inform Kamke that he was almost certain that Bayer would join with one or two other firms in the pharmaceutical industry to cover the deficit in Butenandt's research budget.[100] Kamke had meanwhile reported that he and the *Prorektor* of the university had met with the responsible ministers, who had promised to do their utmost to support Butenandt. But the government simply lacked the resources to compete with the Basel offer, which had taken a final form that appeared to satisfy all of Butenandt's demands. Kamke was willing to help »in any way«; initially he sought to use press reports to dramatize the importance of Hörlein's efforts to recruit industrial support, and it may be due to him that a newspaper subsequently reported that the Swiss authorities were willing to pay for transferring Butenandt's entire research group to Basel, which would have meant the dissolution of his Max Planck Institute.[101] In the end, however, Kamke's key service was to provide a convenient cover for the Triple Agreement. In January the press reported that, with the approval of the ministerial authorities who lacked the funds to cover Butenandt's deficit, the dean had created faculty account No. 9225 in the local savings bank to collect tax-free donations from the public on behalf of Butenandt's institute, with the target being, of course, 150,000 DM.[102] The French

98 Hörlein to Butenandt, 7 Jan. 1949, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 256; Otto Bayer to Butenandt, 6 Jan. 1948 [sic = 1949]), ibid., fol. 31.

99 Kamke (Dekan, Math. & Naturw. Fak. Tübingen) to Hörlein, 8 Nov. 1948, BAL–363–274.

100 Hörlein to Kamke, 27 Nov. 1948, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 251.

101 Kamke to Hörlein, 18 Nov. 1948, BAL–363–274; M, Geht Professor Butenandt, 1948, ibid.

102 T. H.: Gelehrtenschwund. Sorgen um den Forscher Butenandt – Es fehlt Geld für Arbeitsmittel. *Die Zeit* Nr. 2 (13 Januar 1949), clipping, BAL–363–274. The industrial subsidies did not, of course, go through the faculty account, but were sent directly to Butenandt's own bank, to »Research Account Prof. Butenandt«. Cf. Butenandt to Schering AG (Braun-

authorities would therefore not have reason to ask how Butenandt's research budget was suddenly receiving an external subsidy in that amount.

Two days after the press report on the creation of his faculty research account, Butenandt definitively rejected the call to Basel and immediately wrote to Hörlein, »without your help and your decisive action on my behalf, I could not have stayed in Germany.« Because Butenandt's institute was in serious financial straits, however, he then asked Hörlein to ensure that the monthly instalments from the companies could be paid as soon as possible. Bayer had already provided 4,000 DM in December, even before the other two firms had finalized the »gentleman's agreement«.[103] Hörlein followed up with messages to the other two firms, which agreed to have the agreement take effect from the beginning of January, although Hoffmann-LaRoche did not confirm this until the end of the month.[104]

The Triple Agreement offered a clear reward to its three corporate partners for their research subsidies, and this was presumably what had troubled the French: each firm would have equal rights to the commercial development of Butenandt's research, with the understanding that the company with the best prospects for developing any specific result would be given priority in patenting it.[105] Because patents could not be issued for published research, this meant that Butenandt agreed to submit his entire institute's research results, i. e. final drafts of all scientific papers, to all three companies before publication. Thus, in March 1949 he sent the first five papers (three on the synthesis of amino acids and two on the chemical relationship between steroids and carcinogens) to Hörlein and directors of the other two firms, asking if they »would like to express any wishes in regard to these publications«. Hörlein flagged two papers on amino-acid synthesis co-authored by Butenandt and one or two associates, as potentially patentable, and he asked the other partners in the Triple Agreement for their views.[106] During the lifetime of the Triple Agreement until Butenandt's retirement in 1972, Bayer would take out at least nine patents naming Butenandt as a co-inventor, and Hoffmann-LaRoche would take out at least twelve.[107]

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schweig), 12 Feb. 1949, transcription in SchA–B5–074. Karlson, *Adolf Butenandt*, 1990, 183, mentions the Kamke fund, which collected numerous small donations from private citizens in Tübingen, along with larger contributions from industry, as the only source for the funds needed to purchase an electron microscope costing 100,000 DM. Based on the available evidence, I consider this a doubtful claim, though Butenandt and Kamke may have made it at the time. It seems far more likely that the bulk of the cost came from the firms of the triple agreement.

103 Butenandt to Hörlein, 15 Jan. 1949, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 257.

104 Hörlein to Hellmich (Hoffmann-LaRoche) and Schmidt, 19 Jan. 1949, BAL–363–274; Hellmich to Hörlein and Schmidt, 31 Jan. 1949), ibid.

105 Hörlein to Röttger, Rechstabteilung Leverkusen, 4 March 1949, BAL–363–274.

106 Butenandt to Hörlein, 14 March 1949, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 258. Hörlein to W. Hellmich (director, Hoffmann-La Roche, Grenzach) and R. Schmidt (director, Schering, Braunschweig), 8 April 1949, BAL– 363–274. Cf. extensive subsequent correspondence and documents in the Schering and Bayer archives following up on the Triple Agreement.

107 Results of search on the Espacenet website for patents taken out by the three Triple Agreement firms and listing Butenandt and his associates as inventors during the years 1949 to 1972. In addition to the twelve going to Bayer and Hoffmann-

Along with the papers (the first of a long series), Butenandt could joyfully inform Hörlein and the other participants in the Triple Agreement that their support was paying off in the transformation of his institute. Work had already begun on renovating the building, installing several new laboratories with modern apparatus (cf. Fig. 11 below). They had also just celebrated the completion of the roof for the new building for virus research, which would be opened in 1950 and would be equipped with expensive modern apparatus including an electrophoresis laboratory and ultracentrifuges, thus carrying on the tradition of industrial support for virus research begun in Dahlem (Figs. 12 and 13 below). Overall, Butenandt believed that his associates were carrying out their research with a newly optimistic spirit.[108]

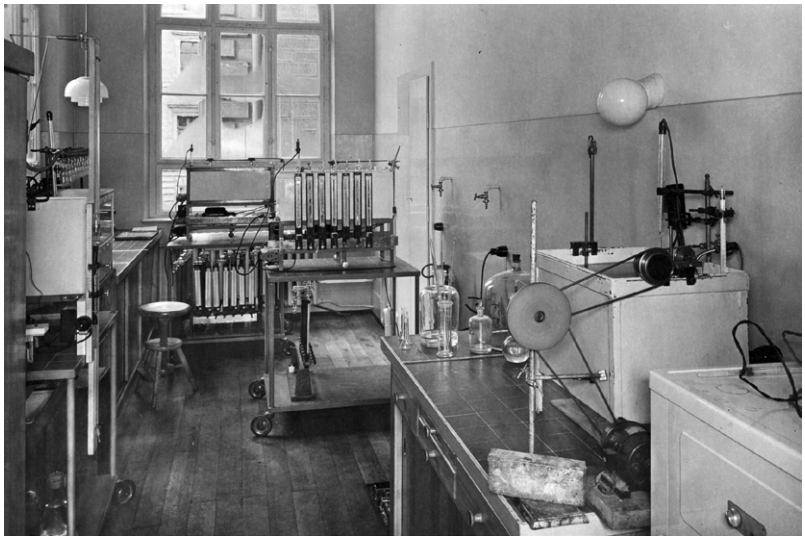

Fig. 11. Metabolism research room with Warburg Apparatus, MPI for Biochemistry, Tübingen, 1951 (inside the old university institute, whose age and limited space is suggested by the narrow room filled with apparatus). Source: Archives of the Max Planck Society.

Fig. 12. Serology lab in the new building for virus research, MPI for Biochemistry, Tübingen, 1951 (compare the more modern, spacious appearance to Fig. 11)

LaRoche, there were ten patents by Schering that listed Butenandt during this period, but all of them had been submitted before 1945. See: Espacenet: Suchergebnisse. https://worldwide.espacenet.com/patent/search/family/007450120/publication/DE756002C?q=pd%20within%20%221949-1972%22%20AND%20in%20any%20%22Butenandt%22. Last accessed 3/27/2023.

108 Butenandt to Haberland, 14 March 1949, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 159; Butenandt to Hörlein, 14 March 1949, ibid., fol. 258.

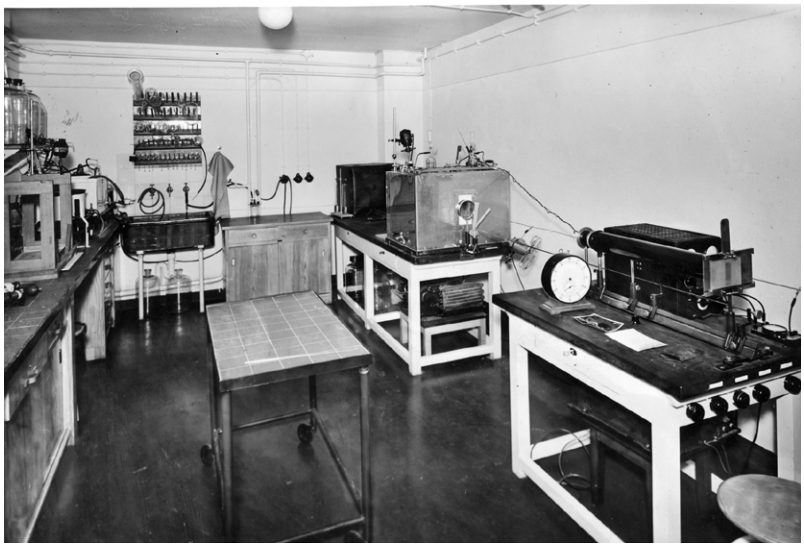

Fig. 13. Electrophoresis lab in the new building for virus research, 1951.
Source: Archives of the Max Planck Society.

By the end of 1951, when it appeared that the renovation of the institute was complete in accordance with Butenandt's needs, Hörlein proposed to the directors of the three supporting corporations that they continue their support for Butenandt, but reduce it to one-third of the original amount, for a total of 50,000 DM annually.[109] They accepted this proposal, and the firms continued to support Butenandt's research on this level until his retirement as director in 1972. The one change came with his assumption of the presidency of the MPG in 1960, whereby he continued to act as nominal director of the MPI, but in fact delegated management of the institute to a deputy director (by this time, as will be discussed later, Butenandt had moved his MPI to Munich, where he held a dual appointment in the university similar to that in Tübingen). At this point, the three firms decided to formalize the »gentleman's agreement« into a written contract. As specified in 1961, the contract was backdated to take effect from 1 January 1949, thus ensuring a clear, legal basis for the arrangements that Butenandt had thus far handled informally. In particular, the contract clearly specified that all the results of the work of Butenandt »or his research laboratories« (thus including everyone working in his institute) should be presented simultaneously to all three firms before publication. Moreover, these research results and any patents or patent applications based on them »belong to all three firms as co-owners.«[110]

Special provision had to be made for Butenandt's former associate Peter Karlson, who became Butenandt's successor as director of the university institute for physiological chemistry and thereby no longer had a position in the MPI for Biochemistry. Because he continued to work in the area of insect and crustacean metamorphosis and molting hormones (in particular, ecdysone) begun under Butenandt's direction in the 1950s and commercially developed under the

109 Hörlein to Pattat (Hoffmann-La Roche, Basel) and Schmidt (Schering, Berlin), 14 Nov. 1951, SchA–B5–074, copy in AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 280; for Butenandt's acceptance see typed copy of Butenandt to Schmidt, 28 Jan. 1952, ibid.; Butenandt to Haberland (Bayer), 28 Jan. 1952, photocopy in BAL–363–274.

110 Hoffmann-LaRoche AG Grenzach to Bayer and Schering, 23 May 1961, BAL–363–275; attached final draft of Vertrag, 23 May 1961, 2; related documents and correspondence ibid.

Triple Agreement, Karlson had also to agree to submit to the firms any future research results related to this area. In return, they would support his research up to the amount of 30,000 DM annually (10,000 DM from each firm).[111] The firms had had some difficulties in dealing with Karlson beginning in the late 1950s, when he tried to enter into partnership for distribution in the USA with an American company headed by Eric P. McNair. McNair was not a scientist but an attorney, whom the three firms considered to be a doubtful partner after their investigation showed his company to be a tiny mail-order firm for fishing gear, run from the McNair residence. After Karlson gave an insulting response to a critical letter from Fritz Mietzsch, Hörlein's successor as director of Bayer's pharmaceutical research, Butenandt hastily wrote an apologetic letter to Mietzsch to avoid a break in his hitherto good corporate relations.[112] One can only speculate as to what Butenandt had said to Karlson; in any case, this particular incident does not appear in Karlson's laudatory biography of Butenandt.[113]

Karlson continued his friendship with McNair into the early 1960s, but McNair, still interested in ecdysone, opened a connection with the competing American Cyanamid company, which attempted to get information from Karlson on his research. By then the Triple Agreement firms were collaborating with Karlson in the complicated process of synthesizing and specifying the chemical structure of the hormone. Hence Karlson warned McNair in 1963 that for the time being he could not discuss ecdysone. Schering had assigned a team of four chemists to concentrate on this research and could thus take much of the credit for the synthesis, for which it applied for a patent in the fall of 1964.[114]

More scientists became participants in these arrangements in 1965. Karlson's assistant Dr. Hoffmeister, who had done significant work on isolating natural ecdysone, took a position in Hamburg and wanted to continue his hormonal research; the firms needed to consider adding him to their collaborative agreements. By this time Karlson had gained another academic collaborator in Walter Hoppe, an expert in structural analysis by x-ray diffraction then in the neighboring MPI for Protein and Leather Research. He too would be offered support under an arrange-

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111 Peter Karlson to Butenandt, Bayer, Schering, and Deutsche Hoffmann-LaRoche, 17 May 1961, BAL–363–275.

112 Schering to Butenandt, with copies to F. Mietzsch [Bayer] and H. Saenger [Hoffmann-LaRoche], 7 Aug. 1957; Peter Karlson to F. Mietzsch, 28 March 1958, forwarding excerpt of letter from the Eric Fare company [Eric McNair]; cc of Mietzsch to Junkmann (Schering), 2 April 1958; Karlson to Mietzsch, 21 April 1958; Butenandt to Mietzsch (23 April 1958), all in BAL–363–274. On the Eric Fare company's deceptive advertising in 1958, see Bill Sonnett: Deconstructing Old Ads: Bass Fisherman Will Say I'm Crazy (1958). Blog Entry. *Fishing for History: The History of Fishing and Fishing Tackle, by Todd Larson*, 4/2/2011. https://fishinghistory.blogspot.com/2011/04/deconstructing-old-ads-bass-fisherman.html. Last accessed 2/14/2023.

113 Karlson, *Adolf Butenandt*, 1990, has no index entry for McNair or Mietzsch, discussing ecdysone from a purely scientific perspective, with no reference to the companies that supported the research. These are mentioned only in a note in connection with Butenandt's decision not to go to Basel in 1949 (note [6, 56], on 320, to 182), with no indication of the longer duration of the Triple Agreement.

114 Peter Karlson to Eric McNair, typed draft, 18 March 1963, BAL–363–275; Besprechungsnotiz vom 23. Juni 1964 (copy), ibid.; Dr. Auhagen, Bayer: Notiz über eine Besprechung im Frankfurter Schering-Haus am 23. Juni 1964, ibid.; Dr. Auhagen: Aktennotiz. Besprechung am 18.–19.2.65 in Berlin im Rahmen des Dreier-Vertrages mit Prof. Karlson, 1 March 1965, ibid.

ment similar to Karlson's, at least until the publication of the synthesis and structure of ecdysone. Karlson and Hoppe of course wanted to move ahead with appropriate publications, but Hoffmann-LaRoche in particular insisted on keeping the results secret while moving ahead with commercial development. Bayer had no commercial interest in this area, and their people counseled against waiting too long, given the possibility that a competing research group might take the credit (and patent rights) away from Karlson and Hoppe. Karlson took a lecture tour in the USA in 1965 to discuss the biochemical role of ecdysone, but he had to be warned not even to mention its structural formula in private discussions with McNair or others, who might bring legal pressure on him to reveal his knowledge of the structure, thereby putting him in breach of his contract under the Triple Agreement.[115] Given the limitations of the current project, I cannot discuss the further ramifications of this case, but it clearly illustrates the difficulties that can arise when commercial corporate interests compete with those of academic scientists.

2.2.2 Richard Kuhn's relations to German and American industry: Heidelberg or Philadelphia?

Following Butenandt's success with the Triple Agreement, Richard Kuhn attempted a similar tactic of using a threat to accept a foreign position (in this case in the United States) to obtain greater support from industry and the MPG for his Heidelberg MPI.[116] One of the main points of contention with the MPG administration in Göttingen involved the future status of Bothe's physics institute within the MPI. Kuhn had insisted on acting as sole managing director of the MPI, as he had been designated in 1937 under Carl Bosch's presidency of the KWG; whereas Bothe, despite having been evicted from his institute by the American military (aside from limited access to the cyclotron in the basement), wanted a return to collegial management, and preferably a significant addition to the building to house his physics institute after the Americans departed. During a meeting with Hahn in Göttingen in July 1949, Kuhn got the impression that the MPG intended to »break up« the MPI, presumably by allocating the greater part of it to the institute for nuclear physics demanded by Bothe.[117] Seeing this prospect as a threat to the development of his own research institute within the MPI, Kuhn began seriously considering the possibility of taking a research professorship at Penn in Philadelphia, where he would have strong research support from local industry while renewing a research collaboration broken off by the events of 1933.

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115 Dr. Auhagen: Aktennotiz: Betr.: Ecdyson / Prof. Karlson. Telefongespräch mit Dr. Gibian, Schering, am 11. 2. 1965, 12 Feb. 1965, BAL–363–275; Dr. Auhagen: Aktennotiz. Besprechung am 18.–19.2.65 in Berlin im Rahmen des Dreier-Vertrages mit Prof. Karlson, 1 March 1965, ibid.

116 Beginning in October 1946 Kuhn was in communication with the educational authorities in Vienna regarding a possible appointment there, but the documentation does not show how he might have used this to improve his situation in Heidelberg. Interzonal connections with the Generalverwaltung in Göttingen were still difficult in 1946, and funding was still precarious, so that it seems unlikely that Kuhn could have received much benefit from the call. See correspondence in AMPG, III. Abt., Rep. 25, Nr. 41.

117 Kuhn to Telschow, handwritten draft, Heidelberg, 10 Aug. 1949, AMPG, III. Abt., Rep. 25, Nr. 396. Kuhn to Hahn, 10 Jan. 1950, AMPG, III. Abt., Rep. 25, Nr. 389 and Kuhn to Hahn, 16 Jan. 1950, ibid.

Kuhn had been in contact with Paul György in the Penn School of Medicine in Philadelphia since 1947, and one of Kuhn's assistants, Gerhard Wendt, had received an American fellowship to work under György's direction on a project related to cancer research, beginning in fall 1948.[118] In the spring of 1949 Kuhn and György began to correspond regarding the renewal of their collaboration, this time focused not on vitamins but other natural products such as antibiotics, and ultimately a study of the components of human breast milk. The latter was a topic of mutual interest to both men, György as a professor of clinical pediatrics studying the nutritional and immunity-strengthening role of breast milk, and Kuhn as a bio-organic chemist interested in the oligosaccharides, including those found in breast milk that have the important function of promoting the growth of beneficial bifidobacteria in the large intestine. György thereupon contacted Dean John M. Mitchell of the Penn medical school, who invited Kuhn to Philadelphia to discuss a possible appointment as a research professor of physiological chemistry.[119]

György also approached Harry S. Howard, the president of the Wyeth pharmaceutical company (a division of American Home Products, henceforth AHP), to offer Kuhn a consulting position and research facilities in Philadelphia. Initially they would supply Kuhn with chemicals in accordance with his needs, and they probably co-financed Kuhn's trip to Philadelphia. György came to Switzerland that summer and probably met Kuhn to discuss details.[120] Kuhn believed he could also get financial support from Otto Haas, director of the Rohm and Haas chemical company in Philadelphia, who, despite the fact that they had never met, had been sending him CARE packages. The connection to Kuhn went through the daughter of one of Haas's top research chemists, who had been a private assistant for Kuhn during the 1930s.[121] Thus in December Kuhn flew to the United States to investigate these possibilities, but first he contacted Hörlein, who was willing to raise industrial subsidies for Kuhn's MPI that might induce him, like Butenandt, to stay in Germany. In Kuhn's case Hörlein's idea, presented to Kuhn in early January 1950, was for Bayer and BASF to provide joint support, in a sense renewing their earlier support (as branches of IG Farben) for his vitamin research.[122] At some point in January, Kuhn had also received an offer from the faculty for natural sciences in Munich to replace the retiring Heinrich Wieland as professor of chemistry at the LMU. He did not decline the offer until

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118 KWI für medizinische Forschung. Institut für Chemie: Aktennotiz, 5 Aug. 1948, AMPG, II. Abt., Rep. 23, Nr. 51, fol. 306.

119 Cf. cc of John M. Mitchell to U. S. Consul, Stuttgart, 6 Sept. 1949, AMPG, III. Abt., Rep. 25, Nr. 240; for the offer of the professorship: György to Kuhn, 26 July 1949, ibid.

120 György to Kuhn, 11 May 1949, AMPG, III. Abt., Rep. 25, Nr. 227. This letter does not specifically mention Wyeth, but later correspondence suggests they would have been the source for chemicals. Kuhn's Austrian passport issued in Höchst (perhaps not coincidentally, the location of FIAT where Kuhn was then working) in 1947 has a Swiss visa for 15 Aug. – 2 Sept. 1949, AMPG, III. Abt., Rep. 25, Nr. 4, when György had said he would be in Switzerland.

121 Cf. Kuhn's handwritten draft letter to György, 11 Sept. 1949, AMPG, III. Abt., Rep. 25, Nr. 227. The correspondence between Kuhn and Howard of Wyeth, Inc., AMPG, III. Abt., Rep. 25, Nr. 260, begins in 1950. For evidence of György's earlier negotiation with Wyeth see Paul György to Kuhn, 11 May 1949, AMPG, III. Abt., Rep. 25, Nr. 227 and Kuhn to György, 11 Sept. 1949, ibid.

122 Hörlein to Kuhn, 2 Jan. 1950, AMPG, III. Abt., Rep. 25, Nr. 228, referring to Hörlein to Kuhn, 29 Nov. 1949, not in this volume.

July 1950, thus increasing his negotiating strength with the Americans in Philadelphia and with the MPG.[123]

Kuhn's December visit began with a stay at György's home in suburban Villanova, a half-hour train ride northwest of Philadelphia. Villanova, Kuhn wrote to his wife, reminded him of Dahlem, and he was also impressed by the many academic institutions in the area with their architecture reminiscent of Oxford and Cambridge.[124] He also went to New York City, which greatly impressed him with its »un-American« elegance, reminding him of downtown Zurich (Kuhn's wife was Swiss), and attended a congress of the Macy Foundation at which György presided over a session on antioxidants, followed by a dinner meeting of the American Chemical Society, New York Section, with a lecture by the ACS president, Linus Pauling. After a stay with relatives in New York, he returned to Villanova and Philadelphia. Dean Mitchell and others gave him a tour of the Penn School of Medicine, including Meyerhof's laboratory, which was evidently rather poorly equipped and did not impress him; a visit to Wyeth Laboratories followed, at which he no doubt discussed potential links to the business and opportunities for additional research support. The last part of his trip was to Montreal to visit Ayerst, McKenna, & Harrison, the Canadian division of AHP. Kuhn's letters to his wife show how much he wanted her to like America, but he also followed his New York relative's advice to bring her along on a second visit in 1950, before making a final decision on the position in Philadelphia.[125] This worried György, who evidently knew Daisy well enough to fear her reaction to American culture.[126]

Having thus accepted his American contacts' invitation to return for follow-up negotiations in April and May, with the expectation that he would make a final decision at that time, Kuhn could take an aggressive approach in his negotiations with the MPG in 1950. Yet he waited until March to answer Hörlein's January letter, at which point Hörlein urged him to prepare a clear statement of the concessions that would suffice for Kuhn to stay in Germany, and further to ask Hahn to convene a special meeting of the Senate of the MPG to resolve the issues between Kuhn and Bothe.[127] In the meantime, Kuhn had met with Hahn in mid-January to clarify the MPG's plans in regard to Bothe's demand for an autonomous, greatly expanded institute for nuclear physics within the MPI for Medical Research. Hahn insisted that given current financial and

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123 Beschluss der Naturwissenschaftlichen Fakultät vom 17. Oktober 1949 (information provided by the UA-LMU, 22 May 2019). Cf. cc of the activity report for the MPI for Medical Research, 1946 to March 1951, submitted by Kuhn to the Generalverwaltung, MPG, 18 April 1951, AMPG, II. Abt., Rep. 23, Nr. 13. Kuhn to Hans Rheinlander, Bayerisches Staatsministerium f. Unterricht und Kultus, 1 July 1950, AMPG, III. Abt., Rep. 25, Nr. 36.

124 Richard Kuhn to Daisy Kuhn, Villanova, 5 Dec. 1949, AMPG, III. Abt., Rep. 25, Nr. 266; the nearest regional rail station to György's home was on the campus of Villanova College, whose main building and chapel were in the gothic style, which may have given Kuhn his first impression of American college architecture.

125 Kuhn to Daisy Kuhn, New York, 9 Dec. 1949, and Kuhn to Daisy, Villanova, 13 Dec. 1949, AMPG, III. Abt., Rep. 25, Nr. 266. Travel notebook with entries for 3–13 December in AMPG, III. Abt., Rep. 25, Nr. 4.

126 György to R. Paltauf, 21 Jan. 1950, AMPG, III. Abt., Rep. 25, Nr. 227 (Paltauf, a physician, was Daisy's cousin); Hans-Jürg Kuhn told me that he believed it was his mother who dissuaded Richard Kuhn from accepting the permanent professorship at Penn.

127 Hörlein to Kuhn, 8 March 1950, AMPG, III. Abt., Rep. 25, Nr. 228.

administrative constraints, there was no plan to divide and expand the institute in this way, but that he did support a return to the original conception of the KWI for Medical Research as a collegially governed institution wherein Bothe would have greater financial and administrative autonomy.[128] After an exchange of correspondence arguing the opposing cases, Hahn met with Kuhn and Bothe in March to arrive at a preliminary understanding. In the clash with Bothe, Kuhn had an ally in Isolde Hausser, widow of Bothe's predecessor and herself a physicist directing a small section for therapeutic physics at the MPI. Due to illness, she could not be present for the confrontation in March, but she sent lengthy letters to Hahn and other leading members of the MPG to strengthen Kuhn's case that with Bothe directing the MPI's institute for physics, it would not be possible to return to the original conception of a collegially directed institution featuring collaborative research between the individual directors. This was simply because, from Kuhn's and Hausser's perspective, Bothe had never made a sincere attempt to achieve the kind of collaboration that Karl Wilhelm Hausser had engaged in with Kuhn in the early 1930s. Of course, Isolde Hausser was also writing in her own interest, because she saw Bothe as an enemy who wanted her out of the MPI.[129]

Hahn also convened a meeting of the Senate in April as Hörlein had suggested, but only Bothe attended as Kuhn had already departed for Philadelphia. The Senate unanimously ruled in favor of Kuhn, confirming him as permanent managing director of the MPI and allocating space in the MPI (pending the departure of the Americans) according to Kuhn's preferences, including the preservation of Hausser's section. Bothe had no choice but to publicly accept the decision, which Hahn immediately conveyed to Kuhn in Philadelphia in the hope that, having had all his demands fulfilled, this would be enough to persuade him to stay in Heidelberg.[130] Later the deputy rector of Heidelberg University, Karl Geiler, assured Kuhn that the MPG would support his MPI with »very considerable resources«, should Kuhn decide to stay.[131] Before leaving, Kuhn had told Hausser, who passed this on to Hörlein, that he would try to postpone his final decision on the American position.[132] Nevertheless, subsequent correspondence with Kuhn gave Hörlein the impression in August 1950 that he would decide for a permanent professorship at Penn as soon as a new laboratory building was available, and Hörlein therefore saw no reason

128 Kuhn to Hahn, 10 Jan. 1950, AMPG, III. Abt., Rep. 25, Nr. 389; Kuhn to Hahn, 16 Jan. 1950, ibid.; Hahn to Kuhn, 19 Jan. 1950, ibid.

129 Isolde Hausser to Otto Hahn, 18 Feb. 1950, AMPG, III. Abt., Rep. 3, Nr. 18; Hahn to Hausser, 24 March 1950, ibid.; Hausser to Hahn, 17 April 1950, ibid.; Hausser to Max von Laue, 17 April 1950, with cc to Butenandt, Heisenberg, Hörlein, Rajewksy, Schreiber, and Windaus, ibid.; Hausser to Kossel, 18 April 1950, ibid.; sympathetic responses from Butenandt, 18 April 1950, ibid; von Laue, also on behalf of Heisenberg (19 April 1950), Hörlein (20 April 1950), Windaus (2 May 1950), ibid.

130 Hahn to Kuhn (c/o György in Villanova, Pennsylvania) (29 April 1950), and enclosed copy of the Senate decision at the meeting of 28 April, AMPG, III. Abt. Rep. 25, Nr. 389; preceding correspondence and documents from January to March 1950, ibid.

131 Karl Geiler (Prorektor, Heidelberg) to Kuhn (copies sent both to the US and to Heidelberg), 1 June 1950, AMPG, III. Abt., Rep. 25, Nr. 36.

132 Hausser to Hörlein, 24 April 1950, AMPG, III. Abt., Rep. 3, Nr. 18.

to make further efforts on Kuhn's behalf.[133] The contrast with Butenandt's approach to Hörlein is striking.

Kuhn's second visit to the USA, from mid-April to mid-June 1950, was inconclusive in regard to the position in Philadelphia, in part because both Mitchell at Penn and Howard at Wyeth left him with some uncertainties regarding the laboratory facilities and financial arrangements he would be offered; in Mitchell's case, however, he received the necessary confirmations just after Kuhn had returned to Heidelberg.[134] Kuhn used a good part of his visit to present lectures on his current work at Penn and in New York at Hermann Mark's Institute of Polymer Research at Brooklyn Polytechnic in April, followed by a lecture tour organized by Mark (on which Daisy did not accompany him due to a lack of travel funds) to three midwestern universities (Illinois-Urbana, Notre Dame, and Wisconsin-Madison) with interesting facilities in chemistry or biochemistry. He was particularly impressed by the LOBUND bacteriological laboratories in Notre Dame, which were superior to any in Europe at that time; he planned to incorporate some related apparatus in the laboratory he was planning for the position in Philadelphia, and two years later he would apply for funds to install similar facilities in the MPI for Medical Research, so as to follow up on the Philadelphia research in Heidelberg.[135]

Immediately after returning to Heidelberg, Kuhn received a definite offer with funding for his supporting staff and laboratory facilities from Mitchell, who had coordinated his proposal with the senior people at Wyeth. He could now offer Kuhn a salary of $17,500 (equivalent to DM70,000) »from all sources«, thus including Wyeth's contribution (perhaps also Rohm and Haas, given Kuhn's previous optimistic references to them, but the documents do not mention them). In addition, he could confirm availability of all »the funds needed to make the necessary building alterations and to obtain the equipment which you require«. And he could begin at any time after 1 July 1950.[136] Although the specific cost of the buildings, equipment, and staff did not appear in Mitchell's letter, Kuhn's expectations were quite high. He had discussed with Howard and György a wide-ranging program that would potentially go well beyond the fairly limited collaborative research project that they had initially considered in 1949. The outline of staffing requirements for 1951, which Kuhn presented to the Wyeth people, foresaw a fairly modest setup in the first half of the year, while the laboratory facilities were being set up, but a very ambitious program for the second half, potentially a transfer of a good part of his research

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133 Hörlein to Kuhn, 10 Aug. 1950, AMPG, III. Abt., Rep. 25, Nr. 228. A copy of Kuhn's letter, to which Hörlein was responding, is not in this volume.

134 Harry S. Howard (Wyeth, Inc.) to Kuhn, 17 March 1950, AMPG, III. Abt., Rep. 25, Nr. 260; Mitchell to Kuhn, 19 June 1950, AMPG, III. Abt., Rep. 25, Nr. 240.

135 Institute of Polymer Research, Brooklyn Polytechnic: Program for Symposium on Physical and Chemical Properties of Polymeric Electrolytes. In AMPG, III. Abt., Rep. 25, Nr. 71. Hermann Mark to Kuhn, 2 letters dated 20 Feb. 1950, AMPG, III. Abt., Rep. 25, Nr. 238; Richard Kuhn to Daisy Kuhn, 11 May 1950 and other letters from this tour, AMPG, III. Abt., Rep. 25, Nr. 266; laboratory plans (one dated 28 May 1951, rest undated), AMPG, III. Abt., Rep. 25, Nr. 71; Kuhn to Generalverwaltung MPG, 26 July 1952, AMPG, II. Abt., Rep. 23, Nr. 14.

136 Mitchell to Kuhn, 19 June 1950, AMPG, III. Abt., Rep. 25, Nr. 240.

group at the MPI for Medical Research to Philadelphia (Table 1). His institute for chemistry had a total of thirty-one on its scientific staff in Heidelberg in 1951.[137]

Table 1. Kuhn's staffing requirements in Philadelphia for the second half of 1951, as envisioned in spring 1950

Section	Director (professor)	Admin. staff	Scientific staff (postdoc) (Wyeth)	Scientific staff (postdoc) (Penn)	Technical staff, male or unsp. (Wyeth)	Tech. staff, male or unsp. (Penn)	Tech. staff, female (Wyeth)	Tech. staff, female (Penn)	Servants, cleaning staff (Wyeth)	Servants, cleaning staff (Penn)	Other
General	n/a	2 (library)	n/a	n/a	9				2		
Analytic	n/a		6		7		6		1		
Animals	1 (P.G.)	1 (secr'ty)	1	?	3	?		?	1	?	?
Organic chem.	1 (R.K.)	1 (secr'ty)	4	2	3	1	2	2	2	1	4 (students)
Expanded program: all of above plus:											
Organic chemistry			2		10				1		6 (students) 4 (guest scientists, other AHP labs)
Protein chemistry	1 (to be appt.)	.5 (secr'ty)	3		6				1		6 (students)
Biochemical genetics	1 (to be appt.)	.5 (secr'ty)	2		3						6 (students)
Totals	4	5	18	2	41	1	8	2	8	1	22 students 4 guests

Kuhn now had to decide whether to go ahead and try to create a research group on this scale in Philadelphia. In his draft reply to Mitchell in July 1950, he began by specifying how he would soon meet with György in Heidelberg to discuss some points about the arrangements for his new position, as well as working out the details of his move to Philadelphia (along with his family and the initial group of his scientific associates) and clarifying his situation with the MPG, his MPI staff, and the university. But he deleted most of the paragraph, leaving a much more concise statement about checking into the immigrant visa requirements for his people, while clarifying his situation in Heidelberg, where there was »no final decision up to date«.[138]

In regard to the University of Heidelberg, Kuhn had made a dramatic gesture before leaving in April 1950. He submitted to the Rector, who happened to be his old rival the biochemist Karl Freudenberg, his resignation from the honorary Ordinarius professorship in the faculty of natural sciences and mathematics that went back to the founding of the KWI for Medical Research. Freudenberg claimed to recall clearly that at that time, Kuhn had promised never to train doc-

137 Professor Kuhn's estimates of personnel requirements (no date, but clearly before 1 Jan. 1951), AMPG, III. Abt., Rep. 25, Nr. 260; compare Tätigkeitsbericht der Max-Planck-Gesellschaft 1946–1951 (Fortsetzung). *Die Naturwissenschaften* 38/18 (1951), 409–431, 420.

138 Handwritten draft, Kuhn to Mitchell, 6 July 1950, AMPG, III. Abt., Rep. 25, Nr. 240.

toral students in chemistry, leaving that privilege solely with Freudenberg.[139] This had become a major issue for Kuhn, as his predoctoral assistants had had to get their degrees at other universities, and his postdoctoral students did not obtain teaching privileges in the natural science faculty at Heidelberg. He was then welcomed into the faculty of medicine with a personal Ordinarius professorship for biochemistry, naturally without an institute or a teaching obligation, but with the right to supervise and examine students in his field. The natural science faculty hastened to confer an Ordinarius professorship of biochemistry on Kuhn, but he refused to accept it or to be listed as a member of the faculty (another contrast with Butenandt, who happily held Ordinarius professorships in both faculties at Tübingen). Negotiations with Freudenberg, however, brought a compromise whereby, in regard to *Diplom* or doctoral students in chemistry who wished to work with him, Kuhn would have the status of an instructor *(Dozent)* of chemistry in the natural science faculty, with equal rights to supervise and examine them.[140]

Kuhn's subsequent negotiations during the second half of 1950 were primarily with Wyeth and AHP, and with György (who also acted as a representative of AHP/Wyeth). In late July, Kuhn joined György and another representative of AHP in a conference with BASF representatives, possibly including Carl Wurster as chairman (Kuhn had asked Howard as president of Wyeth to come to Heidelberg, but he could not attend). This meeting was in order to work out an agreement »for the interchange of products between the two companies.«[141] Kuhn's personal goal in this, as he had proposed to György shortly before leaving Philadelphia, was to set up a deal whereby any results of their collaboration coming mainly from his side would be patented in Germany through BASF and offered to AHP as licensee for North America, while György would patent through AHP in the US, and BASF would be the licensee and distributor in Europe.[142] It appears that the deal worked out between the two companies was simply for AHP to patent all the results in North America (Canada and the USA). Then in October, Kuhn asked György for a written specification of how Wyeth would deal with the royalties for existing products that Kuhn had proposed for patenting, as well as future products that might emerge from their collaboration. He got his answer from the president, Harry Howard, who offered him a share of net sales (up to 3%) for the six current products that Kuhn had listed. But as for future products, Howard was more evasive, suggesting that the consulting salary he was paying Kuhn should suffice as an honorarium for products developed mainly within Wyeth's research team. Additional royalties would only go to Kuhn for »major products that come to Wyeth through your contribution, that are separate and distinct from developments from within.«[143]

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139 Karl Freudenberg to Kuhn, 26 April 1950, AMPG, III. Abt., Rep. 25, Nr. 222.

140 Klaus Schäfer (Dekan, Naturw.-math. Fakultät) to Karl Geiler, 15 July 1950, AMPG, III. Abt., Rep. 25, Nr. 36.: The German *Diplom* was somewhat comparable to a master of science or engineering in an American university, but it could also confer the professional status derived from a licensing or certifying examination in an Anglo-American context.

141 Alvin G. Brush (Chairman, AHP) to Kuhn, 18 July 1950, AMPG, III. Abt., Rep. 25, Nr. 210.

142 Cf. Kuhn to György, 12 June 1950, AMPG, III. Abt., Rep. 25, Nr. 227.

143 Howard to Kuhn, 27 Oct. 1950, AMPG, III. Abt., Rep. 25, Nr. 260.

Following the AHP-BASF negotiations in July 1950, Kuhn dealt individually in November 1950 with the BASF, in the person of its board chairman Carl Wurster, to work out an agreement that would guarantee ongoing support for his research. This specified that beginning in 1951, the BASF would pay Kuhn 18,000 DM per year in quarterly instalments to subsidize research in the area of organic and biological chemistry.[144] The detailed statement of the agreement specified three points: 1) BASF would continue to support the research of Kuhn's MPI by supplying it with free chemicals »to the previous extent« (implying an established pattern of support, probably going back to prewar practice), to be furnished by the BASF Hauptlaboratorium. 2) Kuhn would continue to perform »investigations« on behalf of BASF; these would be coordinated with and subsidized by the director of the BASF Pharmacological Institute, Prof. Dr. Oettel. 3) Correspondence between Kuhn's MPI and BASF would go through the Patent Department (Dr. Kleber), who would see to »rapid processing in connection with the relevant offices of our works«.[145] The new agreement evidently served to renew a corporate relationship with Kuhn going back to the late 1930s, and which (as in the case of Butenandt with the Triple Agreement), saw Kuhn submitting some of his research results to the BASF before publication.[146] Wurster clearly expected a continuing flow of useful results from Kuhn's work, which had already produced patent submission for the BASF in the late 1940s, with others to follow in 1951 and beyond. In February 1951 the Patent Department notified him that he had earned royalties in the amount of around 1400 DM from sales in the years 1948 to 1950; these payments presumably related to a patent submitted earlier by the IG Farben concern.[147] These were in addition to the annual subsidy, which the BASF continued to pay Kuhn in the amount of 18,000 DM until, following Wurster's retirement, the new chairman Bernhard Timm terminated the agreement in 1965.[148] These payments went to Kuhn directly, not to his institute's research budget, and in that sense they differed from the payments of the Triple Agreement firms for Butenandt's insti-

144 Wurster and Kleber (BASF) to Kuhn, 23 Nov. 1950, AMPG, III. Abt., Rep. 25, Nr. 213. By 1951 Wurster was also an MPG senator. Max-Planck-Gesellschaft zur Förderung der Wissenschaften (ed.): *Mitglieder-Verzeichnis nach dem Stand vom 1. Mai 1951*. Göttingen 1951.

145 Wurster and Kleber to Kuhn (23 Nov. 1950, Betreff: Patentabteilung / Zuordnung der Korrespondenz), BASF UA, PB W.1.3./(unverzeichnet): Aufsichtsrat Richard Kuhn.

146 For example, Kuhn to Direktor Dr. Reppe, Hauptlaboratorium I. G. Farbenindustrie A. G. Ludwigshafen am Rhein (22 May 1941), submitting a manuscript on polymerization (Über alpha-Vinylthiophen) for review prior to publication in *Liebigs Annalen*, BASF UA, W.1.3./(unverzeichnet): Richard Kuhn.

147 Kleber and Freiensehner to Kuhn, 23 Feb. 1951, AMPG, III. Abt., Rep. 25, Nr. 213. This message specifies that the payment relates to a percentage of the sales of an antiseptic ointment, Dibromsalicil, but does not specify the patent related to that product, and I have been unable to identify such a patent in Steinhauser's list of Kuhn's patents that were submitted by BASF or IG Farben prior to 1948. Rather than marketing the ointment directly, BASF may have supplied other firms. Cf. Dibrosal. Ein neues Antibiotikum mit zurverlässiger Wirkung (no author, no date [1949 or 1950]). Commercial leaflet in AMPG, Rep. 25, Nr. 236. BASF continued to produce this ointment until 1959, when it sold its rights (and the obligation to pay royalties to Kuhn) to Riedel-de Haën AG in Hanover. Riedel-de Haën AG to Kuhn, 26 Jan. 1960, AMPG, III. Abt., Rep. 25, Nr. 247.

148 Timm and Steinhofer (BASF AG) to Kuhn, 22 Nov. 1965, BASF UA, PB W.1.3./(unverzeichnet): Richard Kuhn; Werner Abelshauser: BASF Since Its Refounding in 1952. In: Werner Abelshauser et al. (eds.): *German Industry and Global Enterprise. BASF: The History of a Company*. Cambridge: Cambridge University Press 2004, 362–620, 367. See also patent correspondence and related documents from 1951 to 1965 in BASF UA, PB W.1.3/(unverzeichnet): Aufsichtsrat Richard Kuhn, Veröffentlichungen Forschung I und II.

tute; moreover, unlike Butenandt, Kuhn was not obligated to submit all of the research results of his institute to the BASF for review prior to publication.

Wurster also brought Kuhn into closer connection with the BASF by appointing him chairman of the supervisory board (Aufsichtsrat) at the company's refounding in 1952, in conjunction with its legal separation from the now-dissolved IG Farben concern. Wurster wanted the company to benefit not only from Kuhn's reputation as a Nobel laureate, but also from his »knowledge of the research terrain«. It is perhaps not a coincidence that although the BASF generally avoided pharmaceuticals during this period, there were some exceptions to this policy, in particular a group of products in Kuhn's special area of expertise, vitamins and beta-carotene, which it supplied to Hoffmann-LaRoche.[149] Kuhn remained on the supervisory board at least until 1965, by which time he was enjoying a substantial bonus as a percentage of the dividends paid by the company.[150] Butenandt joined the supervisory board of Bayer (then under the chairmanship of Hörlein) at its (re-)founding in December 1951, and he remained a member until 1974, but I only have evidence that he attended meetings in 1953 related to the separation of Bayer from the dissolved IG Farben concern.[151] As long as Butenandt remained as a professor in Tübingen, he was legally forbidden to earn any income from his supervisory board activity, which may have been yet another incentive to accept the call to Munich.[152]

Leaving aside other considerations already noted, it is possible that the prospect of continuing research support for Kuhn from BASF, and the fact that it was essentially tied to his remaining in Heidelberg, played a significant role in Kuhn's decision not to take the permanent research professorship at Penn, which Dean Mitchell of the medical school offered him and which György strongly urged him to accept in the winter of 1950–1951. Initially Kuhn had arranged for immigrant visas and permanent positions for his family members and his assistants from the MPI for Medical Research who were to accompany him to Philadelphia in the spring of 1951. But after several urgent letters from György, who warned him that his delay was creating a bad impression on the Penn medical school faculty, Kuhn finally told him and Mitchell that he wanted to convert the permanent position into a visiting professorship for one year, as of 1 Janu-

149 Abelshauser, BASF, 2004, 362–620, 366, 375, 599–600. BASF supervisory board minutes from the years of Kuhn's chairmanship, not currently available to me, would provide greater insight into the question of Kuhn's impact on the company.

150 BASF to Kuhn, 12 May 1965, AMPG, III. Abt., Rep. 25, Nr. 213. The amount in question was DM 7,500 as the base honorarium, with a bonus of more than DM 63,500 from dividends.

151 See correspondence (all from 1953) between Butenandt and Hörlein, AMPG, III. Abt., Rep. 84/1, Nr. 286, fol. 308–313. For dates of the beginning and end of Butenandt's membership: personal communication from Thore Grimm, head of the Bayer Archives, 20 Dec. 2022, with BAL Photo 0-14407 (Gründungsversammlung der Farbenfabriken Bayer am 19. 12. 1951). Unfortunately he was unable to provide any additional information about the possible influence of Butenandt on research policies at Bayer.

152 Württemberg-Hohenzollern Kultusminister Sauer to Butenandt, 7 May 1952, cc in University Archives, Tübingen (henceforth: UAT), 126a/59, fol. 35.

ary (though Kuhn would not arrive until April), with the possibility of renewal.[153] Mitchell accordingly changed the terms of the appointment, and he also converted two of the three positions for Kuhn's German assistants into similar visiting research associates or one-year research fellowships. Only one, Hans W. Ruelius, chose to move permanently to Philadelphia and was thus given a continuing position as research associate.[154] The change also meant a significant scaling-back of Kuhn's earlier plans, which had entailed an initial cost of $750,000 for laboratories, furnishings, apparatus, and supplies, plus annual salaries of $150,000 for eleven scientists and twenty supporting staff. Once Kuhn had arrived and worked out specific plans for modifying three laboratory rooms in a building of the medical school, the cost came to only $38,878.[155]

Kuhn had been very reluctant to discuss the details of his proposed collaboration with the General Administration of the MPG in Göttingen. Thus when Otto Hahn read a newspaper article referring to Kuhn's »permanent guest professorship« and his intention to move his family to America in the fall of 1951, he had to ask Kuhn to explain what his actual plans were.[156] Kuhn hastened to reply that there was no permanent guest professorship, but he declined to provide details in writing, only that the collaborative research he was undertaking in Philadelphia could only be done in America. He mentioned neither Wyeth nor György. Moreover, he urgently requested that nothing be publicized until it was clear that the work would produce something of scientific value.[157]

It appears that Kuhn's change of mind may have had a negative effect on his collaboration with György. Although their joint work produced numerous publications and patents, there appears to have been a growing tension between the two. At the end of June 1951, they had an argument over the division of AHP patent royalties, and György told him that all the samples and patent applications he had brought to the collaboration were worth no more than five cents. So Kuhn wrote up a note to György offering to buy everything back for a five-cent piece, which he would send under separate cover. But a patent attorney's report a week later effectively confirmed György's view by indicating that one of Kuhn's patent applications was certain to be rejected,

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153 Donald K. Angell (Vice President and Secretary, University of Pennsylvania) to Kuhn, 19 March 1951, indicating his appointment would continue until 30 June 1952, AMPG, III. Abt., Rep. 25, Nr. 254. Letters from György to Kuhn dated 5 Jan. 1951, 11 Jan. 1951, 18 Jan. 1951, 23 Jan. 1951, 1 Feb. 1951, and 13 Feb. 1951, AMPG, III. Abt., Rep. 25, Nr. 227. Copies of Kuhn's communications to György in this period are not present in this volume, but their content can be inferred from György's letters. Kuhn had initially wanted his appointment at Penn and his contract with Wyeth to be backdated to 1 October 1950, but György strongly dissuaded him from asking for this, so that he settled on 1 January as a compromise (which was nevertheless a significant concession, given that Kuhn did not take up his duties in Philadelphia until April 1951).

154 György to Kuhn, 23 Feb. 1951, AMPG, III. Abt., Rep. 25, Nr. 227; Mitchell to Kuhn (2 letters, 23 Feb. 1951), and copies of letters of appointment from Mitchell to Karl Dury, Friedrich Zilliken, and Hans Ruelius, all dated 28 Feb. 1951, AMPG, III. Abt., Rep. 25, Nr. 240. Note that Günter Quadbeck, who had been included in Kuhn's original list of assistants, apparently did not accompany him to Philadelphia.

155 Contrast University of Pennsylvania. School of Medicine: Estimation of costs (no date, probably fall 1950) to Alterations to Room 415, 402, and 407, 29 May 1951, both in AMPG, III. Abt., Rep. 25, Nr. 240.

156 Otto Hahn to Richard Kuhn, 16 July 1951, AMPG, II. Abt., Rep. 23, Nr. 13.

157 Richard Kuhn to Otto Hahn, 18 July 1951, AMPG, II. Abt., Rep. 23, Nr. 13.

and suggested that it would not be worth doing the additional experiments necessary to make a better case for acceptance.[158]

In May 1952, the executive council of the Penn School of Medicine approved Kuhn's reappointment as visiting research professor for an additional year, through June 1953.[159] At that time, Kuhn was working in the improved laboratory facilities of the Wyeth Institute for Applied Biochemistry in Philadelphia, with his principal focus on the attempt to isolate and chemically identify nutritional factors in human milk that could be used to improve infant formula, and in the expectation of good results Wyeth had prepared for Kuhn a new consultant's agreement, dated 31 March 1952, on patents to be taken out by AHP for what they were now calling the »György breast milk factor«. Kuhn was obliged to agree that, in return for Wyeth's financial support for his research, any inventions or discoveries he might make in connection with the project would be the property of Wyeth.[160] AHP then successfully applied for several patents on topics vaguely designated »food compositions« or with reference to biochemical aspects of nutrition, naming Kuhn, György, and assistants as inventors.[161] After this initial burst in 1952, however, there were no more patents naming Kuhn until 1956, and this one appears to have resulted from a reworking of processes that Kuhn had tried unsuccessfully to patent with György's help in 1953, only to be told by Wyeth's patent attorneys that from the perspective of the Patent Office, what Kuhn had done did »not amount to invention«.[162] Thus it must have seemed to Kuhn that after 1952, aside from the potential for scientific results from the chemical investigation of breast milk, the collaboration was not going to be of much commercial benefit to himself. Hence, he no longer wanted to deal directly with Wyeth, and in the spring of 1954, he asked Wyeth to renegotiate the consultant's agreement so that it would designate him as an agent of BASF, whereby any commercial results of his research would be transferred to Wyeth/AHP through BASF.[163]

After 1951 György had become increasingly negative about Kuhn's contributions to the project, and the letters exchanged between the two men were greatly diminished in scope and tone.

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158 CCs of Kuhn to György, 30 June 1951, AMPG, III. Abt., Rep. 25, Nr. 277; Kuhn to György, 3 July 1951, ibid.; Louis H. Baer to György, 11 July 1951, ibid.

159 University of Pennsylvania. School of Medicine. Medical Council and Executive Committee: Minutes, Vol. 5 (1948–1953), Meeting of 12 May 1952, 324, UPA, UPC 2.1 School of Medicine. Faculty Minutes, 1800–1976, Box 3.

160 Wyeth (Vice President, signature illegible) to Kuhn, 3 March 1952; Consultant's Agreement, 31 March 1952; H.S. Howard to Kuhn, with Kuhn's signature in concurrence, 27 March 1952, AMPG, III. Abt., Rep. 25, Nr. 260; Wyeth Laboratories (H. W. Blades, Executive Vice President) to Kuhn, 14 March 1955, AMPG, III. Abt., Rep. 25, Nr. 325.

161 Thomas Steinhauser (comp.), Patente Richard Kuhn gesamt, 25.Okt.2017 [Compiled from European Patent Office database using Espacenet search function]. This list, covering patents issued after the end of the war, includes nine US patents and 1 German patent applied for by AHP in 1951 and 1952 naming Kuhn with (usually) György and others as co-inventors, but only one additional AHP US patent naming Kuhn, applied for in 1956.

162 Edmund H. O'Brien to György, 26 Aug. 1953, copy in AMPG, III. Abt., Rep. 25, Nr. 260.

163 Wyeth Laboratories (Blades) to Hermann Kleber (BASF), 7 June 1954, AMPG, III. Abt., Rep. 25, Nr. 260; Wyeth Laboratories (Blades) to Kuhn, 7 June 1954, ibid.

Although Kuhn successfully nominated György to be an external scientific member of the MPI for Medical Research, going back to György's suggestion that he could thereby spend time working with Kuhn in Heidelberg when Kuhn was not in Philadelphia, the appointment took a long time to process, which left György increasingly frustrated.[164] There is no indication that György took advantage of the opportunity for an extended visit to Heidelberg when Hahn finally notified him of his designation as external scientific member of the MPI for Medical Research in July 1952.[165] Kuhn's last extended stay in Philadelphia was from late October 1952 to February 1953, followed by a stay of only four weeks in 1954. When György's last letter arrived in August 1953 (and there had been no other since July 1952), it was addressed not to »*Lieber Herr Kuhn*« as in all previous correspondence, but rather to the more formal »*Sehr geehrter Herr Professor Kuhn*«. It closed with a request for Kuhn's written consent to publish all the virus experiments they had done together on milk and related products first in the USA, because in any case »the ways we formulated the questions all go back to my suggestions«; of course, that would not exclude the possibility, if Kuhn wished and with prior agreement, that he and his group could independently publish the »purely chemical results«.[166] If Kuhn replied to this, he did not save a copy.

Despite the evident tensions between Kuhn and György, their collaboration did reach a conclusion in the form of a series of journal articles published in 1953–1954, reporting on the main points of their investigation into the essential growth factor for the *Lactobacillis bifidus* var. *Penn* (later termed *Bifidobacterium bifidum*), found in human milk and critical for the development of the infant gut microbiome. As desired by György, all the papers appeared in American journals, first in the widely-read *Science* (on the extent to which different human milk fractions inhibited influenza and other virus multiplication, and citing the other, more detailed papers then in press) and *Archives of Biochemistry and Biophysics* for the other five papers in 1954. Kuhn and at least one of his assistants were co-authors with György and at least one of his assistants for all but the first bifidus factor paper, which was solely a product of György's group. The more subordinate placement of Kuhn's name in the author listings for most of the papers, as compared with György's, reflected his primary role as a consultant rather than lead investigator. Only in the fourth paper on the bifidus factor was Kuhn's and his assistants' affiliation given as the Institute for Chemistry of the MPI for Medical Research; in the rest it appeared as the Wyeth Institute for Applied Biochemistry and the Department of Physiological Chemistry at the Penn Medical School. The fourth bifidus factor paper, however, was chemically the most significant for Kuhn, as it indicated the path toward further investigation of the structure and possible synthesis of human milk oligosaccharides (HMOs) as the bifidus growth factor.[167] A recent survey

164 György to Kuhn, 1 Aug. 1951 and György to Kuhn, 31 Jan. 1952, AMPG, III. Abt., Rep. 25, Nr. 277.

165 Cf. Seeliger to Kuhn, 24 July 1952, AMPG, II. Abt., Rep. 23, Nr. 14; György to Kuhn, 21 July 1952, AMPG, III. Abt., Rep. 25, Nr. 277 (György's letter mentioned a long follow-up letter to come in a few days, in response to Kuhn's previous letter, but the next letter in the volume is dated more than a year later).

166 György to Kuhn, 11 Aug. 1953, AMPG, III. Abt., Rep. 25, Nr. 277.

167 Klaus Hummeler et al.: Fractions of Human Milk and Virus Multiplication. *Science* 118/3078 (1953), 781–782. doi:10.1126/science.118.3078.781; Paul György, Robert F. Norris, and Catharine S. Rose: Bifidus Factor. I. A Variant of Lactobacillus

of research in this field considers the collaborative papers of György and Kuhn to be the first »real breakthrough in HMO research«.[168]

Although the series of papers in 1954 marked the end of the collaboration between Kuhn and György, both groups continued to work in their own way on the components of human milk. Kuhn and his associates in Heidelberg researched the chemical nature of the oligosaccharides and related substances, and in the process Kuhn collaborated locally, with with the Women's Clinic of the University of Heidelberg. But Kuhn's activity reports never mentioned Wyeth, American Home Products, or the Wyeth Institute for Applied Biochemistry in Philadelphia. By 1956 Kuhn and his colleagues were able to determine the structure of »several« HMOs (more than two hundred of these are currently known), and they continued to work in this area, but a commercially practical synthesis remained elusive; there has been no breakthrough to mass-produced, synthetic HMOs for infant formula.[169]

Kuhn had become more willing to remain in Heidelberg after the long-awaited departure of the American medical laboratory from the MPI building in June 1952. It was now possible to expand the building and think about hiring new directors to revive the vacant physiology and pathology institutes. The situation had become even more fluid through the death of Isolde Hausser on 5 October 1951, which would free the rooms of her small section for other uses.[170] Intensive discussion on modifications and new appointments to the MPI now began in earnest, and led to an expansion designed by the MPI's original architect, Hans Freese. As noted earlier, Kuhn was now willing to see Hermann Rein become Meyerhof's belated successor as director of a revived institute for physiology. Rein's death in May 1953 left that position vacant again, to be filled in May 1954 by Meyerhof's old friend Hans Hermann Weber, at that time in Tübingen. Weber's interest in the physiology of muscles brought the institute back to one of Meyerhof's main areas of research. Walther Bothe was able to resume full activity in the MPI's institute for

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Bifidus Requiring a Special Growth Factor. *Archives of Biochemistry and Biophysics* 48/1 (1954), 193–201. doi:10.1016/0003-9861(54)90323-9; Paul György et al.: Bifidus Factor. II. Its Occurrence in Milk from Different Species and in Other Natural Products. *Archives of Biochemistry and Biophysics* 48/1 (1954), 202–208. doi:10.1016/0003-9861(54)90324-0; Paul György et al.: Bifidus Factor. III. The Rate of Dialysis. *Archives of Biochemistry and Biophysics* 48/1 (1954), 209–213. doi:10.1016/0003-9861(54)90325-2; Adeline Gauhe et al.: Bifidus Factor. IV. Preparations Obtained from Human Milk. *Archives of Biochemistry and Biophysics* 48/1 (1954), 214–224. doi:10.1016/0003-9861(54)90326-4; Catharine S. Rose et al.: Bifidus Factor. V. The Activity of α- and β-Methyl-N-Acetyl-d-Glucosaminides. *Archives of Biochemistry and Biophysics* 49/1 (1954), 123–129. doi:10.1016/0003-9861(54)90173-3. Cf. Kuhn to Generalverwaltung, 4 May 1956, submitting his institute's Tätigkeitsbericht (1. 4. 1954–31. 3. 1956) and two lists of publications from the MPI f. Medizinische Forschung, Institut f. Chemie: 1 April 1954–31 March 1955 and 1 April 1955–31 March 1956, AMPG, II. Abt., Rep. 23, Nr. 17. Only the first list includes collaborative publications with György (those mentioned above); note that I am listing all the authors of these papers to indicate the pattern of collaborative research between the Kuhn and György research groups.

168 Clodagh Walsh et al.: Human Milk Oligosaccharides: Shaping the Infant Gut Microbiota and Supporting Health. *Journal of Functional Foods* 72 (2020), 1–13, 3. doi:10.1016/j.jff.2020.104074.

169 Kuhn, Tätigkeitsbericht (1. 4. 1954–31. 3. 1956), AMPG, II. Abt., Rep. 23, Nr. 17; Walsh et al., Human Milk Oligosaccharides, 2020, 1–13, 1; Renate Akkerman, Marijke M. Faas, and Paul de Vos: Non-Digestible Carbohydrates in Infant Formula as Substitution for Human Milk Oligosaccharide Functions: Effects on Microbiota and Gut Maturation. *Critical Reviews in Food Science and Nutrition* 59/9 (2019), 1486–1497. doi:10.1080/10408398.2017.1414030.

170 Attachment to: Kuhn to Telschow, Philadelphia, 11 June 1952, AMPG, III. Abt., Rep. 25, Nr. 277.

physics, evidently still resentful of Kuhn's position as senior director of the MPI.[171] Although in principle the MPI was again beginning to resemble the prewar KWI for Medical Research, in practice it appears that there was no significant collaboration between the sub-institutes. Bothe in particular, after receiving the Nobel Prize in Physics in 1954, could make an even stronger case for establishing an independent MPI for Nuclear Physics in Heidelberg, but this only occurred after his death in February 1957. Bothe's successor in 1958 was Wolfgang Gentner, initially as provisional director of the institute for physics within the MPI for Medical Research and then as director of the new MPI. Gentner's group continued to make use of some of the rooms of the former institute for physics, as construction on his MPI did not begin until 1960, in a forest area a considerable distance from the MPI for Medical Research.[172]

From Kuhn's perspective, perhaps the most significant developments of the 1950s in his institute were new and expensive apparatus and instrumentation, which became possible because the federal government allocated Marshall Plan (European Recovery Plan or ERP) funds to the education ministries of six *Länder* to support investments in scientific research.[173] This allowed Kuhn to make the sort of investments in hardware that Butenandt had made a few years earlier with the support of his Triple Agreement industrial patrons in Tübingen, but without having to appeal to industry (another reason why Kuhn's situation in Heidelberg began to look better than the one in Philadelphia). Kuhn could easily find DM 200,000 worth of apparatus to request, including a Perkin-Elmer infrared spectrograph (useful in biochemical analysis to determine the components of complex mixtures), an electron microscope (especially helpful in antibiotic research), two LOBUND bacteriological laboratory units similar to those he had seen at the University of Notre Dame in 1950 but not yet available in Europe (facilitating research with highly toxic substances, anaerobic bacteria, and intestinal flora, as with his human milk research), and two centrifuges (one of which would have to replace a newly inoperable device in the MPI). These requests were based on an allocation of DM150,000 made by the Senate to furnish the future expansion of the MPI, pending the departure of the Americans, but Kuhn was taking advantage of the availability of ERP funds to request the equipment immediately, as he realized that the Perkin-Elmer device would be paid in dollars.[174]

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171 Correspondence with the MPG administration in Göttingen from 1952–53, AMPG, II. Abt., Rep. 23, Nr. 14; Weber's correspondence as director of his institute, 1956–1967, AMPG, II. Abt., Rep. 23, Nr. 16. For a sketchy survey of the development of the MPI from its founding to the mid-1950s see Kuhn, 25 Jahre, 1955, 69–99.

172 Wolfgang Gentner: Max-Planck-Institut für Kernphysik in Heidelberg. In: Generalverwaltung der Max-Planck-Gesellschaft (ed.): *Jahrbuch der Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1961*. Vol. 2. Göttingen 1962, 486–491.

173 Copy (sent to the MPI for Medical Research by Seeliger of the MPG in Göttingen on 22 Jan. 1951) of Bundesminister des Innern to six education ministries, 4 Jan. 1951, AMPG, II. Abt., Rep. 23, Nr. 13.

174 Kuhn to Generalverwaltung MPG, 16 March 1951 and 31 March 1951, AMPG, II. Abt., Rep. 23, Nr. 13; request repeated with more supporting details, 7 May 1951, ibid. The cost of the Perkin-Elmer spectrograph, DM48,000, effectively increased the original DM150,000 allocation to the amount Kuhn was now requesting. On the LOBUND units see also Kuhn to the Generalverwaltung MPG, 26 July 1952, AMPG, II. Abt., Rep. 23, Nr. 14.

A few years later, Kuhn requested funding to purchase one of the early nuclear magnetic resonance (NMR) spectrometers produced by Varian Associates in California, based on a technology adapted from wartime radar research by American physical organic chemists, who had applied it to the structural analysis of organic compounds. Although Kuhn's institute (along with the University of Tübingen) had been a pioneer in Germany in the study of magnetic resonance, it would have been impractical to build their own NMR device. And although such devices were difficult to use, Kuhn had an assistant, Dr. H. Kainer, who had just spent a year in Oxford learning how to operate one.[175] Another member of the institute staff, Karl Hermann Hausser (Isolde's son), was also very interested in applying physical methods to the solution of chemical problems, and accordingly Kuhn sent him to Oxford in the summer of 1956 to participate in a course on Theoretical Chemistry, in which, like Kainer, he trained with a Varian NMR device while also improving his skills in electron paramagnetic resonance (EPR) spectroscopy.[176] During a lecture trip to California in the spring of 1956, Kuhn initiated negotiations to purchase the Varian apparatus.[177] This became the foundation of what ultimately became Hausser's influential section for NMR and EPR analysis at the MPI, for which he was appointed a scientific member and director in 1966.[178] Thus began, along with Weber's retirement as director and the division of his physiological institute into two sections headed by his former assistants Wilhelm Hasselbach and Hartmut Hoffmann-Berling, as well as the establishment of a guest section for biophysics to be headed by the Cambridge-trained phyicist and molecular biologist Kenneth Holmes, the transition to a more diverse and collegially managed MPI for Medical Research, which was not completed until after Kuhn's death in July 1967.[179]

On the fiftieth anniversary of the founding of the KWG in 1961, Kuhn reflected publicly on the development of his institute, considering its scientific significance and, by implication, the legacy of his own leadership. In doing so, he implicitly compared his own work to that of his closest counterpart in the MPG, Adolf Butenandt, who had only recently become president of the MPG. Kuhn pointed out that his own Institute for Chemistry had become »in a certain sense« another Institute for Biochemistry of the MPG, reflecting a current trend (as recommended by the national *Wissenschaftsrat*) toward the creation of multiple institutes covering different aspects of a single discipline. But he immediately added that his institute also dealt with »theo-

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175 Kuhn to Pfuhl (Generalverwaltung, MPG), 24 Oct. 1955, and enclosed Apparatur für magnetische Resonanzmessungen, 24 Oct. 1955, AMPG, II. Abt., Rep. 23, Nr. 15. For the broader context of these innovations, see Carsten Reinhardt: *Shifting and Rearranging. Physical Methods and the Transformation of Modern Chemistry*. Sagamore Beach, MA: Science History Publications 2006; Thomas Steinhauser: *Zukunftsmaschinen in der Chemie. Kernmagnetische Resonanz bis 1980*. Frankfurt am Main: Peter Lang 2014.

176 Kuhn to Generalverwaltung MPG, 4 May 1956, AMPG, II. Abt., Rep. 23, Nr. 17. Cf. Karl Hermann Hausser: Elektronen- und Kernresonanz als Methode der Molekelforschung. *Angewandte Chemie* 68/23 (1956), 729–746. doi:10.1002/ange.19560682303.

177 I have not yet found in the MPI files Kuhn's statement of the actual cost of the Varian NMR device.

178 Gisbert Freiherr zu Putlitz: Nachruf auf Karl Hermann Hausser. *Physikalische Blätter* 57/5 (2001), 65. doi:10.1002/phbl.20010570523.

179 Generalverwaltung der Max-Planck-Gesellschaft (ed.): Die Max-Planck-Gesellschaft zur Förderung der Wissenschaften e. V. im Jahre 1966. *Jahrbuch der Max-Planck-Gesellschaft 1967*. Göttingen: Hubert & Co 1967, 7–23, 16–17.

retical issues of organic chemistry« and included areas of research overlapping with inorganic and analytic chemistry, the latter providing crucial tools for the structural analysis of significant biological molecules such as the vitamins and enzymes, and of course the HMOs and other natural substances that helped to strengthen human immunity. He even mentioned »antivitamins«, substances that counteracted the role of vitamins and could thereby have bacteriostatic effects with significant medicinal value (not, of course, mentioning their value for his wartime nerve gas research).[180] One area of research was, however, conspicuous by its absence: biochemical genetics, for which two decades earlier he had anticipated revolutionary results through his collaboration with Moewus. In 1954, he had mentioned genetics in a different context, a discussion of his mentor Richard Willstätter's work in Dahlem forty years before. At that time Willstätter had studied the chemistry of anthocyanin pigments extracted from live flowers, cultivated in the gardens adjacent to his institute. It was a pathbreaking investigation that opened the way to other studies of biological substances in their natural context, but Willstätter had focused on the issues most interesting to organic chemists, from structural analysis to synthesis. While this might have exhausted the subject for classical organic chemists, more recent research was examining the question of why plants produced such pigments and not others, thus raising a new set of questions related to genetics. And in the case of the current research dealing with similar biochemical-genetic questions in the MPG, Kuhn pointed to Butenandt's work with the eye pigments and secretions extracted from butterflies grown in Tübingen, in collaboration with the MPI for Biology.[181] In this sense, Butenandt's research had carried on the earlier spirit of Dahlem and Emil Fischer's goal of investigating the chemical basis of heredity; but Kuhn was tacitly admitting that he had effectively abandoned that goal. Thus, it is necessary now to return to Butenandt's career and institute as they moved from Tübingen to Munich in the mid-1950s, and to consider how this may have opened the way toward another »new Dahlem« for the MPG.

180 Richard Kuhn and Hermann Weber: Max-Planck-Institut für medizinische Forschung in Heidelberg. In: Generalverwaltung der Max-Planck-Gesellschaft (ed.): *Jahrbuch 1961 der Max-Planck-Gesellschaft zur Förderung der Wissenschaften.* Vol. 2. Göttingen 1962, 535–556, 541; cf. Schmaltz, *Kampfstoff-Forschung*, 2017.

181 Richard Kuhn: Richard Willstätter (12. 8. 1872–3. 8. 1942). Von den Jahren in Dahlem (1912–1916). *Mitteilungen aus der Max-Planck-Gesellschaft zur Förderung der Wissenschaften* Heft 3 (1954), 131–136, 135.

3 From Tübingen to Munich: consolidating biochemical institutes in the mid-1950s

Whereas Kuhn remained in Heidelberg, Butenandt had larger ambitions. When he first began to negotiate moving his institute to Munich and accepting the professorship of physiological chemistry at the LMU in 1952–53, he began to realize the possibilities of consolidating several biochemical institutes in that city. This was the germ of the idea of creating yet another »new Dahlem«, a collaborative community of institutes for biochemists that had existed in the original Dahlem, but which Butenandt had found it impossible to achieve in Tübingen. Butenandt was evidently impressed by the superior resources of the Bavarian government (supplemented by business and industrial support), and its apparent willingness to collaborate with him in developing a community of research institutes in Munich.[182]

3.1 Toward a new Dahlem in Munich? Integrating a biochemical MPI into the rebuilding of the university's physiological institute complex

The process of negotiation with the faculty and authorities in Munich is highly revealing of Butenandt's style and his ability to work within the cultural context of the times. Hence it merits a somewhat detailed discussion. In January 1952, Wolfgang Laves, professor of judicial medicine and Dean of the Medical Faculty of the University of Munich, obtained from Butenandt an assessment of potential appointees to the chair of physiological chemistry vacated by the death of Amandus Hahn (1889–1 Jan. 1952).[183] Although Hahn had only been an extraordinarius (associate) professor, the Medical Faculty in its meeting of 5 March 1952 unanimously decided to recommend the appointment of Butenandt himself, *primo et unico loco*, to the chair – presumably to be elevated to an ordinarius – and to be director of the institute for physiological chemistry (Laves, however, omitted the fact that it had been destroyed in the war). Laves sent Butenandt a list of the required paperwork (to be submitted in four copies) in order to initiate negotiations, including a political questionnaire, the judgment of the denazification court (*Spruchkammer*) in his case, and a sworn affidavit that he had not been dismissed by the occupation authorities for political reasons.[184]

Butenandt evidently weighed his response very carefully, because he took more than two weeks to reply, and his response was initially equivocal. While he was honored by the recommendation and would be strongly attracted to Munich, the faculty, and the university, Butenandt emphasized his great reluctance to leave »a place of work, that in our profession is almost

182 For these developments within the broader context of the MPG's development in the 1950s, see Jaromír Balcar: *Wandel durch Wachstum in »dynamischen Zeiten«. Die Max-Planck-Gesellschaft 1955/57 bis 1972*. Berlin: GMPG-Preprint 2020, 41–47.

183 Laves to Butenandt, 30 Jan. 1952, AMPG, III. Abt. Rep. 84/1, Nr. 689, fol. 37.

184 Laves to Butenandt, 10 March 1952, UAM, N-IX Butenandt, Bd. 1.

unique in Germany«, so that he considered it »highly improbable« – he did not say impossible – that he could accept the call. He went on to describe the advantages of his joint appointment in Tübingen, combining »a fully renovated and modernized university institute for physiological chemistry« with »above all, ... my old Dahlem Kaiser Wilhelm Institute for Biochemistry«, reestablished as an MPI located partly in the university institute, partly in a new building [meaning the section for virus research]. Butenandt was especially pleased to be able to combine research in his MPI with teaching medical students and chemists, who would become the next generation in his field. He could only have reached this goal, he emphasized, through the understanding and support of the education ministry, and through the »financial help of private Württemberg groups based on the united efforts of the Medical and Natural Science Faculties.« Not only did he feel an obligation to stay in Tübingen in gratitude for this support, but he »could only with difficulty separate myself« from the MPI for Biochemistry, »which I rescued with so much effort from the turmoil of war, and which has now found its ultimate location in Tübingen.« In view of the expenses for the rebuilding of the chemistry institute in Munich, Butenandt seriously doubted that the Bavarian education ministry could build an institute for physiological chemistry comparable to that in Tübingen. Hence in the interest of honest negotiations, he wanted to make it clear to the faculty that »the plan for my move to Munich probably cannot be carried out.« It thus went without saying that Butenandt's price would be astronomical. Hence he would »entirely understand« if the faculty rethought its decision and instead chose a »more secure path by calling another [i. e., less expensive] colleague.«[185]

It should be noted here that Butenandt was not fully forthcoming to the Munich faculty (or any of his academic colleagues, perhaps including the administration of the MPG) about his situation in Tübingen. The »private Württemberg groups« he mentioned played an insignificant role in financing the modernization of his institute, in comparison with the 450,000 DM provided to the end of 1951 by the informal Triple Agreement of Bayer, Schering, and Hoffmann-La Roche, as previously discussed. Butenandt evidently preferred not to publicize this arrangement, which continued albeit on a substantially reduced scale (50,000 DM per year) from 1952 on, so he was evidently still using the faculty fund as a cover story for his industrial subsidies. Indeed, it was to Butenandt's advantage that the Munich authorities did not know about his funding under the Triple Agreement, which would continue on the reduced level even after he moved to Munich.

Butenandt's negotiating strategy included another element that took account of a growing willingness in German academic circles to ignore the Nazi pasts of its members: he now arrogantly refused to supply any political documentation. He pointed out that when the Natural Science Faculty in Munich had corresponded with him as a potential successor to Heinrich Wieland as professor of chemistry in 1949, he had adamantly refused to consider filling out a political questionnaire in compliance with the Law on Liberation from National Socialism and Militarism

185 Butenandt to Laves, 29 March 1952, UAM, N-IX Butenandt, Bd. 1.

of 5 March 1946, because he regarded this as »four years after the collapse, no longer consistent with the dignity of free people.« Butenandt saw even less reason to complete such a questionnaire three years later, considering that he had never been dismissed from his post, and moreover had had the honor of representing German science abroad as early as 1946. »Knowing that the faculty did not create this requirement, I even think that they would be pleased to have all the colleagues who might be considered for a call unanimously reject such a questionnaire« – the »historical examples« of which he was returning to Laves.[186] In this way Butenandt could avoid the choice between lying about his past and honestly admitting that he had been a Nazi Party member in good standing. But this was a somewhat risky move, calculated on the assumption that the Munich faculty would welcome his efforts to evade the law. This nevertheless turned out to be a correct assumption, perhaps related to the high proportion of former Nazis already in the faculty.[187] In any case, negotiations proceeded without further reference to the law of 1946, effectively burying the Nazi past as in so many other cases.

The resulting, complicated process was drawn out over about five years: first, negotiations in 1952–53 between Butenandt, the Bavarian authorities, and the MPG over the university professorship for Butenandt, culminating in an agreement whereby the Bavarian government would build two institutes to Butenandt's specifications and transfer the bulk of his university institute staff and MPI research group to Munich. This phase concluded in November 1953 with Butenandt's acceptance of the Bavarian education ministry's offer after he received authorization from the MPG to move his institute. In order to soften the blow to his Tübingen colleagues, the MPG, and the Baden-Württtemberg authorities, Butenandt agreed to have his section for virus research remain in its new building as an independent MPI furnished with the equipment made possible by the Triple Agreement of 1948 (which Butenandt only hinted at in his press release of December 1953).[188] The second phase saw the construction of the institutes themselves (a university institute for physiological chemistry adjacent to a new MPI for Biochemistry), which were integral parts of the new medical faculty building complex, including a new institute for physiology and a lecture hall and classroom structure between the two new university institutes – see Fig. 14), and finally Butenandt's move to Munich, effective 1 October 1956. Equipping the institutes continued beyond this point, with the celebratory opening held in June 1957.[189] The Bavarian government had initially estimated the total price at 7.6 million DM, including 4.7 million DM just for Butenandt's institutes. Although the necessary funds

186 Butenandt to Laves, 29 March 1952, UAM, N-IX Butenandt, Bd. 1.

187 Counting chemists and biochemists alone, in 1950 eight of fourteen professors of all ranks in the university had been party members; cf. Deichmann, *Flüchten*, 2001, 441.

188 Adolf Butenandt: Presse-Notiz auf Anforderung des Rektors der Stuttgarter Zeitung gegeben am 21. 12. 1955, AMPG, III. Abt., Rep. 84/1, Nr. 689, fol. 189–190. See also prior correspondence in this volume including Butenandt to Otto Hahn, 31 Oct. 1953 and Hahn to Butenandt, 3 Nov. 1953, ibid., fol. 91–92 and fol. 94–95.

189 Heinz Dannenberg: Max-Planck-Institut für Biochemie in München. In: Generalverwaltung der Max-Planck-Gesellschaft (ed.): *Jahrbuch 1961 der Max-Planck-Gesellschaft zur Förderung der Wissenschaften*. Vol. 2. Göttingen 1962, 90–110, 91, 102. After Butenandt assumed the presidency of the MPG and gave up his professorship in April 1961 ending the »personal union« between the two institutes, Dannenberg became deputy director of the MPI.

were not available in the state budget to build the two new institute structures, the Bavarian education ministry was able to persuade the five regional social insurance agencies in Bavaria (Landesversicherungsanstalten or LVAs) to provide the necessary capital on the order of at least 3 million DM, in the apparent belief that Butenandt's work was likely to lead to a cure for cancer.[190] The costs continued to increase during the building process as Butenandt continued to negotiate. The government's final cost estimate in 1957 for the entire institute complex was about 8.5 million DM after cost savings of 500,000 DM. Of this, the cost of the MPI and its adjacent structures for a greenhouse, solution storage, and electric power made up nearly half the total, around 4 million DM before cost savings.[191] The MPG's fiftieth anniversary yearbook in 1961 reported the »total cost« for the MPI for Biochemistry as 3.3 million DM, which was, however, apparently an effort to downplay the expense involved.[192]

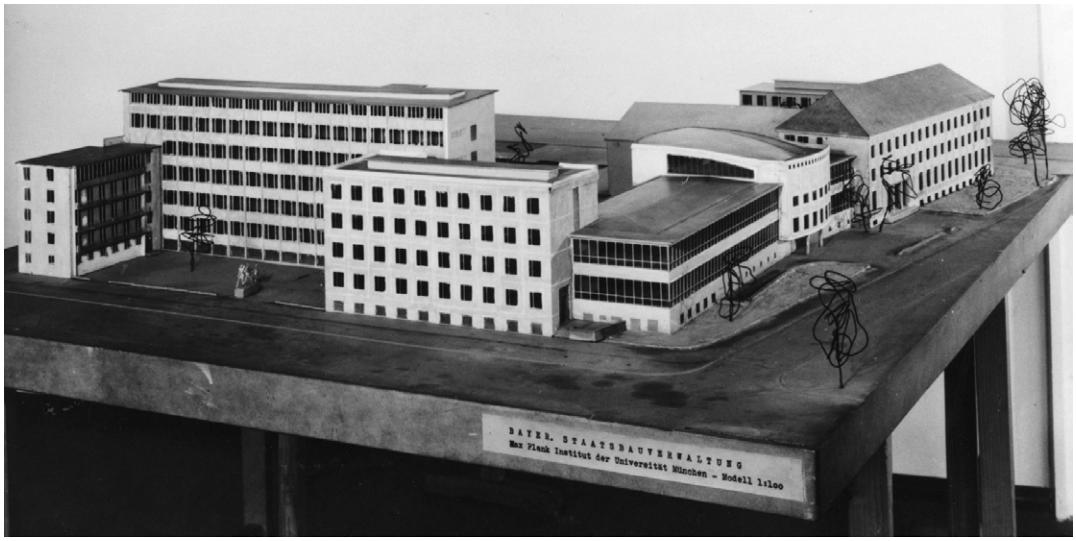

Fig. 14. A construction model, probably from 1953, of the new medical faculty institute complex in the LMU, featuring the institutes designed to Butenandt's specifications and opened in 1956.[193] The MPI for Biochemistry is the seven-story building on the left, facing the Goethestrasse. The two-story building on the corner facing Pettenkoferstrasse (on the right side) is Butenandt's university institute for physiological chemistry. To its right is the lecture hall building shared by the institutes for physiological chemistry and physiology. (facing Pettenkofer Strasse on the other side of the lecture hall). Not visible is Grassmann's MPI, which was built across the inner courtyard on the other side from Butenandt's MPI.
Source: Archives of the Max Planck Society.

190 Memorandum, 10 Dec. 1952, from [signature illegible, possibly Meinzolt] to the (Education?) Staatsminister, the State Secretary, and Referat 1, regarding the meeting in the Landesversicherungsamt, 10 Dec. 1952, BayHStA, MK 69393.: Vormerkung, by [same illegible signature; Meinzolt?] to Staatsminister, State Secretary, Referent 1 and Referent 27, 27 Nov. 1952, ibid.

191 Details on construction and costs in AMPG, III. Abt., Rep. 84/1, Nr. 690 and Nr. 691. For the final estimates see Müller: Aufgliederung der Baukosten nach dem grossen Kostenanschlag vom 15.11.54 (6 May 1957) and Müller: Zusammenfassung der Baukosten für den Neubau des Max-Planck-Instituts für Biochemie, 8 May 1957, ibid., Nr. 691.

192 Dannenberg, Max-Planck-Institut für Biochemie, 1962, 90–110, 102.

193 On the dating of the model to no later than June 1953, see Aktennotiz (copy, s. Ref.1: von Elmenau, 22 June 1953), in BayHStA, MK 69393.

3.2 Adding Wolfgang Grassmann's MPI for Protein and Leather Research to the Munich complex

Butenandt's negotiations with the Bavarian authorities were further complicated because he also wanted to coordinate his move to Munich with that of Wolfgang Grassmann, the director of the MPI for Protein and Leather Research in Regensburg. After the resignation of Emil Fischer's former assistant Max Bergmann, who went to the Rockefeller Institute in New York, Grassmann had directed the Kaiser Wilhelm Institute for Leather Research in Dresden until the firestorm caused by Allied bombing destroyed it with most of the city in February 1945. In the immediate postwar period, he had unsuccessfully sought to reestablish his institute in the West-German cities of Mühleim/Ruhr or Reutlingen near Tübingen. With support from the West-German leather industry, in 1948 he established a new research center in Regensburg under the name Research Institute for Protein and Leather (structural research on peptides and proteins had been a part of the institute's work since its founding under Bergmann, who with his associate Leonidas Zervas in 1932 had achieved the first significant improvement on Fischer's methods for peptide synthesis, work that continued at the Rockefeller Institute).[194] Grassmann's center served simultaneously as an institute within the so-called Emergency University (Notuniversität) of Regensburg, the recently expanded College of Philosophy and Theology (Philosophisch-Theologische Hochschule), where Grassmann had lectured since 1947 on biochemistry. In 1949 Grassmann's institute joined the MPG as the Research Center for Protein and Leather (Forschungsstelle für Einweiss und Leder). At that time the academic leaders of the Regensburg Hochschule were pushing for it to be the fourth Bavarian state university, but after the Bavarian legislature decisively rejected this proposal in October 1952, Grassmann saw little hope of getting additional support to expand his facilities in Regensburg. He now expected his institute to move either to Reutlingen or Tübingen, where his institute could be close to the other MPIs in Tübingen (at this point Grassmann did not mention and perhaps was not yet aware of the possibility that Butenandt might go to Munich). Grassmann thus notified Hahn as well as Butenandt, who had previously endorsed MPG support for Grassmann's institute, that only significant concessions from the Bavarian government would keep his institute in Bavaria.[195] Butenandt apparently then told Grassmann about his prospective call to Munich, which was still uncertain, so that when Grassmann went to Munich to discuss the situation with the Bavarian Education Minister, Dr. Josef Schwalber (1902–1969), he raised the possibility of moving his own institute to Munich as way of countering the leather industry's inclination to move it out of Bavaria. Grassmann now hoped to cooperate with Butenandt and suggested that he bring up Grassmann's institute during his own negotiations with the minister, creating the attractive possibility of two new MPIs working together in Munich. Schwalber had expressed his surprise to Grassmann that Butenandt had not yet received a formal call, and Grassmann told Butenandt to expect a letter to this effect in the near future, with an offer to meet with the

194 Fruton, *Proteins*, 1999, 188–189.

195 Grassmann to Butenandt, 16 Oct. 1952, AMPG, III. Abt., Rep. 84/2, Nr. 2072, fol.15.

minister for further negotiations.[196] Butenandt did receive the minister's letter in early November 1952, but apparently only after Schwalber had received word from the dean of the medical faculty that the LVA for Munich had confirmed its offer of at least 1 million DM for the construction of a new institute, on the condition that Butenandt accepted the appointment.[197]

Grassmann's change in tactics evidently came as a surprise and a potential embarrassment to Otto Hahn, who assured the university authorities in Tübingen in June 1953 that there were no plans to concentrate the MPIs in Munich.[198] Aside from the fact that both the Bavarian government and the MPG wanted Grassmann to remain in Regensburg, Hahn was only willing to support these moves if they entailed no additional costs for the MPG.[199] Financial issues thus became critical, as Butenandt and Grassmann successfully played off against each other the MPG, the Bavarian and Baden-Württemberg governments, and interested business groups including the regional social insurance agencies in Bavaria. Another key player was Max Grasmann, president of the Landeszentralbank of Bavaria and an influential member of the ruling CSU party. Butenandt had argued that Grassmann's and his institutes could complement each other's research, offering far greater prospects than a single, isolated MPI in Munich. Following Butenandt's subsequent request in November 1953 for him to act as a »catalyst«, Max Grasmann began mobilizing his industry colleagues to raise funds to facilitate the transfer of Grassmann's research center to Munich.[200] This may have been the decisive initiative that led the Bavarian authorities to invite Grassmann to move to Munich. For Bavaria, Butenandt was clearly the higher priority, but also much more expensive – too expensive, in the end, for Baden-Württemberg to compete. Grassmann's institute formally became an MPI for Protein and Leather Research (Eiweiss und Lederforschung) in 1954, already in the expectation of moving to Munich. He was subsequently able to negotiate to have his new MPI for Protein and Leather Research to be built near Butenandt's institutes in Munich, in order to foster closer scientific interactions among the staff.[201]

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196 Grassmann to Butenandt, 29 Oct. 1952, AMPG, III. Abt., Rep. 84/1, Nr. 688, fol. 99.

197 Butenandt to Otto Hahn, 3 Nov. 1952, AMPG, III. Abt., Rep. 84/1, Nr. 689, fol. 87; K. Bingold (Dekan, Med. Fak., Munich) to Butenandt, 11 Nov. 1952, AMPG, III. Abt., Rep. 84/1, Nr. 688, fol. 26. According to Bingold, the LVA's offer originated with a lucky accident: he had lamented the lack of funds to meet Butenandt's demands with one of his patients, who happened to be a director of the Munich LVA. To Bingold's surprise, this gentleman immediately offered to subsidize the construction.

198 H. Weber: Bericht über die Verhandlungen mit dem Präsidenten der Max-Planck-Gesellschaft, Prof. Hahn, ihrem Geschäftsführer Herrn Beneke und Herrn Telschow am Sonntag, den 7. 6. 1953, nachmittags von 15–17 Uhr im Hotel Gebhardt in Göttingen, UAT, 126a/59, fol. 46.

199 Cf. Hahn to Butenandt, 19 June 1953, AMPG, III. Abt., Rep. 84/1, Nr. 688, fol. 114; Benecke to Butenandt, 3 July 1953, AMPG, III. Abt., Rep. 84/1, Nr. 689, fol. 90.

200 Butenandt to Grasmann, 29 Sept. 1953, AMPG, III. Abt., Rep. 84/1, Nr. 688, fol. 143–145; Grasmann to Butenandt, 15 Feb. 1953 [sic=1954], ibid., fol. 86–87. The relevant Bavarian Ministry of Education correspondence is in BayHStA, MK 71239.

201 See correspondence regarding construction of Grassmann's and Butenandt's institutes in BayHStA, MK 71243 and MK 69419, MK 69420 respectively.

3.3 A third biochemical MPI? Feodor Lynen and the MPI for Cell Chemistry

Somewhat more distant was Feodor Lynen's MPI for Cell Chemistry (Zellchemie). Lynen, son-in-law of the LMU's OP (ordinarius professor) for chemistry and Nobel laureate Heinrich Wieland, had started his career within Wieland's institute as an organic chemist studying biologically active substances. He gained appointment as an AOP (extraordinarius or associate professor) for biochemistry in the Natural Science Faculty of the LMU created after the war at Wieland's recommendation. Lynen had found his international reputation growing, as he was among the few Germans doing first-rate work in »dynamic« biochemistry, examining the chemistry of metabolism.[202] But one of his first significant results, the clarification of the structure of a newly isolated »Coenzyme A« in 1951, was based on the classical organic chemistry he had learned from Wieland; once found, the solution seemed so simple that Lynen was only reassured of its significance when Otto Meyerhof and Carl Neuberg reported from the U. S. that it had struck American biochemists »like a bomb.«[203] The critical significance of Lynen's work on Coenzyme A can be seen in the 1964 Nobel lecture of his co-laureate Konrad E. Bloch: »Lynen's fundamental discovery of acetyl-CoA in 1951 paved the way for studying the early steps in sterol biosynthesis as it did for the understanding of all phases of fatty acid metabolism.«[204] This was of course the work that won Bloch and Lynen the Nobel Prize and made Lynen the emerging »star of modern biochemistry«, more significant even than Butenandt to some in the younger generation such as Gerhard Pfleiderer.[205]

The reputation Lynen thus gained in the early 1950s enabled him to visit American laboratories in the spring of 1953 with a fellowship from the Rockefeller Foundation. At the same time, the University of Bern invited him to apply for a professorship. Like previous calls to Butenandt and Kuhn, this raised consternation in German academic circles, because as the dean of the Munich natural science faculty pointed out, there were then only four AOPs and no OPs for »biochemistry« in the science faculties of German universities (chairs for »physiological chemistry« tended to be in the medical faculties, to which Richard Kuhn had shifted in 1950, as discussed earlier).[206] To keep Lynen in Munich, Wieland helped to negotiate an improvement in his working conditions and support, including a higher salary and budget, additional assistants, and a promise of a new building in a few years.[207] Laboratory space was the most problematic aspect,

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202 See Heike Will: *»Sei naiv und mach' ein Experiment«. Feodor Lynen. Biographie des Münchner Biochemikers und Nobelpreisträgers.* Weinheim: Wiley-VCH 2011, 87–146 for the developments discussed in this paragraph; for his role as a biochemist see Konrad Bloch: Feodor Lynen – Architekt der klassischen Biochemie. *Feodor Lynen. Gedenkfeier*, Max-Planck-Gesellschaft. Berichte und Mitteilungen 2, 1980, 21–30.

203 Will, *Feodor Lynen*, 2011, 74.

204 Konrad Bloch: The Biological Synthesis of Cholesterol. Nobel Lecture, December 11, 1964. *Nobel Lectures, Physiology or Medicine 1963–1970.* Amsterdam: Elsevier 1971, 78–100, 86–87.

205 Cited in Deichmann, *Flüchten*, 2001, 145.

206 G. Menzer, Dekan der Naturwiss. Fak., to Rektorat der Univ. München, 9 June 1953, BayHStA, MK 69786.

207 Aktennotiz, 29 May 1953, in PA Lynen, BayHStA, MK 54904.

because the university chemistry laboratory building, like many others, was still being rebuilt after its wartime destruction. Lynen's laboratory had been located since 1948 in the attic of the Institute for Zoology, furnished with the help of a loan from the Boehringer Ingelheim pharmaceutical firm. Even so, the laboratory remained sparsely furnished, and experiments requiring certain types of instruments had to be done at other institutes.[208] Hence Lynen had welcomed the opportunity given by his Rockefeller fellowship to work in well-equipped American laboratories. Indeed, he made so strong an impression on the Americans that shortly after his return to Munich in 1953, he received a call to Harvard University.

Lynen's call to Harvard got the attention of the MPG, as once again a German biochemist was threatening to leave for the United States. Otto Hahn proposed that the MPG offer him a directorship and scientific membership if Lynen declined the call, an idea that Butenandt (as a member of the MPG's appointments commission) initially rejected. Although Lynen did in fact decline the Harvard call (and thus repeated what Butenandt had done in 1935), he was appointed to a personal OP for biochemistry but could not expect the Bavarian government to construct an independent institute for him in the near future. Thus, after further discussions, the MPG appointed Lynen in 1954 as director of a small Institute for Cell Chemistry within the German Research Center for Psychiatry (Deutsche Forschungsanstalt für Psychiatrie, MPI).[209] This was not an especially elegant solution, because Lynen's specialty did not connect especially closely to psychiatric research. Following calls to the ETH in Zürich and the University of Frankfurt in 1955, Lynen gained additional concessions: the Bavarian ministry of education elevated his position to a formal OP for Biochemistry within the Natural Science Faculty.[210] Lynen's institute also became an independent MPI in 1956 and in 1958 finally moved into a separate building housing both his MPI and his university institute for biochemistry, a pattern analogous to Butenandt's dual institutes completed in 1956.[211] There were now three biochemical MPIs in Munich, but Lynen's institute was significantly smaller than those of Butenandt and Grassmann.

208 Will, *Feodor Lynen*, 2011, 70–71.

209 Aktennotiz, 26 Nov. 1953, in PA Lynen, BayHStA, MK 54904.

210 Urkunde, 17 Sept. 1956, in PA Lynen, BayHStA, MK 54904.

211 Details in Will, *Feodor Lynen*, 2011, 87–146.

4 Martinsried as Another New Dahlem? The MPI for Biochemistry as a Model for Large-Scale Research Institutes in the Max Planck Society, 1965–1990

4.1 Why the MPG decided to create a new biochemical center in Martinsried

Although Butenandt's new institute complex may have seemed almost ideal in 1956, his situation and the future of his MPI (and of the MPG itself) changed dramatically when the Senate elected him to the presidency of the MPG as Otto Hahn's successor in November 1959. Here his professional trajectory had again intersected with that of Richard Kuhn, who came forward as a competing but somewhat reluctant candidate, largely at the behest of two influential individuals in the MPG, Otto Warburg and Georg Schreiber, who for various reasons did not want Butenandt to be president.[212] Had Kuhn been elected, he would probably have left the administration of the MPG in Göttingen; he had been accustomed to act as a remote president for the German Chemical Society during the period 1938–1945, allowing the routine business to be done at the society's headquarters in Berlin. But Butenandt wanted to take a more active role, and he thus set up a small presidential office in his Munich institute. Although for the time being the rest of the MPG's administration remained in Göttingen, he had begun the process that would ultimately transfer it to Munich.[213]

The early years of Butenandt's presidency also saw several developments that completely disrupted the institutional arrangements he had so painstakingly set up in Munich. First, Butenandt continued as nominal director of his MPI but found it impossible to retain his professorship in the medical faculty, which nullified the close connection he had designed between the MPI and the university institute for physiological chemistry. At the same time, the rising popularity of the medical sciences in the 1960s coincided with a rapid increase in the overall student population, which put pressure on the university to reclaim space occupied by MPIs for teaching institutes; in 1963, in conjunction with agreements made at the appointment of Butenandt's successor Professor Theodor Bücher, the university proposed a new building for physiology and physiological chemistry in the inner court east of Butenandt's institute (Fig. 15).[214] This clashed directly with the needs of Wolfgang Grassmann, who wanted to enlarge his institute into the same area. The space problem was further complicated when it became clear that a larger MPI for Lynen could not be accommodated in the university institute for chemistry. Lynen's institute was too isolated and too small where it was, but the only place where it could be near the other two MPIs would have been in the already contested space between Butenandt and Grassmann. For Butenandt, this was part of a larger problem. Munich had become a prime location for the MPG and would house ten MPIs by the end of the 1960s, but »one had recognized too late« that the MPG had failed »to acquire a large, connected area of land on which the

212 Cf. Balcar, *Wandel*, 2020, 38–39. On Warburg and Schreiber urging Kuhn to seek the presidency: Hans-Jürg Kuhn, personal communication to the author, 19 Feb. 2020.

213 Balcar, *Wandel*, 2020, 48–51.

214 Correspondence on the building of the institute for physiological chemistry, 1960–1967, BayHStA, MK 69421.

majority of institutes in the Munich area could be established in close proximity according to the well-known Dahlem model.«[215]

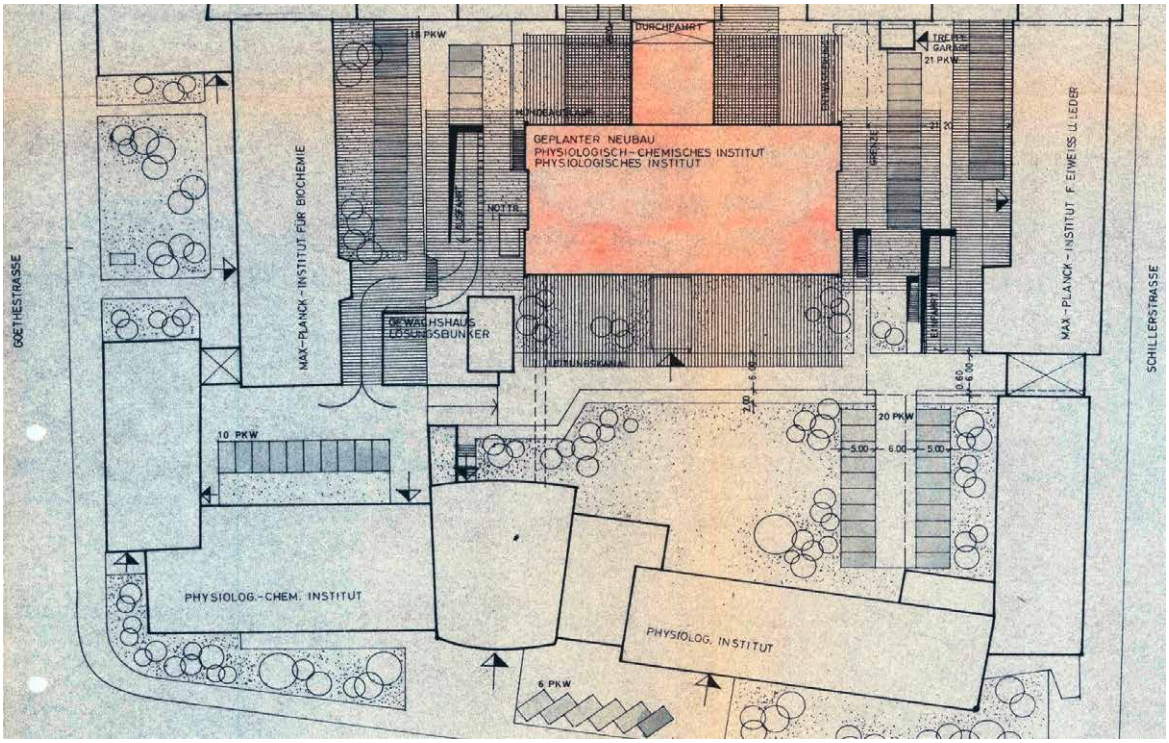

Fig. 15. Proposed addition (in red) to the LMU institutes for physiology and physiological chemistry (1963). Source: Universitätsbauamt München: Neubau physiolog.-chem. Institut und physiolog. Institut der Universität München. Lageplan (Abstandsflächen). Pl. Nr. 1.02a (Juni 1963). In Archives of the Max Planck Society, IV. Abt., Rep. 2 (Karten und Pläne der MPG), Nr. 968 (detail).

Thus in January 1962 Butenandt, Grassmann, and the dean of the medical faculty had begun to discuss the possibility that in the next five to seven years, both MPIs would move to another location, thus opening space in their former buildings for use by the medical faculty. The MPG began to scout the surrounding suburban areas for appropriate spaces to fit their biochemical institutes. Land near Garching in the north was already set aside for physics, while other suggested locations were not sufficiently close to a large clinic, which would be useful for collaboration in biomedical research. Butenandt asked the Bavarian government to be permitted to acquire the necessary land in the vicinity of the planned Klinikum Großhadern, which was also expected to attract some of the university's medical institutes and thus would lend itself to expanded collaboration.[216] This prospect led directly to the choice of Martinsried as the location for the biochemical institutes, a location that, by coincidence, was located about as far from

215 Adolf Butenandt: Geschichte und Konzeption des Instituts. In: Max-Planck-Gesellschaft (ed.): *Max-Planck-Institut für Biochemie*. Max-Planck-Gesellschaft. Berichte und Mitteilungen 2, 1977, 11–21.

216 Ballreich (MPG) to Bavarian Finance Minister, 6 Aug. 1962; Butenandt to Bavarian Finance Minister, 6 Aug. 1962, AMPG, II. Abt., Rep. 66, Nr. 621, fol. 78–81.

the center of Munich as Dahlem was from the center of Berlin. The intent was in any case clear: not only to accommodate the three institutes, but also to offer room for later projects of the MPG. Making up for the earlier oversight in planning, enough land should be obtained »to make possible the development of a ›little Dahlem‹ in the more distant future.«[217]

The need to move to a larger site became acute in 1964, after Lynen won the Nobel Prize, which would normally prompt the MPG to offer him a significant improvement in his institutional arrangements. Moreover, his prestige as a Nobel laureate would make him the natural leader of a much larger institute as well as heightening the advantages of bringing his institute into closer proximity to the biochemical research groups in the other MPIs, which could only be accomplished by moving them all outside of Munich. This raised a host of fundamental questions about institute structure and scale, as well as the most efficient way to organize modern scientific research, that lay at the heart of Butenandt's and his MPG administrative colleagues' thinking and planning during the mid-1960s. With only some oversimplification, these might be reformulated into a single question for the Martinsried project: could one recreate the spirit of Dahlem within a framework of »big science«?

4.2 »Big science« and the Dahlem ideal under Butenandt's presidency in the 1960s

As president of the MPG, Otto Hahn had carried over the KWG's tradition of small-scale institutes, but even before he retired in 1960, the MPG was preparing to establish its first truly large-scale scientific enterprise, which became the Institute for Plasma Physics GmbH (IPP) in Garching north of Munich. The »GmbH« signifies that this was literally a scientific enterprise, a new type of institutional activity for the MPG and one that would not yet be called a »Max Planck Institute«, in part because it was not solely the responsibility of the MPG but jointly supported through a contract with EURATOM. Werner Heisenberg, who had taken the initiative in this project, acted as contractor. Without discussing this in detail, one may note that it set a precedent for the MPG, putting the question of larger institutes permanently on the agenda for the incoming president Butenandt and his administrative colleagues. In principle, it was now agreed, no area of scientific activity should be foreclosed to the MPG due to reasons of scale or cost. Thus, in the annual report for 1959–1960, one reads that the MPG »in future will also accept responsibility for research facilities and research projects that exceed the scale of the conventional institutes of the Society.«[218]

The MPG had decided to found the IPP only as the latest in a series of similar research enterprises that began to develop around nuclear reactors and other large nuclear devices in the Federal Republic and West Berlin during the late 1950s, continuing into the 1960s. Initially designed as

217 Butenandt, Geschichte, 1977, 18.

218 Generalsverwaltung der Max-Planck-Gesellschaft (ed.): Jahresbericht. *Jahrbuch 1960 der Max-Planck-Gesellschaft zur Förderung der Wissenschaften.* Göttingen 1960, 15–26, 25.

cooperative ventures involving the federal ministry for atomic energy and *Land* or local governments, industry (whose participation tended to be more limited as the basic research aspects predominated), and academic institutions. These included the Karlsruhe Nuclear Reactor, the Jülich Nuclear Research Facility, the Society for Nuclear Energy Development in Shipping (Gesellschaft für Kernenergieverwertung in Schiffbau und Schiffahrt or GKSS), the Hahn-Meitner-Institute for Nuclear Research in Berlin-Wannseee, and the German Electron Synchrotron (Deutsche Elektronen-Synchrotron or DESY) particle accelerator research center in Hamburg.[219] This was an effort to emulate the »big physics« institutions already established in the USA, involving large pieces of apparatus such as nuclear reactors and accelerators served by huge technical staffs. The Americans had had great success with such enterprises since the wartime Manhattan Project, perhaps the most famous example of successful large-scale scientific mobilization.[220]

By 1963, as he was wrestling with the dilemma of what to do with the biochemical institutes in Munich, Butenandt publicly reflected on the questions raised by these new types of larger institutes, for which he did not yet have a general name. In particular, might their potential efficiencies of scale offer a solution to the problem of restoring the productivity and quality of science in Germany, which even two decades after the war was still not up to international standards in many fields? This was not simply a question of money, though more funds would certainly help. It was also an organizational issue; here he cited a comparative analysis by the American physicist Frederick Seitz, who asserted that after the war, senior German scientists had continued to embrace the traditional, hierarchical institute system. Frustrated by this system, younger scientists were finding greater opportunities abroad, especially in the United States.[221] Butenandt argued in contrast that there was reason for optimism, and that some younger German scientists were returning.[222] Yet he avoided mentioning the idea of an American-style, collegially managed department with a large number of equal members as a way to attract younger colleagues, even though physics professors in the Technical University of Munich had recently argued in favor of that idea.[223] Instead, Butenandt continued to favor the MPG's ideal, which had continued to produce top-quality work in key areas, including biochemistry: a well-organized, Dahlem-style institute, run by a wise director who could unify his research groups around com-

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219 Margit Szöllösi-Janze and Helmuth Trischler (eds.): *Großforschung in Deutschland.* Frankfurt am Main: Campus Verlag 1990.

220 The official history is Francis George Gosling: *The Manhattan Project: Making the Atomic Bomb.* Washingon, DC: United States Department of Energy, History Division 1994; there is of course an enormous literature beyond this.

221 Adolf Butenandt: Ansprache des Präsidenten Professor Dr. Butenandt in der Festversammlung der Max-Planck-Gesellschaft in Augsburg am 16. Mai 1963. In: Generalverwaltung der Max-Planck-Gesellschaft (ed.): *Jahrbuch der Max-Planck-Gesellschaft 1963.* Göttingen 1963, 18–30, 22.

222 Butenandt, Ansprache Präsident Butenandt 1963, 1963, 18–30, 25.

223 Paul Kienle: The Second Mössbauer Effect. In: Georg Michael Kalvius and Paul Kienle (eds.): *The Rudolf Mössbauer Story. His Scientific Work and Its Impact on Science and History.* Berlin: Springer 2012, 417–423, 417–418, referring not to Mössbauer's demands in 1964, but rather to the March 1962 memorandum of Heinz Maier-Leibnitz and three colleagues, which they had submitted both to the Bavarian ministry of education and to the German Council of Science (Wissenschaftsrat); Nachmansohn, *German-Jewish Pioneers*, 1979.

mon goals. Thus, Germans did not need to import American phrases such as »teamwork«, which others had used to criticize the hierarchical institute system. Change might be in the air, but with what implications? Admitting that »[m]any research institutes today are turning into research factories«, he suggested that industry might furnish models for the rational control of scientific research.[224] In that case, however, he was still thinking in hierarchical terms.

In considering Butenandt's praise for the virtues of Dahlem-style institutes, one might associate his thinking with that of the biochemists Erwin Chargaff, David Nachmansohn, and others who had experienced the open, free, and creative atmosphere that flourished in Dahlem's smaller institutes during the fading years of the Weimar Republic.[225] But Butenandt had not experienced that Dahlem. Instead, his Dahlem had flourished a decade later, as scientists self-mobilized under the racial-nationalist slogans of National Socialism, whose »ideal«, it has been argued, was the »highly structured organization of ›big science‹ or ›big engineering‹ projects« in national military service.[226] While these had failed to win the war, one could attribute their failures to the defects of bureaucratic, inefficient »mammoth« institutions, directed by individuals who were not fully capable of mastering their organizational challenges. But if one could find the right organization, one might also recapture the positive spirit of a large scientific community cooperating for a common goal that had characterized the Dahlem that Butenandt remembered, while avoiding the evils and inefficiencies that had characterized National Socialism. It is thus arguable that at the outset of the planning for Martinsried, Butenandt's perspective on »big science« and big-institute policy reflected at least in part his experiences of wartime mobilization, in both its positive and negative aspects, offering lessons that might be applied to the improvement of German scientific institutions in the 1960s. It would seem that he was still far from embracing the American idea of a department as an organizational approach.

It was precisely at this time that the Federal Republic began to address the issues connected with large-scale institutes. In 1962 the federal ministry originally established to deal with problems of atomic energy broadened its scope to include space flight and potentially other national scientific priorities such as computing technology, consequently becoming the Federal Ministry for Scientific Research (Bundesministerium für wissenschaftliche Forschung). On the question of how the government should approach the IPP and other large, expensive scientific enterprises requiring significant federal support, the ministry's State Secretary Wolfgang Cartellieri pro-

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224 Butenandt, Ansprache Präsident Butenandt 1963, 1963, 18–30, 28–29.

225 Erwin Chargaff: Amphisbaena. *Essays on Nucleic Acids.* Amsterdam: Elsevier 1963, 174–199; cited in Pnina G. Abir-Am: The Politics of Macromolecules. Molecular Biologists, Biochemists, and Rhetoric. *Osiris* 7/1 (1992), 164–191, 189–190. doi:10.1086/368709.

226 Walter E. Grunden et al.: Laying the Foundation for Wartime Research: A Comparative Overview of Science Mobilization in National Socialist Germany, Japan, and the Soviet Union. Edited by Carola Sachse and Mark Walker. *Osiris* 20/2: Politics and Science in Wartime. Comparative international Perspectives on the Kaiser Wilhelm Institute (2005), 79–106, 82. See also Ryan Dahn: Big Science, Nazified? Pascual Jordan, Adolf Meyer-Abich, and the Abortive Scientific Journal Physis. *Isis* 110/1 (2019), 68–90. doi:10.1086/701352.

duced an influential short paper that set forth a plan for coordinating the development of what he termed »*Großforschung*« as the closest counterpart to the American term »big science«.[227]

A year later, impressed by Cartellieri's arguments, Butenandt in his presidential address to the MPG annual meeting returned to the issues presented by »institutes of so-called *Großforschung*, of ›big science‹ in Anglo-Saxon terms.«[228] By 1964, however, »big science« enterprises had become the subject of contentious debate in the United States, as the physicist Paul Zilsel, in contrast to Butenandt's 1963 comments, likened »the huge ›think factories‹ of our time« to »the Lancashire cotton mills of the industrial revolution«, extraordinarily productive but also deadening as they transformed scientific knowledge into a »mere commodity« and research into »assembly line production«.[229] In this same year, the physicist Rudolf Mössbauer accepted an appointment to the Technical University of Munich, on the condition that it establish the departmental system previously proposed by his mentor Maier-Leibnitz and others; as a recent Nobel laureate returning from a lucrative position in the United States, he could exert greater leverage to force through this reform – the »second Mössbauer effect« as *Der Spiegel* dubbed it. Yet even with his prestige, the reform would require two more years of negotiation to bring about, and it did not last, dissolved by the Bavarian university law of 1974.[230] He and his colleagues faced the scorn of traditionalists who declared that »they were digging the grave of the authority of the German professor.«[231] Those traditionalists no doubt saw the 1968 student movement as confirmation of their fears.

Yet despite Butenandt's apparent skepticism toward a departmental organization for institutes (skepticism that may have been in part designed to mollify the traditionalists in the MPG), in December 1964 a specially called general meeting of the MPG approved a new constitution, which set forth the option of collegial institute management as an equally acceptable option besides the traditional assumption of a single institute director. That also meant that although the great majority of institutes might continue to be designed for and controlled by single directors in accordance with the Harnack Principle, certain circumstances, »in particular the desire for close collaboration among several scientists (Teamwork) and the development of big-science research institutions, could necessitate a collegial structure of institute management«.[232]

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227 Wolfgang Cartellieri: Die Großforschung und der Staat. In: Bundesminister für wissenschaftliche Forschung (ed.): *Die Projektwissenschaften*. München: Gersbach & Sohn 1963, 3–16, 3; for the context and impact of his arguments see Gerhard A. Ritter: *Großforschung und Staat in Deutschland. Ein historischer Überblick*. München: C. H. Beck 1992, 192, 93–98.

228 Adolf Butenandt: Ansprache des Präsidenten Professor Dr. Butenandt in der Festversammlung der Max-Planck-Gesellschaft in Hamburg am 11. Juni 1964. In: Generalverwaltung der Max-Planck-Gesellschaft zur Förderung der Wissenschaften (ed.): *Jahrbuch der Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1964*. Göttingen 1964, 23–38, 30–32. The phrase »big science« is in English in the original.

229 James H. Capshew and Karen A. Rader: Big Science: Price to the Present. *Osiris* 7 (1992), 2–25, 5.

230 Mössbauer – Zweiter Effekt. *Der Spiegel* 20 (5/13/1964), 58–59; Kienle, Second Mössbauer Effect, 2012, 417–423, 417–420.

231 Robert Gerwin: Der zweite Mössbauer-Effekt. *Die Zeit* 15 (1964). https://www.zeit.de/1964/15/der-zweite-moessbauer-effekt?utm_referrer=https%3A%2F%2Fwww.google.com%2F. Last accessed 5/2/2023.

232 Hans Dölle: *Erläuterungen zur Satzung der MPG vom 3. 12. 1964*. München: Max-Planck-Gesellschaft 1965, 78–79. Teamwork is in English in the original. On the problem of adapting the Harnack Principle to a modern departmental system,

1964 also saw a new agreement between the federal (Bund) and state (Land) governments in Germany that allowed the federal government to assume a regular role in supporting German scientific institutions in cooperation with the states. Ultimately the Bund would pay 90% of the cost of major projects. This opened the way to overcome the financial limitations that had hitherto confronted the MPG and limited its ability to develop large-scale institutions that might employ a thousand staff members and entail an investment of hundreds of millions of DM. Hence Butenandt could argue that these institutions need not be restricted to physics, but that they were already developing in fields of chemistry related to radioactivity, and »biology is also certain to see the development of big science«.[233] By implication, this also applied to biochemistry and other related fields, but it should be noted that the Federal Republic's term »Großforschungseinrichtung« or big science facility, and the corresponding pattern of support, applied only to facilities »built around large devices«,[234] which might apply to biophysics but not biochemistry as then constituted. Yet an American-style elite university and its departments constituted another type of big science, in the sense of a large but non-hierarchical organization composed of many autonomous, freely interacting scientists, each with their own research group but together constituting a highly productive and innovative scientific community. The research-intensive Rockefeller Institute for Medical Research in New York (which became the Rockefeller University in 1965, and which by then had already produced eight Nobel laureates with ten more to come by 1984)[235] or the California Institute of Technology (Caltech) in Pasadena, which had deeply impressed Mössbauer, both exemplified this type of community. Butenandt was perhaps now coming to see in the departmental system a potential model for a modern Dahlem, to be brought to life in a series of new large institutes or research complexes, integrating smaller institutes that might complement each other, as Butenandt and Grassmann had visualized the interaction of their institutes in Munich. Plans for several such institutes were already under discussion in the MPG. An MPI for Molecular Genetics was to be built in Dahlem. In Göttingen, there was a project inspired by Manfred Eigen to unite the MPIs for physical chemistry and for spectroscopy, moving them to a larger location in Nikolausberg outside the city center to create a »center for biophysical chemistry« (as with the IPP, the MPG was initially reluctant to call its new large-scale enterprises Max Planck Institutes). Initiated a year before the planning began for the Munich-Martinsried project, the MPG's experience with the smaller-scale development in Göttingen-Nikolausberg promised useful lessons for the planners in Munich. Certainly the newly appointed (in 1963) head of the MPG's building department,

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see also Helmuth Trischler: Nationales Innovationssystem und regionale Innovationspolitik. Forschung in Bayern im westdeutschen Vergleich 1945 bis 1980. In: Thomas Schlemmer and Hans Woller (eds.): *Politik und Kultur im föderativen Staat 1949 bis 1973*. München: Oldenbourg 2004, 117–194, 191–192.

233 Butenandt, Ansprache Präsident Butenandt 1964, 1964, 23–38, 31.

234 Szöllösi-Janze and Trischler, *Großforschung*, 1990, 20.

235 The Rockefeller University: Nobel Prize. https://www.rockefeller.edu/about/awards/nobel-prize/. Last accessed 1/17/2023. It is worth recalling that Adolf Harnack had cited the Rockefeller Institute in his famous 1909 memorandum to the Kaiser, leading to the establishment of the KWG's original Dahlem complex.

Otto Meitinger, found some benefit in comparing the projects.[236] Would there thus be a big-science institute in Martinsried, and if so, how would it be designed and organized? Could one re-create in Martinsried the innovative scientific atmosphere of the original Dahlem community? Answers to the first questions would gradually emerge over the next decade; they would then help to shape the final answer.

4.3 Preliminary Planning for Martinsried, 1964–1966. The prospect of a »New Dahlem« and the problem of institutional scale and structure

Thus it was in the shadow of discussions over big science and its proper organization, whether hierarchical or departmental, that the Munich biochemists began to consider the implications of creating a new center in Martinsried.[237] »Twenty-five years ago [in 1964] the wish was expressed to found a ›new Dahlem‹«.[238] Gerhard Braunitzer, who recalled this »wish« in 1989 in a look back at the history of the MPI for Biochemistry, did not mention whose wish it was, but at the time he was the leader of the working group for protein chemistry in Adolf Butenandt's MPI for Biochemistry, and given the circumstances, it could hardly have come from anyone but Butenandt. As Braunitzer recalled, his colleagues in the institute were at first thinking on a rather small scale, in keeping with the relatively enclosed urban setting in which they were working. But they were soon to realize that their »new Dahlem« in Martinsried would offer far greater opportunities for expansion, as well as the opportunity to develop a new institute structure.[239]

As Butenandt recalled the planning process a decade later, over the course of the year 1965 the MPG obtained a 37-hectare (370,000 square meters) site east of the village of Martinsried in 1965, purchased from local landowners with support from the Bavarian ministry of education, the government of Upper Bavaria, the city of Munich, and the municipality of Planegg, which

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236 Otto Meitinger: Die Neubauten der Max-Planck-Institute in Göttingen und München. *Baumeister* 70 (1973), 1251–1264; for a survey of his work for the MPG (1963–1977), see Otto Altendorfer: Max-Planck-Gesellschaft 1963–1977. Bauten für die Forschung. In: Victor Lopez Cotelo (ed.): *Otto Meitinger. Architekt – Denkmalpfleger – Hochschullehrer*. Tiefenfach: Druckerei Schmerbeck 1997, 33–51.

237 For the broader institutional context of the Martinsried project, see Trischler, Innovationssystem, 2004, 117–194. These issues will also be covered in Altschäfl, *Globale Konkurrenz*, forthcoming.

238 Gerhard Braunitzer: Max-Planck-Institut für Biochemie – vor 35, 30 und 25 Jahren, 3 May 1989, AMPG, III. Abt., Rep. 84/1, Nr. 686, fol. 12.

239 There is an interesting discussion of architectural planning for Martinsried and the presumed impact of the chosen design on scientific collaboration, including the influence of American-style departmental organization and the MPG's Dahlem model, in Martina Heßler: *Die kreative Stadt. Zur Neuerfindung eines Topos*. Bielefeld: transcript Verlag 2007, 170–180, 219–223. Heßler cites with skepticism the MPI's [actually Butenandt's – JAJ] claims that the new building promoted collaboration, arguing instead that the available evidence did not permit a definite conclusion (222). Elsewhere in her analysis, Heßler describes in considerable detail the initial skepticism toward the MPI of local community leaders in Planegg and Martinsried (177–185) and the sometimes negative reaction of the public to the modernist architecture of Martinsried, as well as post-1990 opposition to the destruction of protected forest land through the expansion of the site (213–218). I can therefore omit the latter issues here.

included Martinsried. By March 1965, access to the site appeared secure enough for the MPG Senate to make a preliminary decision on the project. During the discussion, the Senate addressed four options for the overall organization of the Martinsried site. The MPG could have simply built three separate buildings for each of the three institutes, with no change in their internal structure, or the MPG could have created a fourth institute with its own building for Walter Hoppe's already large section for structural research by x-ray crystallography (then in Grassmann's institute). A third, somewhat more innovative option would have been to dissolve the existing institutes and create a »biochemical center«, containing separate buildings for all of the sections directed by individual scientific members, resulting in a dozen or so smaller institutes. The first two options would have continued the classic Dahlem tradition, while the third also corresponded to the Harnack Principle by building individual institutes around individual researchers, although in this case the individual institutes would be linked to centralized supporting facilities that could improve the economic efficiency of the center. Finally, there was the option of creating the biochemical center in the form of a single large building or interconnected group of buildings, including all the sections of the previous institutes plus common supporting facilities, but designed in a way that would permit later expansion.[240]

The fourth option was to create a single, large biochemical research complex with a unified design that would integrate all of the previous institutes and sections. The new center would be designed ideally to foster both the autonomy of each individual section while also fostering collaboration among sections with related or overlapping areas of research by placing these in close proximity to each other. At the same time, as with the third option there would be centralized supporting facilities (central library, guest house, etc.) and equipment (computer mainframe, electron microscopes, etc.) While the Munich biochemists generally favored the fourth option, there was some concern within the MPG's Senate that this would turn into an undesirable »mammoth institute«.[241] In the crucial meeting in March 1965, however, MPG Vice President Richard Kuhn, who had never worked in Dahlem, enthusiastically supported an approach based on his own experience of the collegially-governed American department combining several autonomous laboratory units. This had of course been the original conception of Kuhn's KWI for Medical Research, as a union of several autonomous sub-institutes whose directors would each assume in rotation the responsibility for managing the central facilities. In Kuhn's view, this avoided the danger of a mammoth, centralized institute, although the physicist Erich Regener pointed out that such a structure could hardly be avoided in the big-science institutes built around large devices. Even here, however, one could combine a big institute with several smaller ones. For Butenandt, the central problem with mammoth, centrally-controlled institutes was their inflexibility over the long term, as they were dependent upon a single director who would need to be replaced every couple of decades, at which point there might be a complete reorientation of the institute. The new approach, for which the biological institutes in

240 Butenandt, Geschichte, 1977, 18–19.

241 Butenandt, Geschichte, 1977, 20.

Tübingen already provided a model, was to preserve the overall structure of the large biochemical center while replacing individual section leaders and scientific members as needed. This would allow for more gradual, incremental changes whereby the biochemical center could keep abreast of the latest scientific developments. Butenandt insisted that this approach did not change the fundamental »principle of the Society, only to establish a new section (*Abteilung*) when the right scientist is there for it«.[242] But his substituting »section« for »institute« might have been a surprise to Adolf Harnack.[243]

The implications of these different options for institute design become clearer in a brief essay by Wolfgang Osterwalder, an architect at the Technical University of Stuttgart, introducing a special issue of an architectural journal featuring the MPG's subsequent competitions for the design of new MPIs. Osterwalder defined three phases in the development of the institutional structure (or three systems of institutional management) of the KWG/MPG since its origins in Dahlem. The first phase had been that of »directorial management«, whereby institutes were built »around the leader« in a hierarchical structure. This corresponds of course to the so-called »Harnack Principle«, though Ostwalder did not use this term. In a second phase, relatively autonomous research groups developed as sections within individual institutes that were kept in place (despite the Harnack Principle) following the retirement of the initial director, whose successor would have a weaker control over the entire institute. Because in such cases »the contacts between research units are relatively weak«, this form could be considered »quasi-collegial management«. Through recent internal reorganizations of the MPIs, their organization was entering a third phase, »collegial management« or a »department«, as shown in the following Fig. 16.[244] Notably, in this form a traditional director is wholly absent, as leadership (the role of managing director) rotates among the various section heads.

242 Minute of the 50th senate meeting on 12. 3. 1965, AMPG, II. Abt., Rep. 60, Nr. 50.SP, fol. 275.

243 But perhaps not, considering that the idea of an institute based on several autonomous sections staffed by scientific members, rather than a centrally-directed institute »built around« a single scientist, went back to the earliest days of the KWG in Dahlem, in the KWI for Chemistry. Jeffrey Allan Johnson: Die Gründung und Entwicklung des Kaiser-Wilhelm-Instituts für Chemie 1905–1930. In: Horst Kant and Carsten Reinhardt (eds.): *100 Jahre Kaiser-Wilhelm-/Max-Planck-Institut für Chemie (Otto-Hahn-Institut): Facetten seiner Geschichte.* Berlin: Archiv der Max-Planck-Gesellschaft 2012, 21–52, 31–32. Johnson, *The Kaiser's Chemists*, 1990, 165.

244 Wolfgang Osterwalder: Einführung. *architektur wettbewerbe. Internationale Vierteljahresschrift* Heft 53: Max-Planck-Institute / Max-Planck-Institutes (1968), v–viii, v-vi (my translation). The English translation of Osterwalder's essay at the beginning of the issue uses the somewhat less felicitous terms »loose associate management« for »unechte kollegiale Leitung« and »organic associate management« for »echte kollegiale Leitung«. Wolfgang Osterwalder: Introduction. *architektur wettbewerbe. Internationale Vierteljahresschrift* Heft 53: Max-Planck-Institute / Max-Planck-Institutes (1968), 1–2.

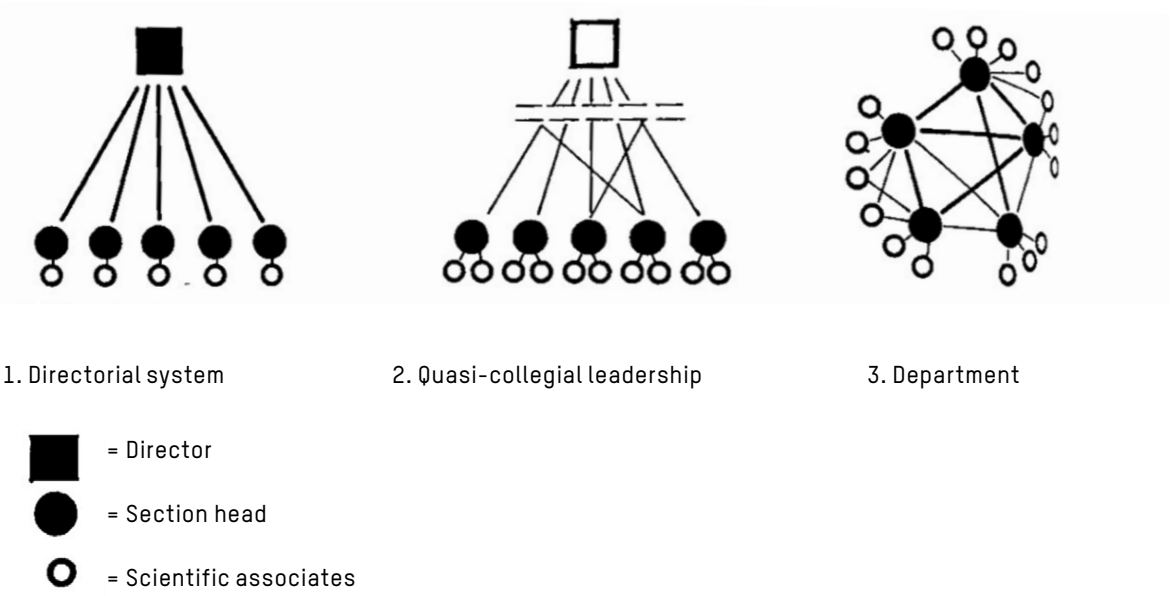

Fig. 16. Varieties of organizational structure in the KWI/MPIs by the late 1960s.
Source: Adapted from Wolfgang Osterwalder: Einführung. *architektur wettbewerbe. Internationale Vierteljahresschrift* 53. *Max-Planck-Institute / Max-Planck-Institutes* (Feb. 1968), v–viii, Fig. 4 on vi.

Osterwalder conceded that the original system »built around the director« was still suitable for smaller research institutes, in which a single director, normally someone with a strong personality doing Nobel-Prize-level research, could effectively manage all aspects of the institute and interact well with all of their associates. But in a »big science« world characterized by increasingly large-scale and expensive apparatus, this system seemed obsolescent. It created unacceptable limits on growth and might entail the inefficient use of equipment that would be more suitable to a larger institutional context. Moreover, by burdening a single director with managerial responsibilities not directly related to scientific research, it impinged on the time and energy needed to carry out an effective research program. The directorial system also limited possibilities for interdisciplinary collaboration and international scientific exchange. Finally, in an increasingly egalitarian contemporary world, it would be difficult to recruit promising younger scientists who might resent having to work within a traditional hierarchical order under an all-powerful director.[245]

As portrayed by Osterwalder and illustrated in Fig. 16 above, the quasi-collegial system lacked the advantages of the old directorial system, retained some of its disadvantages, and offered fewer advantages of its own as compared with the department. Its principal advantage was the opportunity for larger-scale research activity, along with a greater degree of scientific autonomy for the section heads. But having developed in an accidental, unplanned way, the system maintained a single director and elements of the old hierarchical structure, which might manifest

245 Osterwalder, Einführung, 1968, v–viii, vii.

themselves to a greater or lesser degree depending on the personalities of the director and section heads (the horizontal lines in the figure suggest weaker control by the director over the work of individual sections). The lack of a truly collegial organization further complicated the problems of efficient institutional and scientific management, creating almost unavoidable frictions between section heads in the allocation of resources. It also inhibited opportunities for collaboration between sections within the institute or across institutional lines (as indicated by the limited number of connecting lines between sections in the figure for this type of organization). A »mammoth institute« with a single director would have magnified these defects even more.

All the problems of the other systems could be reduced or eliminated within a departmental system. In particular, the Martinsried biochemical center exemplified the advantages to be gained from a truly collegial structure: efficiencies of scale, more effective use by all sections of centralized supporting facilities (library, mensa, guest house, etc.) and apparatus (electron microscopes, mass spectroscopy, etc.), and more equitable allocations of funds and resources to individual sections. Moreover, the more flexible departmental structure would make it possible to develop new forms of collaborative research outside and above the sectional organization. But above all, and here Osterwalder cited Rudolf Mössbauer, »the advantage of the departmental system for individual scientists consists … in the great reservoir of highly competent colleagues available for scientific discussions, and the fruitful and stimulating scientific atmosphere that results from these discussions.«[246] Having just returned from several years in the Physics Department of CalTech in Pasadena, California, Mössbauer was well-acquainted with the open, informal and non-hierarchical atmosphere of a department in an elite American university.[247]

4.4 Planning for the Martinsried Center. Architectural and organizational structure

According to the MPG's building director Otto Meitinger, planning for new MPIs needed to consider and balance requirements and expectations from several sides, including the state and federal agencies funding the construction as well as the scientists with their specialized styles of work and research methodologies. Architecturally, the new institutes should not only reflect contemporary design, but also integrate innovative building forms and technologies as appropriate to their scientific goals. In order to produce the best possible designs for their newest institutes, during the 1960s the MPG held several architectural competitions involving limited numbers of architects invited to submit proposals that would meet the criteria set forth for each

246 Mössbauer, Rudolf: *Strukturprobleme der Deutschen Universität.* Bremen 1965, cited in Osterwalder, Einführung, 1968, v–viii, viii.

247 Cf. Kienle, Second Mössbauer Effect, 2012, 417–423, 418.

institute.[248] Behind these competitions was the idea that architectural form and scientific organization were inter-related, and that an effective combination of the two could decisively influence the effectiveness of the institute as a research organization. Both in Martinsried and Nikolausberg, the goal from the beginning was to create what came to be called »department institutes«.[249]

In view of the linkage of architectural and organizational structure, it was important to find a way to systematically involve all the future members of the department in the planning as a group, not as competing individuals. Initially there were to be at least nine prospective scientist-members in the Martinsried center, currently working in the three separate biochemical MPIs in Munich. Each was apparently surveyed individually for their needs during the preparation of guidelines for the competition in the summer of 1966, but in December they would constitute themselves as a group, the »Martinsried Consulting Circle« (Beraterkreis Martinsried), later just Martinsried Circle.[250] Each member would have the same status and the same voice in making decisions about the architecture and furnishings of the new center, but they also decided to elect their most prestigious member, Lynen, as a »speaker« who would chair their meetings and take responsibility for the quick decisions needed in the process of construction, subject of course to later confirmation by the group. Butenandt and Grassmann could attend meetings and offer suggestions as non-voting members of the Circle. Each individual voting member would of course have the principal voice in determining the layout and facilities of their own particular laboratory, but broader decisions would be collaborative. Thus, the organization was to be much less hierarchical and more collegial than the typical German institute, and thereby much more like an American department.[251]

4.5 The MPG's »competition of ideas« and its outcome for the Martinsried biochemica center

The »competition of ideas« produced excellent results in the case of the Martinsried biochemical center, thereby exemplifying the advantages of the MPG's approach to institutional design in the mid-1960s. The competition began with an invitation including detailed guidelines and specifications sent to a select group of architectural firms. These were asked to submit innovative solutions to the design problem of creating a large research center comprising several autonomous but interacting sections in a diverse and innovative scientific field. To ensure blind

248 Otto Meitinger: Die Bauaufgaben der Max-Planck-Gesellschaft. *architektur wettbewerbe. Internationale Vierteljahresschrift* Heft 53: Max-Planck-Institute / Max-Planck-Institutes (1968), iv, iv.

249 Altendorfer, Max-Planck-Gesellschaft 1963–1977, 1997, 33–51, 43–44.

250 Protokoll der Sitzung Beraterkreis Martinsried am 5. Dezember 1966, AMPG, II. Abt., Rep. 66, Nr. 686, fol. 31–45.

251 Minutes and decisions of the planning sessions of the Martinsrieder Kreis are extensively documented in the AMPG, with copies both in the Butenandt Nachlass, AMPG, III. Abt., Rep. 84/1, and in the documents of the Generalverwaltung, AMPG, II. Abt., Rep. 66, Nr. 624–625, 686, 688.

judging, the submitted models and plans were not to be labeled with the names of the architects, nor was a return address to be displayed on the delivery crates. For Martinsried the announcement included a definition of biochemistry, reflecting the nature of the discipline as it was perceived in the mid-1960s in Germany.

> Biochemistry deals with the chemical and to some extent the physico-chemical analysis of the processes of life. In view of the methods applied, this analysis takes many forms. Equally diverse are the experimental objects, which may include multi-celled organisms, individual organs, isolated cells and parts of cells, and finally substances in solution. This dual diversity necessitates the organization of biochemistry into numerous subfields and subgroups, which must closely collaborate to achieve a comprehensive insight into the chemical interconnections of life. The planned individual sections of the biochemical center will be working in these subfields of biochemistry, and their spatial proximity should ensure their fruitful collaboration.[252]

Although the research fields for the first nine sections – all of which were to be directed by members of the three biochemical institutes in Munich – and their spatial and technical requirements were already determined, with detailed specifications in the announcements (see Table 2 below), the plans were also to include six additional sections whose areas were not yet specified, plus two functional areas for common facilities. Together, these would be designated Construction Phase 1; the designs were also to consider a prospective Phase 2, consisting of facilities in similar or related fields, taking into account the proximity of the planned Bavarian state medical center in Großhadern to the north (and limited to the west by the village of Martinsried). This fulfilled the MPG's goal of including potential expansion in the design. But along with the scientific requirements of the design, the MPG also specified another requirement, namely that the building itself should express the new center's departmental organization. Reflecting a still-conservative academic culture, the MPG phrased this requirement rather tentatively: »it is to be investigated, to what extent« the building plan could not only incorporate the necessary close spatial connections among the scientific sections and between them and the common facilities, but also express the autonomy of the individual sections, while maintaining the overall unity of the institution.[253]

In addition, design entries were to foster collaboration between individual sections by placing some adjacent to others, in particular: connective tissue (section 2 [Kühn]) with biochemical research methods (7 [Hannig]), experimental medicine (4 [Ruhenstroth-Bauer]) with the central area for experimental animals, and biochemistry of gene expression (8 [Zillig]) with virus research (9 [Hofschneider]). Hoppe's double-section for x-ray structure research (5) also needed to have its electron microscope on a stable, vibration-free ground-floor foundation (this was also

252 Ausschreibung eines Ideenwettbewerbes zur Erlangung von Entwürfen für Institutsneubauten eines Biochemischen Zentrums in Martinsried bei München . . . [der MPG], 1 Aug. 1966, AMPG, II. Abt., Rep. 66, Nr. 686, fol. 92–132, on fol. 128.

253 AMPG, II. Abt., Rep. 66, Nr. 686, fol. 108–109.

required for three electron microscopes in Hofschneider's section), and the central computer center was to be placed near Hoppe's section, which it would primarily serve. Finally, Hoppe's special-area bunker for high-voltage electron microscopy was to be built underground, separate from the other buildings and accessible by an underground tunnel.[254]

TABLE 2A

Allocations of space to proposed sections in the Martinsried biochemical center, Aug. 1966.[255]

Section number	Director (MPI)	Main field	Area (sq. m.)	Apparatus or rooms of special interest	Sq. m.
1	Lynen (Cell Chem.)	cell chemistry [enzymes, Metabolism]	1500[256]	Bacterial breeding, photometry, electrophoresis, ultracentrifuge, animal holding room Constant-climate rooms[257] Isotope lab	 460 160 35
2	Kühn (Proteins)	connective tissue research	1500	Electron microscopy, electrophoresis, ultracentrifuge, animal rooms, cool room Constant-climate rooms Isotope lab (B-Lab)[258]	 250 150 70
3.1	Dannenberg (Bioch.)	organic chemistry	1500	Paper chromatography, microbiology, high-vacuum distillation, hydration Constant-climate rooms Isotope lab	 240 200 90
3.2	Dannenberg	spectroscopy	600	Gas chromatography, general measurement Constant-climate rooms (infrared, NMR, ultraviolet, mass spectroscopy, balance)	 100 250
3	Dannenberg	total	2100		
4	Ruhenstroth-Bauer (Biochemistry)	experimental	1500	Centrifuges, balances, animal stalls, organ preparation rooms Constant-climate rooms Isotope lab (B-Lab)	 340 180 90

254 AMPG, II. Abt., Rep. 66, Nr. 686, fol. 129–130. I will return later to the difficult question of how effectively the architectural design fostered collaboration.

255 Compiled from list in Ausschreibung, 1 Aug. 1966, AMPG, II. Abt., Rep. 66, Nr. 686, fol. 113–123.

256 Besides the special-purpose laboratories itemized here, the space allocated for every section (usually 1500 square meters) included general laboratory and work areas, office space, workshops and storage, and libraries.

257 Unless otherwise indicated, constant-climate rooms consisted of darkrooms and measurement rooms, and occasionally cool rooms. They were to be located in the inner areas of the buildings, away from windows.

258 In the isotope laboratories, »B-Lab« indicated a level of radiotoxicity as specified by the IAEC, Oct. 1965. See Ausschreibung eines Ideenwettbewerbes..., 6. 9. 1966, AMPG, II. Abt., Rep. 66, Nr. 686, fol. 79–85, on 84.

5.1	Hoppe (Proteins)	X-ray structure research	2950 (double)	Computers, x-rays, electronics lab, room for ion-exchange apparatus Constant-climate rooms for electron microscope, cool rooms, measurement, chromatography	600 400
5.2	Hoppe		150	Special area: Bunker for high-voltage electron microscopy	150
5.	Hoppe	total	3100		
6	Braunitzer (Biochemistry)	protein chemistry	1500	Centrifuges, column chromatography Constant-climate rooms Isotope lab (B-Lab)	410 180 70
7	Hannig (Proteins)	biochemistry of genetic	1500	Sterilization rooms, tissue and cell culture, physical testing room Constant-climate rooms Isotope lab (B-Lab)	160 120 90
8	Zillig (Biochemistry)	biochemistry of genetic activity	1500	Measurement, technical, and culture rooms Constant-climate rooms (dark-, cool-, breeding rooms, etc.) Isotope lab (B-Lab)	290 270 40
9	Hofschneider (Biochemistry)	virus research	1500	Measurement, technical, and culture rooms, prep for electron microscopy Constant-climate rooms (dark-, cool-, breeding and balance rooms, rooms for electron microscopy) Isotope lab (B-Lab)	220 200 70
1–9		all sections	15700	all special laboratories and facilities (35% of total)	5525
10–15	not yet appointed		1500 each		
10–15	total space		9000		
Phase 1	total space	all sections	24700		

TABLE 2B

Central supporting and technical facilities (areas for each will be omitted here as subject to modification in final plans).

Central administration	
Classroom area	
Central workshop area	
Central experimental animal area	
Greenhouse	
Central library	
Lecture halls	
Mensa	
Apartments for guests and service personnel	
Main entrance (reception desk) and auto service	
Central power station	
Central technical facilities, including radioactive and normal waste disposal	
Total area for central facilities (sq. m.)	8400
Total area for entire center, Phase 1 (sq. m.)	33100

It is noteworthy that the central facilities of all types were expected to occupy about a quarter of the total constructed area, as opposed to the space allocated to individual sections in the first phase of construction, including those sections yet to be assigned to individual scientists. Of the space for individual sections that were already assigned, about thirty-five percent would go to special laboratories, apparatus (notably, centrifuges, ultracentrifuges, electron microscopes), and facilities as opposed to general laboratory and work space. These were of course preliminary estimates, based on the perceived needs of the scientists working in the existing institutes, and subject to modification with as the planning and construction of the Martinsried center proceeded.

The competitions for the center for biophysical chemistry in Göttingen-Nikolausberg and for the center for biochemical research in Martinsried were held almost simultaneously in the summer and fall of 1966, with Nikolausberg decided shortly before Martinsried. It appears that Meitinger employed the Nikolausberg competition as a learning experience for the future Martinsried biochemists. Hence, a few days before the Martinsried jury was to meet, Meitinger discussed the Nikolausberg results with the newly constituted Martinsried Circle (all of whom had been given the list of criteria for both competitions), explaining the strengths and weaknesses of each design proposal and how the prizes were allocated. Of special significance for biophysical chemistry was the connection between laboratories and workshops, which had to work closely together. Each proposal was to be judged above all from a functional perspective, and a basic principle was that »the smaller the intermediate distance between the individual institutes [i. e. laboratory sections], the better the project«; another factor was the possibility of standardizing individual parts of the overall design to achieve greater cost efficiency. He noted that

for Nikolausberg there was no first prize, and the contract was awarded to one of the two second-prize winners (the proposal by Walter Henn), which despite its weaknesses appeared to be the most promising design.[259]

The prize jury for Martinsried, which met to determine the winners on 16–17 December 1966, included three groups with voting rights: first, seven people with various special interests in the project; second, eight architectural professionals, each of whom would discuss the pros and cons of at least one of the fourteen entries; and third, eight »experts« (six members of the Butenandt and Grassmann institutes and two regional planning authorities for the Martinsried area). The first group included Butenandt and Lynen, Dr. Friedrich Schneider (general secretary of the MPG), Gerhard Schramm (Butenandt's former associate, then director of the MPI for Virus Research) as well as a representative of the Bavarian Ministry of Education (the minister himself, Ludwig Huber, was the only voting juror not present) and Richard Naumann, the senior mayor of the municipality of Planegg. The final member of this group was Emil Tonutti, professor of clinical morphology and vice-rector of the University of Ulm (included presumably to provide a medical perspective, given Martinried's potential connection to the Großhadern medical center). The professional group included Gerd Albers, a specialist in city planning and also then rector of the Munich Technical University (a logical choice considering that university's creation of a physics department, in response to Mössbauer's demands); Meitinger of the MPG; architectural experts for the city of Munich and the Bavarian government; as well as others invited from Munich, Stuttgart, Berlin, and Marburg. The six biochemists in the expert group included Grassmann (who was planning to retire rather than move to Martinsried), Kurt Hannig and Klaus Kühn from Grassmann's institute, and Gerhard Braunitzer, Peter Hans Hofschneider, and Wolfram Zillig from Butenandt's institute. Two men from the MPG's building department and one from the Munich city building department acted as »pre-examiners«, and provided preliminary reports and evaluations of each entry. Finally, there were several non-voting alternate jurors, including Heinz Dannenberg and Gerhard Ruhenstroth-Bauer from Butenandt's institute and Walter Hoppe from Grassmann's institute, along with Edmund Marsch of the MPG general administration and another member of the MPG building department, as well as a Munich architect and Albert Hummel, the chair of the Bavarian state building administration for university medical centers.[260] Thus, all the members of the Martinsried Circle took part in one capacity or another, albeit not with a majority on the jury, but certainly with enough votes to ensure that the winning designs would reflect their priorities. The other members, representing architectural concerns as well as local and regional political and planning interests, seemed likely to ensure an outcome that would be reasonably satisfactory for all those involved. To minimize potential personal bias, the authors of the proposals were not revealed until the end of the competition.

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259 Aktennotiz über eine informative Besprechung in der Bauabteilung ..., 12. 12. 1966, AMPG, II. Abt., Rep. 66, Nr. 686, fol. 63–65.

260 Niederschrift über die Sitzung des Preisgerichts für den Ideenwettbewerb »Neubau eines biochemischen Zentrums in Martinsried bei München« ..., 17 Dec. 1966, AMPG, II. Abt., Rep. 66, Nr. 686, fol. 47–62, list on fol. 48–50.

Under the chairmanship of Horst Linde, professor of university planning at the Technical University of Stuttgart and director of the building construction department in the finance ministry of Baden-Württemberg, the jury began by discussing the broader concerns of the competition, including architectural priorities, regional planning, landscape preservation, and the new center's significance to the Großhadern medical center. An intensive discussion of the various proposals followed, after which the jurors compared models of each design inserted into a larger topographical model of the site. Then, the jury began three rounds of voting to narrow down the entries. Although all the entries had met the basic requirements, only three seemed truly superior; of the rest, the jury eliminated nine by unanimous vote and two others with only one or two opposing votes. On the following day the jury voted to rank the three finalists, after intensive critical assessments of their strengths and weaknesses by Albers, Meitinger, and Werner Düttmann, an architect and city planner from West Berlin.[261] Although these detailed assessments primarily entailed general architectural perspectives, in each case they paid special attention to the relationship between the central common facilities (library, computer center, other large-scale technical apparatus, etc.) and the laboratory sections, as well as to how effectively the design connected the different laboratory ections to each other, given that the potential for collaborative research among different sections throughout the research center was a key criterion in the competition.

Ultimately the prize jury unanimously agreed that only two of the three finalists were outstanding enough to qualify as prize-winners, and that the first prize went to a proposal by the architectural firm of Hannsgeorg Beckert and Gilbert Becker (with four associates) in Frankfurt/Main. The jurors were impressed by the efficiencies inherent in the design and its effective integration into the landscape, as well as by its overall coherence, which attained »the desired structural unity of the whole institute while maintaining the special autonomy of the individual sections«, as well as their equal valuation. This was achieved by what came to be called a »star« design, in which four separate laboratory wings, with either two or four upper floors, were connected at a central junction point (see Figs. 17, 18, and 19 below). This allowed for good communication within each »star« between sections dealing with related research areas, thus enhancing possibilities for collaboration across sectional lines while also securing a desirable flexibility.[262]

261 AMPG, II. Abt., Rep. 66, Nr. 686, fol. 52–53.

262 AMPG, II. Abt., Rep. 66, Nr. 686, fol. 58–60.

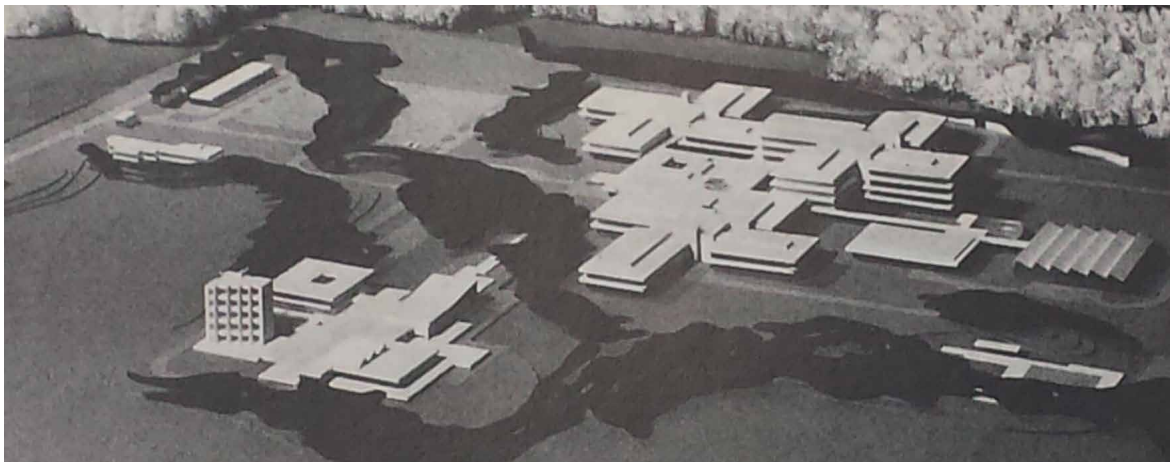

Fig. 17. The model of the winning entry by Hannsgeorg Beckert and Gilbert Becker in the MPG's competition of ideas for the Martinsried biochemical center, December 1966. The main building complex on the right contains three laboratory »stars« (two on the left side with two stories, each containing four laboratory section units, and one on the right with four stories, accommodating eight laboratory section units, for a total of sixteen as in the competition's guidelines). On the far right is the technical workshop area, to its left the experimental animals area. The guest house-cafeteria (*mensa*)-library-lecture hall complex is in the lower left.
Source: Gutachten für Max-Planck-Gesellschaft in: München 1966. Biochemisches Zentrum in Martinsried. *architektur wettbewerbe. Internationale Vierteljahresschrift* 53. Max-Planck-Institute / *Max-Planck-Institutes* (Feb. 1968), 52–63, 52 (detail).

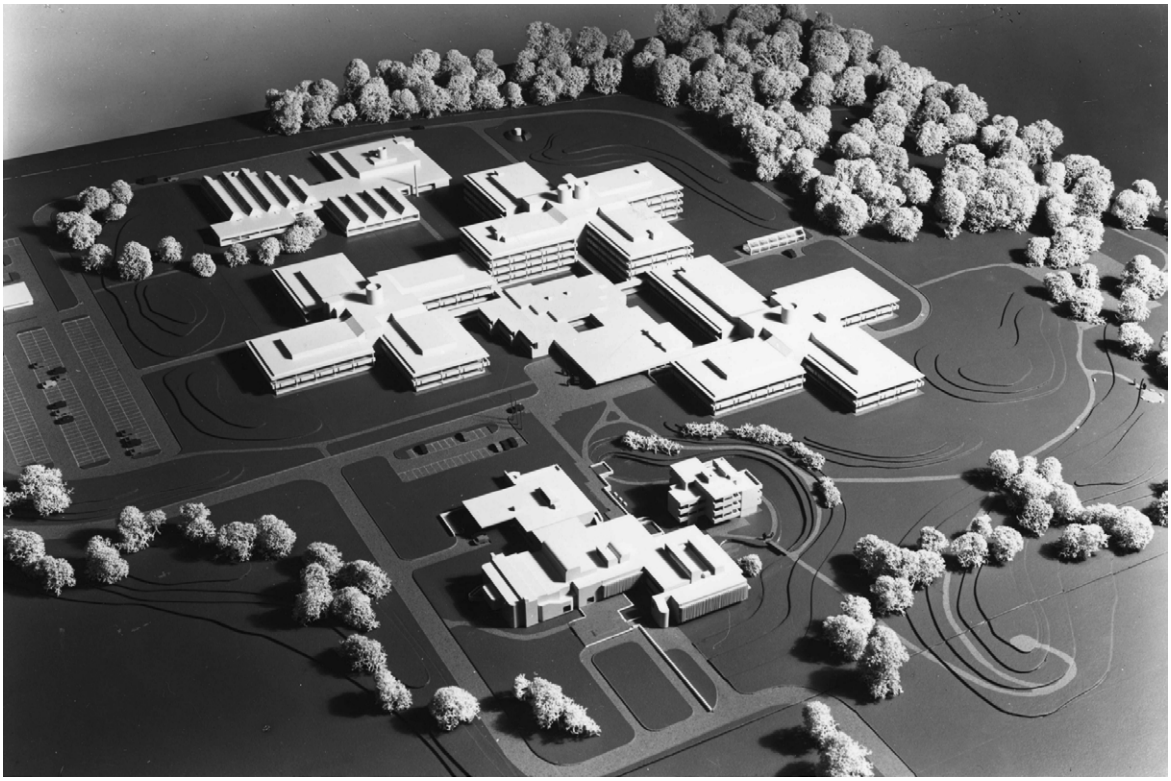

Fig. 18. The construction-plan-based model for the Martinsried biochemical center of 1969, with similar laboratory star units but substantially different designs for the other buildings (compare Figs. 17 and 24 [the latter showing the completed building]).
Source: Archives of the Max Planck Society.

4.6 Allocating laboratory space in the planned biochemical center

The Beckert-Becker »star« design allowed for a straightforward allocation of laboratory space in each section, in accordance with the guidelines (cf. Table 2 above) and as shown in Fig. 19 below, illustrating the upper floor of the first »star« unit.

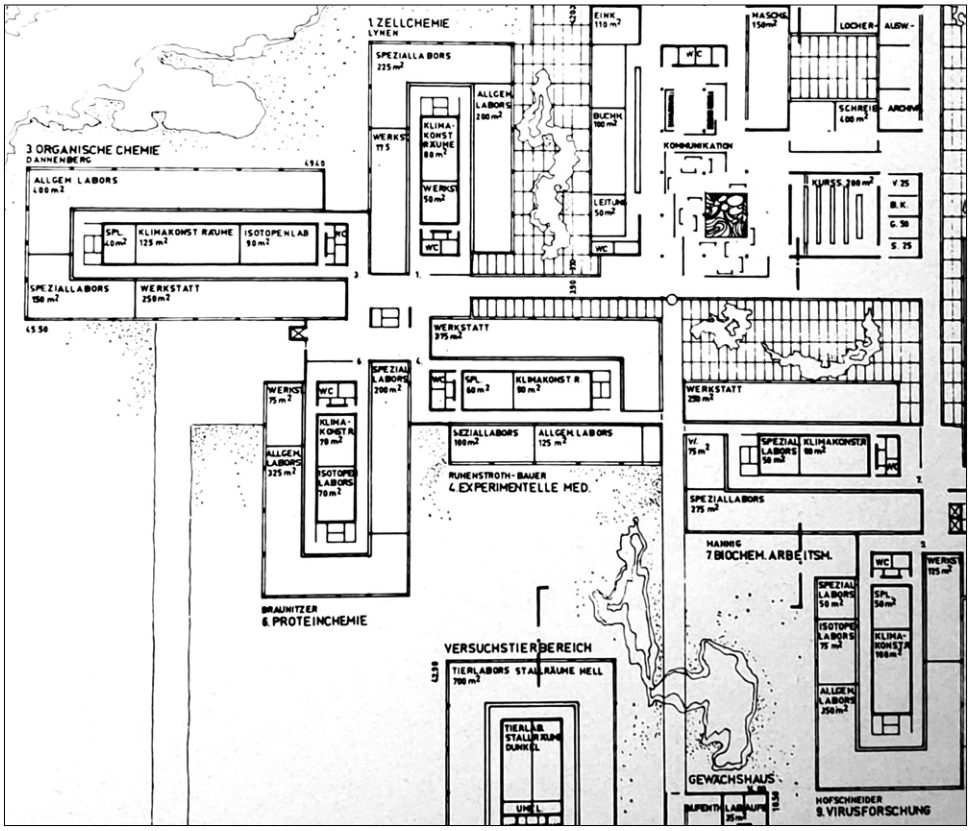

Fig. 19. The proposed layout in the Beckert-Becker design of the ground floor of one laboratory »star« unit, containing (counter-clockwise from top) the sections for (1) cell chemistry (Lynen), (3) organic chemistry (Dannenberg), (6) protein chemistry (Braunitzer), and (4) experimental medicine (Ruhenstroth-Bauer). Each section consists of outer rooms open to sunlight, and an inner block of rooms, surrounded by a corridor, which are away from the outer walls and can serve as dark rooms, constant-climate rooms, etc. Access between sections is via a central hallway with staircase, and on the far right a hallway connects to the next »star« unit in the complex (closest section: (7) biochemical methods (Hannig), which made sense given that Ruhenstroth-Bauer had most frequently collaborated with Hannig in the past [Fig. 21]), as well as to the test-animal section (bottom center), conveniently close to both Ruhenstroth-Bauer's and Hannig's sections.
Source: Gutachten für Max-Planck-Gesellschaft, 1968, 52–63, 55 (detail).

As shown in Fig. 20 below, the subsequent planning sessions of the Martinsried Circle led to a somewhat different allocation of the various sections in the stars of the Martinsried plan than in the Beckert-Becker proposal based on the MPG's competition guidelines. Note that only two »star« units are shown, as the third unit was not part of the first phase of construction. Here we see how Braunitzer and Ruhenstroth-Bauer moved from the left unit to the right, and Lynen and Dannenberg are on the opposite side of the unit than shown in Fig. 19.

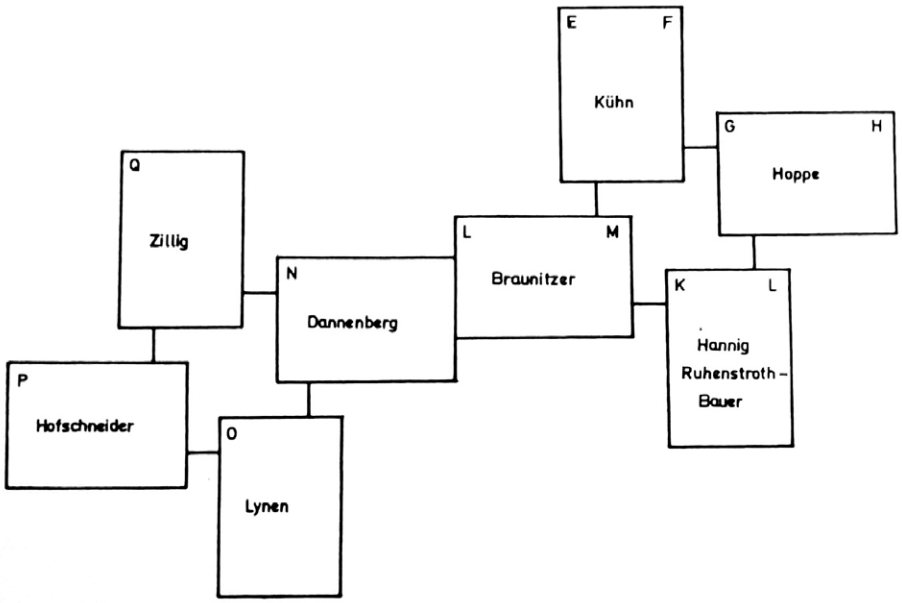

Fig. 20. Allocation of Munich institute section heads to laboratory units (indicated by letters) in the Martinsried »stars«, as proposed by the MPG in initial planning early 1967. See below for the special research interests of each section. Note that in this version, Ruhenstroth-Bauer and Hannig are in the same wing (K, L) of the larger »star« unit, on the right.
Source: InfraTest-CMP, Institut für Unternehmensberatung GmbH München, Beitrag zur Planung des biochemischen Zentrums in Martinsried bei München (November 1967), Part 6, Fig. 14, on p. 97, in Archives of the Max Planck Society, II. Abt., Rep. 41, Nr. 47.

Connected in the MPG plan, 1967:

Laboratory building 1 (two floors, four sections; left side in Fig. 20)

N: Organic chemistry and spectroscopy (Dannenberg)
O: Enzyme chemistry and metabolism (Lynen)
P: Virus research (biosynthesis of nucleic acids and proteins) (Hofschneider)
Q: Molecular biology [previously »biochemistry«] of genetic action (Zillig)

Laboratory building 2 (four floors, eight sections; right side in Fig. 20)

E, F: Connective tissue research (Kühn)
G, H: Structural research (by x-ray crystallography and electron microscopy) (Hoppe)
K: Biochemical methods (Hannig)
L: Experimental medicine (on the level of cells and tissues) (Ruhenstroth-Bauer)
L, M: Protein chemistry (featuring hemoglobin and respiration chemistry) (Braunitzer)

This plan was accepted by the Martinsried Circle and then approved by the Senate on March 10, 1967.[263] To provide additional input into the planning and to enhance prospects for collabora-

263 Butenandt, Geschichte, 1977, 20.

tive research within the new institute, the MPG commissioned an analysis by a consulting firm in Munich, InfraTest-CMP, which from May to October 1967 conducted a detailed study of the interrelationships among the nine scientists in the three Munich biochemical MPIs who were going to lead sections of the Martinsried research center. This was somewhat artificial, as it omitted other groups in the existing institutes that would not be moving to Martinsried. Nevertheless, it did provide a useful basis for illustrating collaborative interactions.

They began by studying the previous pattern of scientific contacts among the three institutes in Munich, up to June 1967 (Fig. 21).

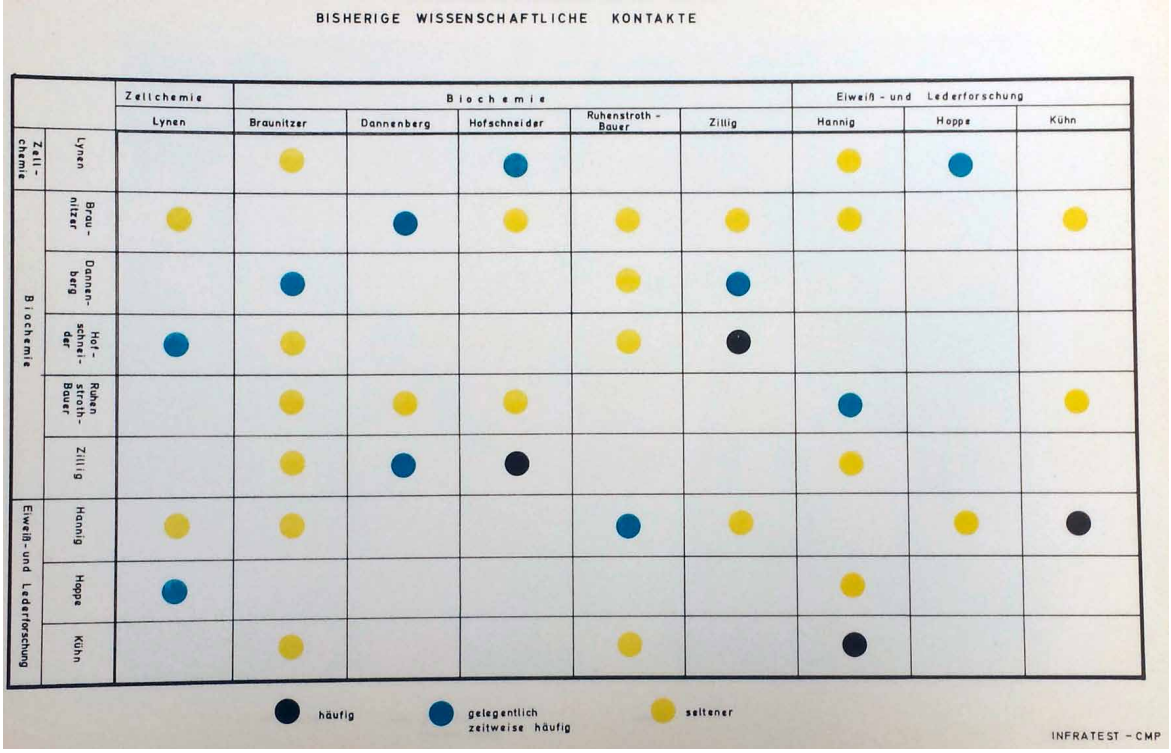

Fig. 21. Illustration 1 from the consultants' report on planning for the Martinsried biochemical center, highlighting the level of previous scientific contacts up to June 1967 (frequent in black, occasionally frequent in blue, rare in yellow) among the section heads of the three Munich MPIs who were expected to move to Martinsried. Note the absence of contacts in many cases.
Source: InfraTest-CMP, Beitrag, 1967, Part 4, Fig. 1, in AMPG, II. Abt., Rep. 41, Nr. 47.

By appropriately allocating the sections within the new center, the consulting firm believed that scientific contacts and exchanges among them would increase, thus maximizing the prospects for inter(sub)disciplinary collaboration (cf. Fig. 22 below). This was decisive in confirming the argument for bringing the previously separate institutes together in a large, connected building.

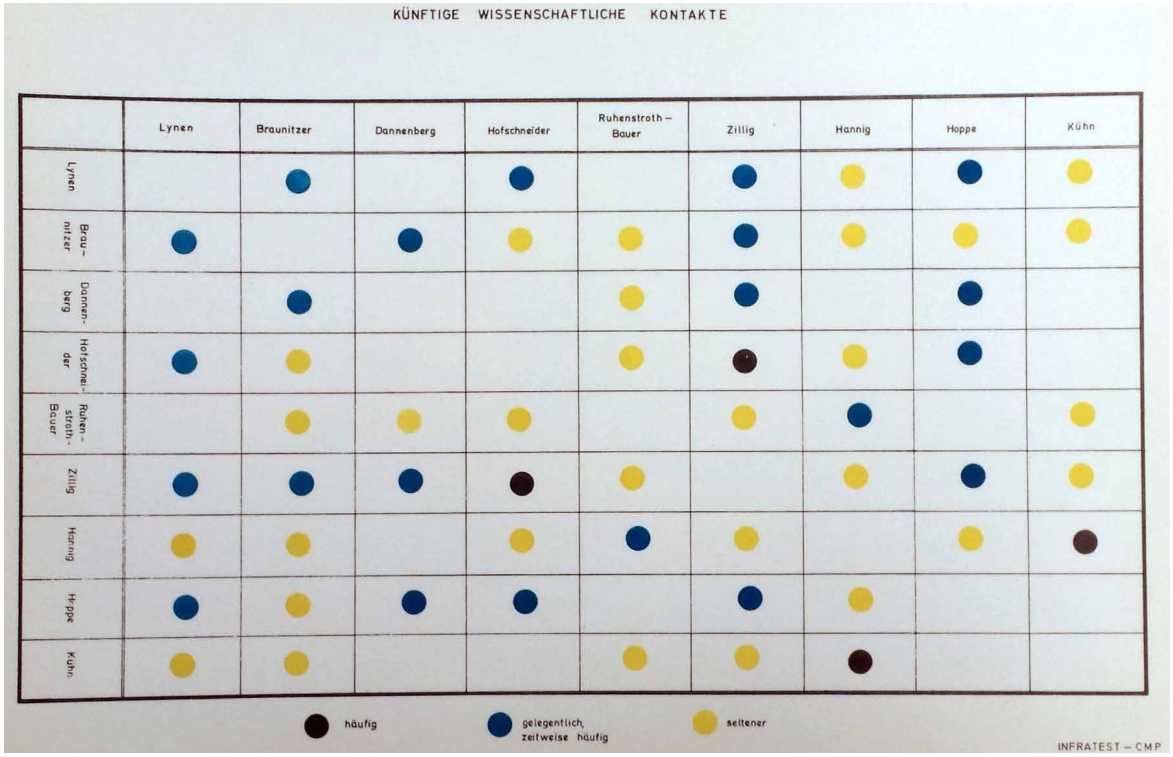

Fig. 22. Illustration 5 from the consultants' report on Martinsried, highlighting the higher level of scientific contacts expected from an optimal arrangement of sections in the new center. One sees here that while the most frequent contacts were expected to remain the same, there were supposed to be more than twice the number of intermediate level contacts (blue dots) and a significantly greater number of low-level contacts than in the separate institutes shown in the previous figure. Note however that in the case of Hannig, Kühn, and Ruhenstroth-Bauer, intermediate- or higher-level contacts were expected only with each other (as before), not with any other sections, suggesting a somewhat self-contained group belonging to a separate unit (cf. Fig. 23).
Source: InfraTest-CMP, Beitrag, 1967, Part 4, Fig. 5, in Archives of the Max Planck Society, II. Abt., Rep. 41, Nr. 47.

The InfraTest-CMP consultants, however, argued for a somewhat different allocation of sections than the MPG and the Martinsried Circle had thus far proposed (compare Fig. 23 below with Fig. 20 above). In some ways the InfraTest-CMP alternative might have been superior to the MPG's suggested layout. It certainly made sense, for example, to put Hofschneider and Zillig in the same building, given that Hofschneider's work on biosynthesis would overlap well with Zillig's molecular biology. Similarly, Dannenberg and Braunitzer were both working in areas of what could be called bio-organic chemistry. It should be recalled, however, that the original guidelines for the prize competition had already specified that Hofschneider and Zillig should be adjacent to each other. The 1966 guidelines had also specified that Kühn and Hannig be adjacent (as here recommended by InfraTest-CMP), and the MPG plan did attain this goal by locating both in the same »star«. The MPG ultimately chose to continue with its original layout, albeit with a different numbering of the sections than shown in Fig. 20 above. This in turn became obsolete with the appointment of three additional section heads in the early 1970s.

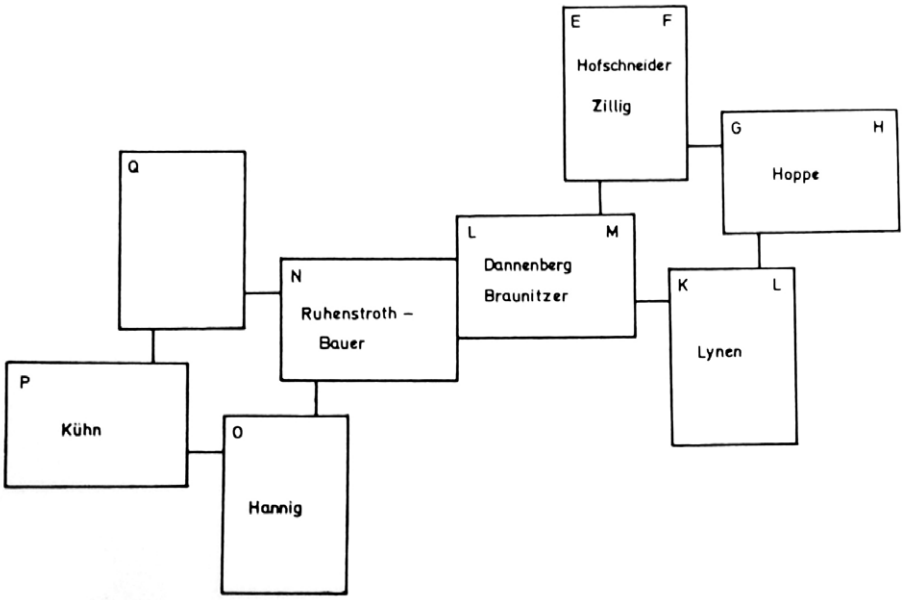

Fig. 23. Allocation of sections to laboratory units as proposed by InfraTest-CMP (see Table 3 below for specialties), based on frequency of contacts in Munich institutes (cf. Figs. 21 and 22 above; because Hannig had collaborated most frequently with Kühn, followed by Ruhenstroth-Bauer, it seemed logical to have all three in one unit). This was not accepted by the MPG.
Source: InfraTest-CMP, Beitrag, 1967, Part 6, Fig. 15, in Archives of the Max Planck Society, II. Abt., Rep. 41, Nr. 47.

TABLE 3

Allocation of sections to buildings and wings in the Infratest-CMP proposal (November 1967). (Table based on Fig. 23 above. Section designations were later changed, so that sections A, B, C, D were in building 1, and E, F, G, H, I, K, L, M were in building 2, as in Table 4 below)

Laboratory building 1

N: Experimental medicine (on the level of cells and tissues) (Ruhenstroth-Bauer)
O: Biochemical methods (Hannig)
P: Connective tissue research (Kühn)
Q: not yet assigned

Laboratory building 2

E: Virus research (biosynthesis of nucleic acids and proteins) (Hofschneider)
F: Molecular biology [previously, »biochemistry«] of gene expression (Genwirkung) (Zillig)
G, H: Structural research (by x-ray crystallography and electron microscopy) (Hoppe)
K, L: Enzyme chemistry and metabolism (Lynen)
L: Organic chemistry and spectroscopy (Dannenberg)
M: Protein chemistry (featuring hemoglobin and respiration chemistry) (Braunitzer)

On the basis of the MPG's discussions up to mid-1968, the architects prepared and submitted detailed construction plans, which were approved by the regional authority (Landratsamt) in Munich in August 1969, allowing construction to begin. According to these plans, the sectional allocations were as shown in Table 4 below.

TABLE 4

Allocation of sections to buildings and wings as of summer 1968 (based on plans submitted by Beckert and Becker).[264]

Section (Letter, Location)	Field	Section head
Laboratory building 1 (2 floors, 1 section in each wing)		
A (North)	Biochemistry of gene expression	Zillig
B (West)	Virus research	Hofschneider
C (South)	Cell chemistry	Lynen
D (East)	Org. chemistry, spectroscopy	Dannenberg
Laboratory building 2 (four floors, 2 sections in each wing)		
West wing:		
E	Connective tissue research	Kühn
F	Protein chemistry	Braunitzer
South wing:		
G	Biochemical research methods	Hannig
H	Experimental medicine	Ruhenstroth-Bauer
East wing:		
I, K	X-ray structural research	Hoppe
North wing:		
L, M	Vacant	To be appointed
Laboratory building 3 (2 floors, sections N–Q, reserved for 2nd phase)		

4.7 Building the new MPI, 1969–1973

Construction began in June 1969, with the roofing celebration (Richtfest) in November 1970. Internal furnishing took much longer to complete. The Munich institutes did not begin to move into the new building until late 1972, after they were officially unified on August 1 as the MPI

264 Copies of detailed plans by Architektenbüro Beckert and Becker, Frankfurt/M., in Archiv und Galerie der Gemeinde Planegg, GP 2104/1 and GP 2104/2, dated variously from May to July 1968, and all labeled Baugesuch vom 15. 8. 1968, Max-Planck-Gesellschaft München. Plans are stamped approved by Landratsamt München, 27. 8. 1969, s. Roselius.

for Biochemistry (a name accepted by the Martinsried Circle from December 1966 and ratified by the Senate of the MPG on March 10, 1967, even though the architectural planners continued to use the name Biochemical Center Martinsried). When the Martinsried MPI officially opened on March 23, 1973, the overall process of planning, building, and furnishing the new MPI had taken the better part of seven years. The huge resulting total cost, around 85.5 million DM, was unprecedented for a single MPI, yet it represented only the first phase of construction, omitting the third laboratory star unit. To put this figure into perspective, it exceeded the entire operating budget of the MPG (81 million DM) when Butenandt became president in 1960. By 1966 the budget had tripled to 245 million DM, and its subsequent expansion largely came from the growth of federal funds after the agreement of 1964. Although the number of institutes in the MPG did not increase from 1966 to 1972, remaining at a total of 52 when Butenandt retired, the closing or consolidation of older, smaller institutes into larger, more expensive institutes like Martinsried increased the number of permanent scientific staff, fellowships, and guests from 4,000 to 10,000, and more than doubled the MPG's operating budget to 528 million DM.[265]

4.8 The Martinsried MPI as completed

Figure 24 below shows the MPI for Biochemistry in Martinsried as it appeared at the opening in 1973, with the first phase complete. From the outline of a third laboratory star unit on the ground, it appears that the MPG had intended to proceed with a second phase of building in the near future. But these expectations proved to be illusive in the difficult economic climate of the 1970s. Nevertheless, the completed building complex was enough to unite the three previous institutes in strikingly innovative accommodations.

Only with the completed building did a key aspect of the laboratory building design manifest itself most clearly: its horizontality, as seen from a distance (Fig. 25 on page 97 below). Why was this important? The essence of a department is its non-hierarchical nature, providing relative autonomy and equality to all members. This essential idea is reflected in the wide, low-lying buildings of the Martinsried laboratories, accented by strongly horizontal, alternating light and dark stripes, which serve to unify the entire structure as specified in the original guidelines of the 1966 competition. The importance of horizontality becomes even more obvious when set against a competing design that was not among the finalists in the 1966 competition, but for some reason was the only other submission for which the plans and layout were preserved in the archives of the MPG. One may surmise that this was the alternative option that Butenandt found attractive, as he recalled in 1977, because it conveyed to him a sense of the old institutional relationships in Dahlem.[266] The striking feature of this design, prepared by the architect

265 Adolf Butenandt: Ansprache des Präsidenten Professor Dr. Adolf Butenandt in der Festversammlung der Max–Planck–Gesellschaft in Bremen am 23. Juni 1972. In: Generalverwaltung der Max-Planck-Gesellschaft (ed.): *Jahrbuch der Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1972*. München 1972, 28–47, 32.

266 Butenandt, Geschichte, 1977, 19.

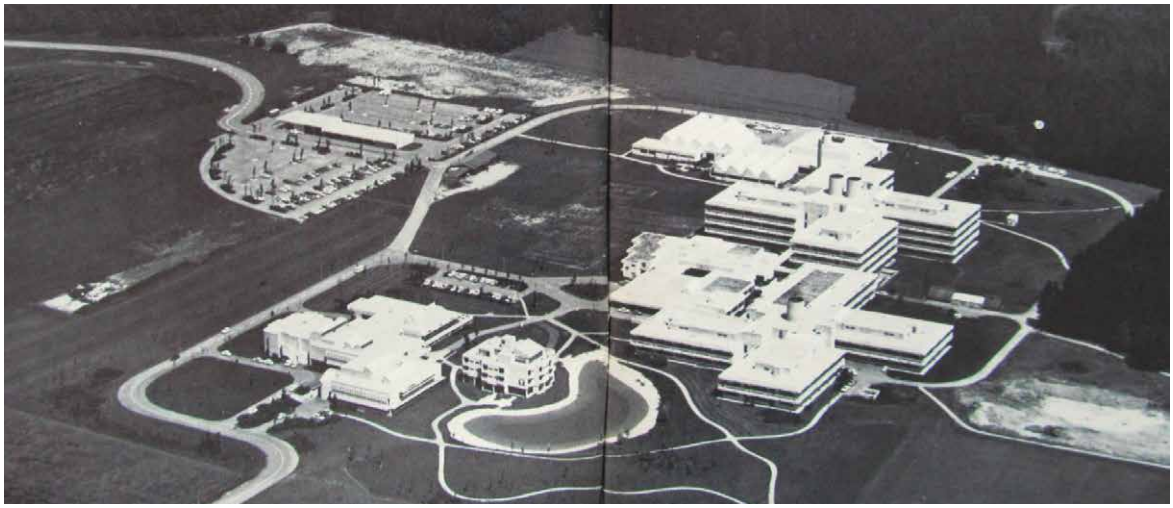

Fig. 24. The MPI for Biochemistry, Martinsried, around 1977. The main building complex, on the right, houses the administration and in its wings, the specialized research sections (one in each wing of the 2-story »star« in front [ABCD] and 2 in each wing of the 4-story »star« to the rear [EF/GH/JK/LM]); the outline of the planned third »star« unit is faintly visible to the left of the main complex. Behind is the central workshop, storehouse, and animal care facility. The guest house is the small building facing an artificial lake (bottom center); to its left, the library, cafeteria and lecture hall complex. Compare the construction model (Fig. 18). Source: Max-Planck-Gesellschaft (ed.): *Max-Planck-Institut für Biochemie*. Max-Planck-Gesellschaft. Berichte und Mitteilungen 2, 1977, frontispiece.

Egon Eiermann in Munich, is the pyramidal shape of the laboratory buildings (Figs. 26 and 27), in each of which there are two sections, whose directors occupy offices near the top of the pyramid. This could facilitate communication between the directors, given that they are separated only by a narrow hallway, and this may have jogged in Butenandt pleasant memories of his collaborations and stimulating discussions with the institute directors in biology and anthropology before and during the war in Dahlem.[267] At the same time, however, the pyramidal design isolates the directors from the other members of their section in a building whose shape conveys a strong sense of hierarchy. Despite Butenandt's recalling this design as »attractive«, it was unanimously rejected in the second round of the competition.[268] Not even he had been prepared to vote for a design that effectively denied the principle of collegiality set out in the guidelines for the competition.

267 Cf. Rheinberger, Zusammenarbeit, 2004, 169–197; Carola Sachse: Adolf Butenandt und Otmar von Verschuer. Eine Freundschaft unter Wissenschaftlern (1942–1969). In: Schieder and Trunk, *Butenandt*, 2004, 286–319, esp. 294 citing Butenandt on the pleasure of conversations with Verschuer toward the end of his time in Dahlem.

268 Niederschrift über die Sitzung des Preisgerichtes »Neubau eines biochemischen Zentrums«, 17 Dec. 1966, AMPG, II. Abt., Rep. 66, Nr. 686, fol. 53.

Fig. 25. Photograph of the newly completed MPI for Biochemistry (1973), with wings B and C of the first laboratory star in the foreground and parts of wings EF and GH of the second star in the background.[269]
Source: Otto Meitinger: Die Neubauten der Max-Planck-Institute in Göttingen und München. *Baumeister* 70 (1973), 1251–1264, 1260 (reproduced from copy in Archives of the Max Planck Society, III. Abt., Rep. 84/1, Nr. 685).

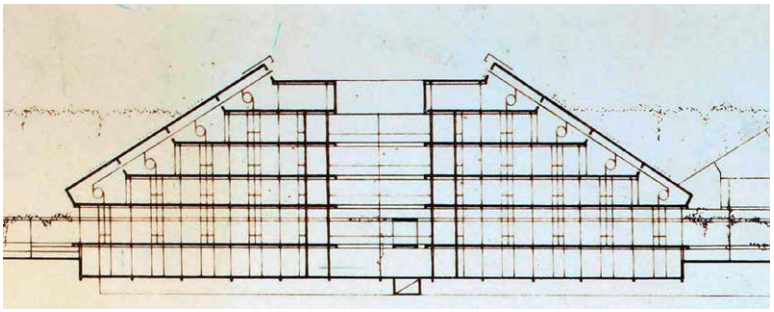

Fig. 26. Longitudinal section C-C of a laboratory building in the southeast corner of the Martinsried competition entry by Egon Eiermann (detail). Each end of the building houses a scientific section with its laboratories on the lower floor, separated by the central hallway space containing apparatus common to both sections. The offices of the directors of each section, with their secretaries, libraries, and a seminar room, face each other across the central hallway and lounge on the third upper story of each building (the top floor is reserved for the ventilation system).
Source: Archives of the Max Planck Society, IV. Abt., Rep. 2: Karten und Pläne der MPG, Nr. 0983–1

Fig. 27. Detail (southeast corner) of the architectural model of the design proposed for Martinsried by the architect Egon Eiermann. The pyramidal structures are the laboratory buildings, each containing facilities for two sections connected by a narrower junction area to contain apparatus common to both sections. While the central junction is meant to be the locus of collaborative work, its narrowness and darkness seems to belie the significance of collaboration. Compare the longitudinal section in Fig. 26 above, which applies to the building in the upper right corner of the five-building unit shown here.
Source: Gutachten für Max-Planck-Gesellschaft, 1968, 52–63, 53.

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269 The design of the MPI for Biophysical Chemistry in Göttingen-Nikolausberg, which was built at the same time as the Martinsried MPI, also exhibits a similar trait of horizontality through the use of cantilevered balconies creating lines of alternating light and dark stripes to tie together a line of five square laboratory units. Illustrations in Altendorfer, Bauten für die Forschung, 1997, 43.

The interior of the completed laboratory units at Martinsried was highly complex, reflecting the diversity and complexity of apparatus and methodologies in the biochemistry of the time, with some 50 different types of rooms or apparatus, just for one floor (Fig. 28 below; compare the competition entry, Fig. 19, and sectional structure, Table 4).

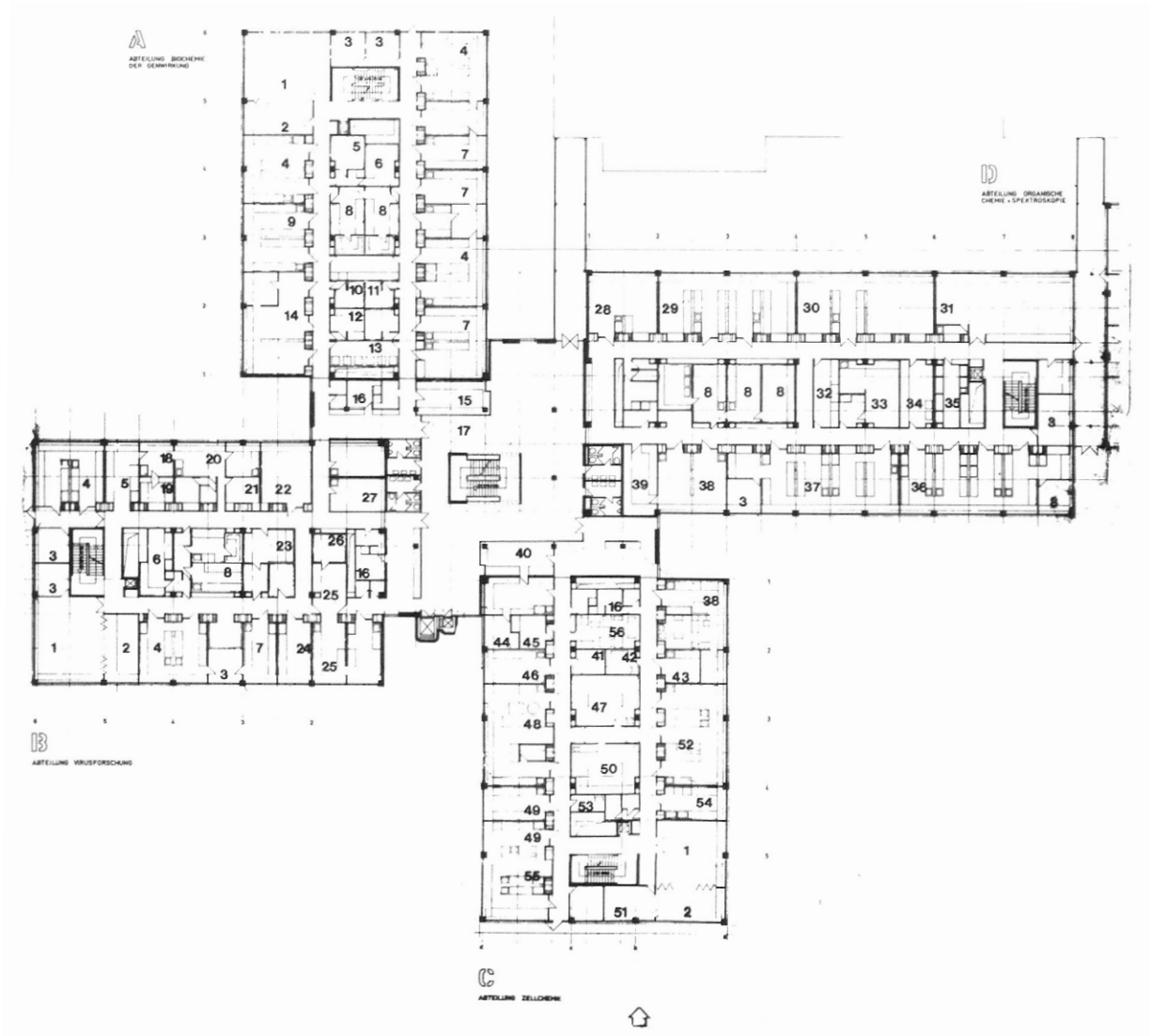

Fig. 28. Ground floor of the two-story star unit ABCD (legend below).
Source: Meitinger, Die Neubauten, 1973, 1251–1264, 1261 (reproduced from copy in Archives of the Max Planck Society, III. Abt., Rep. 84/1, Nr. 685).

Legend to Fig. 28 above: the parenthetical phrases in the field designations for Zillig (A) and Lynen (C) represent changes in their self-description after the submission of the original building plans in 1968

A = Zillig, biochemistry (molecular biology) of gene expression
B = Hofschneider, virus research
C = Lynen, cell chemistry (enzymes and metabolism)
D = Dannenberg, organic chemistry & spectroscopy

1 Colloquium
2 Library
3 Writing room
4 Double lab
5 Isotope measurement room
6 Isotopes
7 Technical lab
8 Cold lab
9 Tissue breeding
10 Constant climate (warm)
11 Constant temperature (warm)
12 Darkroom
13 Centrifuges
14 Development lab
15 Chemicals
16 First-aid room
17 Hall
18 Single lab
19 Photo darkroom
20 Work room
21 Electron microscope
22 Electron microscope developer
23 Measurement room
24 Bacterial breeding
25 Scullery
26 Constant temperature (cool)
27 Glass storage
28 Measurement room
29 Infrared and UV spectroscopy
30 Nuclear resonance (NMR) and electron spin resonance
31 Mass spectrometry and gas chromatography
32 Balance room, constant climate
33 Counter-current distributor, constant-climate night operation, paper chromatography
34 Apparatus room for night operation
35 Night operation
36 Chemical lab
37 Biochemical lab
38 Development lab
39 Storage
40 Glassware and chemical storage
41 Animal holding, constant climate
42 Ultracentrifuge
43 Craig distribution [chromatography]
44 Lab technician [Laborant]
45 Photo lab
46 Analytic lab
47 Centrifuge
48 Microbiology lab
49 Measurement room
50 Cool room
51 Writing and reading room
52 Technical training
53 Breeding room
54 Scullery
55 Measurement room
56 Bacteria and fungus breeding

One can compare the list above to that in Table 2a. As expected in the planning guidelines, constant-temperature and constant-climate rooms as well as darkrooms and certain types of measuring rooms, including isotope labs, are in the central part of each section, isolated from contact with outer walls and windows. Not listed here, however, are rooms set aside in the original plans of 1969 for (international) guests and younger scientists, who were given special priority during the planning process, as will be noted in the following section. It appears that the building units as initially completed may have lacked dedicated rooms of this type.

4.9 Staffing the Martinsried MPI. Openings to internationalism, youth, and molecular biology?

The Martinsried Circle represented the core of the scientific leadership in the new Martinsried MPI, but they could not simply transfer as a group to the new institute. Instead, by MPG regulations, each of them would have to be »called«, undergoing review by the Scientific Council of the MPG in the same way as any new appointment to an MPI. Nor, given the imperative to expand, could the institute leadership remain restricted to the original Martinsried Circle and their particular array of research fields. Moreover, over the near-decade of the planning period, those fields and their special interests continued to evolve, leading in some cases to new self-definitions. A special priority was to internationalize the new MPI's staff by recruiting at

least one prominent scientist from abroad. In this the planners were successful, though they may have been less happy with the ultimate results. Providing opportunities for training younger scientists, postdocs, and doctoral students was also a priority; organizing this required different approaches than in Munich, where many of the MPI staff also held positions on the faculty of the LMU or the Technical University, providing them a ready source of new talent.

It was expected from the beginning that each section in the Martinsried center would be significantly larger than the corresponding laboratories in the three existing biochemical MPIs in Munich. Limited space in these institutes made them »rather top-heavy« in their scientific staff, so that the average ratio of these to their supporting staff was only 1:2.5, which seemed »very low« to the central planners.[270] Martinsried would have far more space for scientific guests, early-career scientists [Nachwuchswissenschaftler], and fellowship holders, thus increasing the need for supporting staff beyond what might be expected from the increase in permanent scientific positions. Indeed, as Dr. Edmund Marsch pointed out, »guests and early-career scientists cannot be viewed as fellowship-holders in the conventional sense; instead, they will be a permanent feature and cost as much as the holder of a permanent scientific position [Planstelleninhaber].«[271] Hence the planners put fellowship-holders in a separate category in addition to the permanent staff in each new section at Martinsried. The latter would thus include eight positions in each of twelve sections, as initially planned: a director, five permanent staff associates, and two guests or early-career scientists. Two fellowship holders would be extra. Preliminary planning for the transition from the three existing institutes in Munich to the new central institute in Martinsried indicated the need for a 50% increase in budgeted staff from 30 senior scientific staff in 1969 to 45 in 1973. The additional positions were to be added in stages, with increases of 2 or 3 in each year's new budget, culminating in a larger addition of 7 new positions in 1973. The director of planning for Martinsried, Krell, calculated that for each scientific position, there would need to be additional supporting staff at a ratio of 1:3.5, making a total of 158 by 1973. Given that the three institutes had only 76 non-scientific staff positions in the 1969 budget, they would need to more than double their supporting staff. This apparently disproportionate increase in supporting staff could be attributed to a disproportionately low proportion of supporting staff in the existing institutes, as reflected in the lower ratio indicated above.

Another challenge arose from the continuing development of molecular biology; several members of the Circle, as well as other scientific members of the MPG, had a growing interest in that field and were increasingly orienting their research in that direction. Because the planning period coincided with discussions about the founding of a European Molecular Biology Laboratory (EMBL), for a time there appeared to be a real prospect of integrating the EMBL into the Martinsried complex. Much to the surprise of the planners, however, that prospect vanished, a development that highlights the problematic relationship between the two disciplines.

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270 Dr. Marsch: Schlüsselzahlen, 19 Oct. 1967, AMPG, II. Abt., Rep. 66, Nr. 648, fol. 107–108.

271 Dr. Marsch: Für Haushaltsausschußsitzung am 25. 10. 1967, 19 Oct. 1967, AMPG, II. Abt., Rep. 66, Nr. 648, fol. 104–106.

4.10 The imperfect opening to molecular biology: Why the EMBL did not go to Martinsried

Thus, the late 1970s–early 1980s became a period of significant transition for the Martinsried center. The younger researchers who replaced the departed biochemists shifted the institute's focus increasingly toward molecular biology. This was a very different situation from November 1961, when (as Benno Müller-Hill found in his initial exploration of the Butenandt Papers) Ruhenstroth-Bauer had written a dismissive reply to Heinrich Matthaei's inquiry regarding an assistantship and possible Habilitation in Munich. Matthaei was an ordinary postdoc, but the significance of his work was only just becoming known. Working in collaboration with Marshall Nirenberg at the National Institutes of Health in Bethesda, Maryland, only six months earlier Matthaei had helped to decipher the first triplet in the genetic code by carrying out the poly-U experiment that produced the amino acid phenylalanine from a cell-free preparation of artificial RNA composed entirely of uracil-containing nucleotides, thus demonstrating that the active codon was a multiple of U, later confirmed as the triplet code UUU. In August 1961, Nirenberg had presented their results to an enthusiastic plenary session of the International Congress of Biochemistry in Moscow, and their joint paper had just appeared in a prestigious American journal. It was one of the foundational experiments in modern molecular biology, yet Ruhenstroth-Bauer (whose main area of experimental work had little to do with nucleic acids) was evidently unaware of its significance.[272] Even so, perhaps one should not be too hard on Ruhenstroth-Bauer, because better-known scientists also doubted that »two outliers of the scientific enterprise« could have discovered the genetic code. In particular, they had shown their paper before publication to a number of prestigious scientists who had not grasped its significance, and later Matthaei had had a disappointing interview with Max Delbrück in Cologne at the institute Müller-Hill would eventually direct. After being rejected in Munich, he took a two-year assistantship at the MPI for Biology in Tübingen, then moved to the Medical Research Institute (Forschungsanstalt) of the MPG (1965: MPI for Experimental Medicine) in Göttingen, which in 1963–1964 added to its goals the deciphering of the genetic code.[273]

Matthaei's initial difficulties in finding a good position in an MPI, despite the fundamental implications of his research, illustrate the problematic situation of molecular biology in German institutions at that time. As is well known, German scientists lagged behind their colleagues elsewhere in developing the discipline of molecular biology, as it became known during the 1950s in Britain, France, and the USA.[274] Matthaei's case shows that without a change in atti-

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272 Benno Müller-Hill: Erinnerung und Ausblendung. Ein kritischer Blick in den Briefwechsel Adolf Butenandts, MPG Präsident 1960–1972. *History and Philosophy of the Life Sciences* 24/3/4 (2002), 493–521, 514; cf. Marshall W. Nirenberg and J. Heinrich Matthaei: The Dependence of Cell-Free Protein Synthesis in *E. Coli* upon Naturally Occuring or Synthetic Polyribonnucleotides. *PNAS* 47/10 (1961), 1588–1602. doi:https://doi.org/10.1073/pnas.47.10.1588.

273 Franklin H. Portugal: *The Least Likely Man. Marshall Nirenberg and the Discovery of the Genetic Code.* Cambridge, MA: MIT Press 2016, 87–90; Generalverwaltung der Max-Planck-Gesellschaft (ed.): Medizinische Forschungsanstalt der Max-Planck-Gesellschaft z. F. d. W., Göttingen. *Jahrbuch der Max-Planck-Gesellschaft 1964.* München 1964, 248–253, 251.

274 Cf. Simone Wenkel: *Die Molekularbiologie in Deutschland von 1945 bis 1975. Ein internationaler Vergleich.* Inauguraldissertation. Köln: Universität zu Köln 2013. https://d-nb.info/1049523393/34. Last accessed 6/10/2020; Ute Deichmann:

tude, the growth of training and research in biochemistry in Germany during the 1960s would not necessarily offer concurrent opportunities to develop molecular biology. Nevertheless, one should not exaggerate the now-conventional view of the fundamental hostility between these disciplines. As a multidisciplinary science, molecular biology could certainly benefit from some varieties of biochemical research, and there were certainly many German biochemists with a growing interest in the new field. This can be seen in the early membership of the European Organization for Molecular Biology (EMBO) following its establishment in 1963–1964. No German-based biochemists were involved in the initial discussions of 1963 leading to EMBO, but participants in follow-up discussions on prospective members included Butenandt's former Tübingen colleagues Hans Friedrich-Freksa, director of the MPI for Virus Research (who was in communication with the EMBO's first elected leaders Max Perutz (chair) and John Kendrew (secretary-general) in Cambridge), and the biophysicist Alfred Gierer, director of that MPI's newly created (in 1960) section for molecular biology.[275]

Once the EMBO was formally organized in 1964, its council invited Friedrich-Freksa and 21 other members of the MPG to join, including Butenandt and other biochemists as shown in Table 5 (in order to highlight membership from the MPG, I have not included members from the universities and other research institutions). What the EMBO subject classification of these researchers demonstrates, however, is the limited significance of classic biochemistry for molecular biology as it was envisioned in the mid-1960s. Notably absent was Richard Kuhn or anyone else from his chemistry section in Heidelberg, as opposed to the section for physiology. Not surprisingly, the location with the largest share of members was Tübingen with eight, but it was closely followed by Munich. The six Munich members included a majority of the Martinsried Circle as of 1967 (Butenandt of course without voting rights). Matthaei was one of the two members from Göttingen (with Manfred Eigen). The most common subject area was structural biology and biophysics (more than 40% of the members). The subject areas »proteins and biochemistry« and »cellular metabolism« appear linked to 32% of the members, but in several of these cases (e. g., Lynen) it was necessary to assign these areas manually due to missing data in the database, which leads one to wonder how actively involved in EMBO those individuals were.[276]

Emigration, Isolation and the Slow Start of Molecular Biology in Germany. *Studies in History and Philosophy of Science Part C. Studies in History and Philosophy of Biological and Biomedical Sciences* 33/3 (2002), 449–471. doi:10.1016/S1369-8486(02)00011-0.

275 Alfred Gierer: Curriculum Vitae. *Max Planck Institute for Biology Tübingen.* https://www.bio.mpg.de/185356/curriculum-vitae. Last accessed 2/27/2023. Max Planck Institute for Biology Tübingen: Formal Curriculum Vitae in German. https://www.bio.mpg.de/191445/formal-curriculum-vitae. Last accessed 3/21/2023; Wenkel, *Molekularbiologie*, 2013, 106; Georgina Ferry: *EMBO in Perspective. A Half-Century in the Life Sciences.* Heidelberg: European Molecular Biology Organization 2014.

276 Files of the Wissenschaftlicher Rat on molecular biology 1964–69, AMPG, II. Abt., Rep. 62, Nr. 1309.

TABLE 5

Scientific members of the MPG who joined EMBO during the founding years 1963–1967, with their institutional affiliation and subject areas as defined by EMBO (see abbreviations below).[277]

MPG Member	Birth-death (years)	MPI/section (location)	Year joined EMBO	CTA/ D/N	PB/ EE	D/N/ SB	CM/ MT	GSD/ SBB	MM/ ST/ SBB
Beermann, Wolfgang	1921–2000	Biol. (Tüb.)	1964	1					
Henning, Ulf	1929–2000	Biol. (Tüb.)	1964				1		
Reichardt, Werner E.	1924–1992	Biol. (Tüb.)	1964						1
Bonhoeffer, Friedrich	1932–2021	Virusforsch. (Tüb.)	1967	1					
Friedrich-Freksa, Hans	1906–1973	Virusforsch./ Physik. Biol. (Tüb.)	1964	1					
Gierer, Alfred	1929–	Virusforsch./ Mol. Biol. (Tüb.)	1964			1			
Schäfer, Werner	1912–2000	Virusforsch./ Biol.-med. (Tüb.)	1964						1
Schramm, Gerhard	1910–1969	Virusforsch./ Bioch. (Tüb.)	1964		1*				
Braunitzer, Gerhard	1921–1989	Biochem. (Munich)	1964		1				
Butenandt, Adolf	1903–1995	Biochem. (Mun.)/ MPG Pres.	1964	1					
Hofschneider, Peter Hans	1929–2004	Biochem. (Mun.)	1964					1	
Zillig, Wolfram	1925–2005	Biochem. (Mun.)	1964						1
Hoppe, Walter	1917–1986	Eiweiss & Lederf. (Mun.)	1964		1*				
Lynen, Feodor	1911–1979	Zellchem. (Mun.)	1964				1*		
Fischer, Herbert	1919–1981	Immunbiol. (Freiburg/Br.)	1964	1					
Westphal, Otto	1913–2004	Immunbiol. (Freiburg/Br.)	1964						1
Eigen, Manfred	1927–2019	Physik. Ch. (Gött.)	1964		1				
Matthaei, Heinrich	1929–	Exper. Med. (Gött)	1964						1

277 Compiled from the EMBO database (https://people.embo.org/). My thanks to the (now former) archivist of EMBL, Anne-Flore Laloë, for suggesting this source and others, as well as providing me with documents from the EMBL Archives (EMBLA) in Heidelberg (primarily from the Kenneth Holmes Papers, hitherto cited as Holmes Papers, EMBLA, DE 2324 P-HOL) in the spring of 2021. Note that Holmes himself is not listed, although he joined the EMBO in 1967, because he was not yet a scientific member of the MPI for Medical Research.
*Because the database did not provide Subject Areas for these members, the author has indicated the most likely areas based on the member's field of research in the mid-1960s.

Hasselbach, Wilhelm	1921–2015	Med. Forsch./ Physiol. (Heid.)	1964				1		
Hoffmann-Berling, Hartmut	1920–2011	Med. Forsch./ Physiol. (Heid.)	1964					1	
Schuster, Heinz	1927–1997	Mol. Gen. (Berlin-Dahlem)	1964						1
Wittmann, Heinz G.	1927–1990	Mol. Gen. (Berlin-Dahlem)	1964						1
Totals		(22 members)		5	4	1	3	2	7
Percentage distribution (%)				23	18	5	14	9	32

Abbreviations for EMBO Subject Areas in above table; % of total MPG members of EMBO, 1967

CTA/D/N:	Cell & Tissue Architecture|Development|Neuroscience	23%
PB/EE:	Proteins & Biochemistry|Evolution and Ecology	18%
D/N/SB:	Development|Neuroscience|Systems Biology	5%
CM/MT:	Cellular Metabolism|Membranes and Transport	14%
GSD/SBB:	Genome Stability and Dynamics|Structural Biology and Biophysics	9%
MM/ST/SBB:	Molecular Medicine|Signal Transduction|Structural Biology and Biophysics	32%

The Volkswagen Foundation was the primary sponsor of the EMBO's early efforts to promote education in molecular biology through lectureships, fellowships, etc.; but the EMBO also had the goal of establishing an international laboratory, the EMBL, which would require governmental support. A series of meetings at CERN in 1967–1968 attended by delegates of EMBO, UNESCO, and several European governments, including the FRG (represented by Manfred Eigen, among others), led to the formal establishment of the European Molecular Biology Conference (EMBC) in 1969 as a permanent intergovernmental body that could offer EMBO the necessary political support for its long-term projects. Fourteen governments joined the EMBC initially, providing the organization with stable funding and scientific independence.[278] In 1969, the MPG also opened formal ties to the leaders of the EMBO, appointing Kendrew and Perutz as External Scientific Members of the MPI for Biochemistry at the request of Walter Hoppe, representing the Martinsried Circle.[279] At the same time, Manfred Eigen became the most influential member in the EMBO when he was elected chair as the successor to Perutz.

278 Ferry, *EMBO*, 2014, 42–44; John Krige: The Birth of EMBO and the Difficult Road to EMBL. *Studies in History and Philosophy of Science Part C. Studies in History and Philosophy of Biological and Biomedical Sciences* 33/3 (2002), 547–564. Krige however does not address the decision to locate the EMBL in Germany, much less whether in Munich/Martinsried or Heidelberg. Minutes of the Second Session of the international meetings to organize EMBC (January 1968) are in EMBLA, Holmes Papers, DE 2324 P-HOL-B.

279 Eckart Henning and Marion Kazemi: *Handbuch zur Institutsgeschichte der Kaiser-Wilhelm-/Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1911–2011. Daten und Quellen.* Vol. 1. Berlin: Archiv der Max-Planck-Gesellschaft 2016, 190; Butenandt to Kendrew and Perutz, 17 March 1969, AMPG, II. Abt., Rep. 62, Nr. 733, fol. 12–15.

When first proposed, it was expected that the EMBL would be established in conjunction with CERN in Switzerland, but no formal offer came from that side. In order to clarify the situation and develop perspectives on the future tasks of an EMBL, the EMBO under Eigen convened a meeting in Konstanz in November 1969. In intensive, all-day discussions on November 27 with as many as ten current or future Nobel laureates participating, a possible research agenda for the EMBL emerged.[280] The EMBL project was now put in the hands of a laboratory planning committee, and by April 1970, it seemed likely that the EMBL would be located in the Federal Republic of Germany, as the Germans, especially the leading members from the MPG, were »very keen to have an international lab«, with the preference for Munich.[281] Between November 1969 and October 1970, the laboratory committee in EMBO developed its formal plans for EMBL. On October 3, *Nature* reported that the Federal Republic of Germany (presumably inspired by Butenandt, who had already served on the council of the EMBO) had just issued the first and only formal invitation to the EMBC to house the EMBL in Munich, in a place (presumably Martinsried, though not mentioned in this report) that would allow temporary accommodation and thus a head start to beginning its work in »new buildings which are presently under construction at the site.«[282] There was also much to recommend Martinsried. In the 1967 version of the EMBO's laboratory committee's proposal, research on the structure of proteins and other biological macromolecules was at the top of the list of five scientific priorities for an EMBL.[283] In developing the revised proposal following the Konstanz meeting, Walter Hoppe of the Martinsried Circle played a major role in the Working Group on Techniques and Instrumentation, highlighting the issues of structural analysis of proteins and synthesis of peptides using X-ray crystallography, sequencing, and other techniques, all of which were going to be significant subjects for research in Martinsried.[284]

Thus, the Martinsried Circle began to discuss the possible integration of the EMBL in 1970, specifically considering whether the projected EMBL might take over one wing of the new structure. By this time, at least one member had already redefined himself as a molecular biologist; in 1967, as seen in Table 3 above, Zillig had changed the title of his section at Martinsried from »biochemistry of gene expression« to »molecular biology of gene expression«, and others such as Hofschneider were focusing more closely on issues relevant to molecular biology (e. g. the biosynthesis of nucleic acids). As the laboratory sites committee of the EMBO prepared to visit sites in the Munich area and elsewhere in late October 1970 to evaluate their suitability for the EMBL, the momentum toward locating the EMBL near Munich, most likely in the Martinsried complex, seemed to be inescapable.

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280 Ferry, *EMBO*, 2014, 55–57.

281 Sydney Brenner, cited in Ferry, *EMBO*, 2014, 59.

282 No Home for EMBO Laboratory. *Nature* 228 (1970), 7. doi:https://doi.org/10.1038/228007a0.

283 EMBO Council: Proposal for a European Laboratory of Molecular Biology (November 1967), 9–10, EMBLA, Holmes Papers, DE 2324 P-HOL-B.

284 Revised Proposal for a European Laboratory of Molecular Biology (corrected draft, Feb. 16, 1970), Annex D, pp. 46–66, EMBLA, Holmes Papers, DE 2324 P-HOL-B.

And then everything changed. The EMBL would come to Germany, yes, but not to Munich or Martinsried, and not immediately. Instead, the laboratory opened in Heidelberg in 1978 as a result of a joint agreement among the founding nations in 1973.[285] The key role in stopping the momentum toward the Munich area came from an unexpected source, the MPI for Medical Research in Heidelberg. At the time of Kuhn's death in 1967 it was reorganizing and transitioning to a revised form of its original collegial structure.[286] In the process the MPI had established, besides sections for molecular physics (Hausser's EPR and NMR spectroscopy) and molecular biology (under Hoffmann-Berling), a guest research section for biophysics, to which it invited Dr. Kenneth Holmes of the Molecular Biology Lab at Cambridge University. Holmes was a key figure, because he was on the EMBO's sites committee. He had also been trained in John Kendrew's laboratory, and Kendrew trusted Holmes. Shortly before the sites committee tour in October 1970, Peter von Sengbusch, a young German scientist working in Holmes' lab, and Hermann Bujard, the newly appointed professor of molecular genetics at the university, both of whom had recently come back from postdocs in the United States, approached Holmes with the idea of bringing the EMBL to Heidelberg. They found Holmes to be receptive, so they put together a proposal to the EMBO, and Holmes then arranged for the sites committee to stop off in Heidelberg on its way to Munich. The classic university-town atmosphere of Heidelberg, its proximity to France (so that French scientists working at the EMBL could go back over the border for dinner), and the existence of Wolfgang Gentner's MPI for Nuclear Physics all helped to incline the committee more favorably to Heidelberg. Continuing on by Autobahn to Munich where they would be elegantly received by Butenandt and municipal dignitaries, the committee drove past the exit to Dachau northwest of the city, which (it was later said) resonated badly in at least one Jewish member's mind.[287] The sites committee could therefore argue, at least informally, that being in the vicinity of Dachau might make a Munich location »unacceptable for Jewish scientists«.[288] In its formal report, however, perhaps to avoid embarrassing its German hosts, the committee simply emphasized that »the general character of a region is of the greatest importance as a con-

285 Ferry, *EMBO*, 2014, 59–68.

286 By 1969 the MPI had five directors in the following sections and was having its facilities renovated and upgraded to meet their research priorities: physiology (Wilhelm Hasselbach), molecular physics [with strong emphasis on biochemical analysis using EPR and NMR spectroscopy, as noted earlier] (Karl H. Hausser, then managing director), molecular biology (Hartmut Hoffmann-Berling), organic chemistry (later: natural products chemistry) (Theodor Wieland), and biophysics [guest section] (Kenneth C. Holmes). Hans H. Weber continued as an additional scientific member, having retired as director of the old institute for physiology in 1966. Generalverwaltung der Max-Planck-Gesellschaft zur Förderung der Wissenschaften (ed.): Die Max-Planck-Gesellschaft zur Förderung der Wissenschaften e. V. im Jahre 1964. *Jahrbuch der Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1965*. Göttingen 1965, 7–20, 14; Generalverwaltung der Max-Planck-Gesellschaft (ed.): Die Max-Planck-Gesellschaft zur Förderung der Wissenschaften im Jahre 1968. *Jahrbuch der Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1969*. München 1969, 7–28, 19; Generalverwaltung der Max-Planck-Gesellschaft (ed.): Max-Planck-Institut für medizinische Forschung, Heidelberg. *Jahrbuch der Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1969*. München 1969, 351–358.

287 Personal communication from Anne-Flore Laloë, the EMBL archivist (15 April 2021). The member in question may have been the Hungarian-Swiss molecular biologist Charles Weissman (cf. Ferry, *EMBO*, 2014, 59).

288 See MPG Senat document in AMPG, II. Abt., Rep. 60, Nr. 71, fol. 159–160; I thank Alison Kraft for bringing this to my attention, as part of her current research on the EMBO and EMBL.

dition for attracting the best scientists«.[289] A bad impression in another sense arose from the practical difficulties posed by the great distance between the facilities in Martinsried and Garching, not yet connected by subway. One British molecular biologist on the committee emphasized the role of both scientific and non-scientific factors; the latter would affect the willingness of scientists to move their families to the new site, which thus must »be in an attractive environment«. As to the latter, connections to »neighbouring scientific institutions«, both physical and biological, were critical, but of the two, »perhaps a couple of miles separation from any really big local biology set-up might have certain advantages« – a point against moving into the MPI at Martinsried – whereas, »Neighbouring physics institutes are of much greater importance«, in that »the most useful collaboration would be in the development of sophisticated techniques and instrumentation«[290] – a point in favor of Garching, but not so much if the Germans insisted on putting the EMBL in Martinsried. These considerations only served to raise further doubts about Munich. When other German options such as moving near DESY in Hamburg (a site preferred by Holmes, who made frequent use of its facilities) proved to be unworkable, there were still more committee visits to both Munich and Heidelberg, more efforts at persuasion by the scientists in Heidelberg, and more internal discussions among the EMBO leaders and committee members, before the EMBO formally voted in favor of Heidelberg at the end of April 1971.[291] In the meantime, they confronted the fact that the education ministers of all of the German states had agreed that the EMBO should go to Bavaria. Kendrew contacted the German delegation to EMBC to inquire about the possibility of changing the invitation of the Federal Ministry of Education and Science from Munich to Heidelberg. The executive secretary of EMBO, Raymond Appleyard, then met with an official in the ministry he knew well – and the only one he knew who was not a Bavarian – to draft a revised invitation in favor of Heidelberg. The German delegation thereupon formally offered to the EMBO a grant of ten million DM toward the construction of an EMBL in Heidelberg in June 1971, which led in October 1971 to a unanimous vote by the delegations of the EMBC in favor of Heidelberg, a decision that would be ratified by the member governments in 1972.[292]

Thus, in one year, the apparently inevitable prospect of an EMBL in Martinsried had vanished. Yet the episode had important consequences for the future of the MPI, because it gave Martinsried Circle members, and particularly Walter Hoppe who had been involved in planning the

289 European Molecular Biology Organisation: Report on a Selection of Sites in Germany prepared by a working party of the EMBO Sites Committee, 22 Feb. 1971, EMBLA, Holmes Papers, DE 2324 P-HOL-C. None of the other correspondence or documents in the Holmes Papers appears to contain any reference to Dachau.

290 Hugh Huxley to M. G. N. Hine, 31 Oct. 1970, EMBLA, Holmes Papers, DE 2324 P-HOL-C.

291 Ferry, *EMBO*, 2014, 62. A critical voice for Heidelberg on the non-scientific side was an Englishwoman who had lived there for 30 years and loved the town. Holmes asked her to meet the committee »and tell them a little about life in Heidelberg.« Holmes to Mrs. Fischer, 29 March 1971, in EMBLA, DE 2324 P-HOL-C; according to one EMBO delegate, when Fischer spoke, »the whole atmosphere changed.« (Mary Holmes, Why EMBL came to Heidelberg [lecture typescript, no date, copy in EMBLA]).

292 Ferry, *EMBO*, 2014, 62; Heidelberg wird Zentrum für Biologie-Forschung, *Heidelberger Lokalnachrichten*, 29 Oct. 1971, p.15; EMBO kommt nach Heidelberg, *Tagblatt*, 15 Oct. 1971; both clippings in EMBLA, Holmes Papers, DE 2324 P-HOL-C.

EMBL, a greater awareness of current international trends in molecular biology. This in turn probably added to their sense of urgency in re-orienting their research toward molecular biology, and it influenced their efforts to recruit at least one additional member from abroad, Pehr Edman, as will be seen in the next section.

4.11 Adding internationalism and youth to the scientific staff at Martinsried in the early 1970s

The diversity of fields to be brought together in the new center is reflected in the list of fields of the first scientific members who were to be part of the biochemical center when it opened in 1973 (Table 6):

TABLE 6

Principal research interests of the section heads at the opening of the Martinsried MPI, 1973 (letters indicate building locations of sections).

Laboratory Building 1 (A / B / C / D = east / south / west / north wings)

(A) Molecular biology of gene expression (Genwirkungen) (focusing on transcription) (Wolfram Zillig)
(B) Virus research (biosynthesis of nucleic acids and proteins) (Peter Hans Hofschneider)
(C) Enzyme chemistry and metabolism (Feodor Lynen)
(D) Organic chemistry and spectroscopy (study of a variety of biological compounds including hormones, pheromones, and chromosomes; mass spectrometry) (Heinz Dannenberg)

Laboratory Building 2 (E, F / G, H / I, K / L, M = east / south / west / north wings, lower 2 and upper 2 floors respectively)

(E) Connective tissue research (biochemical analysis of components including collagen, fibrin, and fibrinogen) (Klaus Kühn)
(F/1) Protein chemistry I (developing a device for automated sequencing of proteins) (Pehr Edman)
(F/2) Protein chemistry II (featuring hemoglobin and respiration chemistry) (Gerhard Braunitzer)
(G) Biochemical methods (in particular, developing processes for separating biological substances by electrophoresis) (Kurt Hannig)
(H) Experimental medicine (on the level of cells and tissues) (Gerhard Ruhenstroth-Bauer)
(I, K) Structural research I (x-ray crystallography and electron microscopy of biologically significant organic compounds) (Walter Hoppe)
(K, I) Structural research II (x-ray crystallography of biological macromolecules) (Robert Huber)
(L, M) To be assigned later [from 1976: Peptide chemistry (Erich Wünsch)][293]

293 In 1974 the MPI established two groups of junior or early-career scientists (Nachwuchsgruppen), including students and postdocs (some from abroad), which were assigned to sections A and C (respectively, chromosome research under Ulrich Großbach and biochemistry of intercellular communication in the nervous system under Bernd Hamprecht). By 1976 there was a third junior scientists' group, physical biochemistry, in sections H and K under Eberhard Neumann. Max-Planck-Gesellschaft zur Förderung der Wissenschaften (ed.): *Verzeichnis der Organe und Institute. Stand vom 1. Juni 1974*. Göttingen 1974, 15. *Max-Planck-Institut für Biochemie. Forschungsgebiete und Struktur.* 2nd ed. Munich: Max-Planck-Institut für Biochemie 1976, 95–96, 117.

With the exception of Edman, Huber, and Wünsch, all of these scientists were part of the Martinsried Circle as formed in 1966. Butenandt and Grassmann were also members but increasingly withdrew from active participation, as they would not be actually working in the new center. The Circle needed a chairperson, of course, and immediately elected Lynen, a natural leadership role given his recent Nobel Prize. Once the MPI in Martinsried was completed, Lynen became its first managing director, supported from 1974 by two others as associate directors, serving in two-year rotations. Lynen was influential but not dominant, as reflected in the discussions over one of the potential members of the new center, Erich Wünsch, then leader of Grassmann's peptide chemistry group at the MPI for Protein and Leather Research. Initially many of the younger members of the circle argued that Wünsch should be brought into the new center, given the central importance of peptide chemistry in the study of proteins (peptide bonds form chains of amino acids, and some of the earliest scientists attempting protein synthesis, such as Emil Fischer, began by synthesizing polypeptides; some proteins are assemblies of such chains of polypeptides). Lynen, however, objected, arguing that Wünsch did not have the scientific stature needed to be director of an autonomous section, and he therefore insisted on excluding peptide chemistry. This was related to the problem of finding space and a corresponding budget in the new center for Wünsch's group. The debate over the future of the peptide section went on for several years before Lynen conceded. To better judge the international scientific status of Wünsch, the MPG sought the recommendations of several outside referees, including such luminaries as Robert Bruce Merrifield of the Rockefeller University. Merrifield's judgment carried particular weight because Wünsch had been highly critical of the limitations of Merrifield's pathbreaking solid-phase peptide synthesis, developed in the early 1960s. But Merrifield was not at all vindictive; much to the surprise of the Germans, he designated Wünsch one of the top four or five European peptide chemists, considering that Wünsch's total synthesis of glucagon announced in 1968 was the »ultimate in peptide synthesis against which all other work must be measured.«[294] On the basis of such evaluations, Lynen finally agreed in February 1973, shortly before the official opening of the Martinsried center, that Wünsch had matured enough scientifically to be selected as a scientific member of the MPG and thus of the new biochemical center. Space limitations, however, forced the compromise that Wünsch's working group (Arbeitsgruppe) would initially remain in Grassmann's former institute in Munich, while the rest moved as planned to Martinsried.[295] In 1976, however, Wünsch did join the others in Martinsried.

Lynen's opposition was related to the problem of how to adapt traditions developed in Dahlem, in particular the Harnack Principle, to the new, larger scale and collegial organization of the biochemical center in Martinsried. The idea of organizing institutes around creative individuals rather than arbitrarily chosen disciplines or subfields could no longer apply in a building

294 Bruce Merrifield to Otto Westphal, 25 April 1972, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 56–57. Cf. Stephen Kent: Bruce Merrifield (1921–2006). *Nature* 441 (2006), 824. doi:https://doi.org/10.1038/441824a.

295 Kommission: Zukunft der Arbeitsgruppe E. Wünsch, 3. Sitzung der Kommission am 7.2.73, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 13.

that was collectively designed and collegially managed, like Martinsried. As already discussed, this policy would need modification in the larger research centers the MPG was creating in the 1960s.[296]

The extensive institute complex when completed had a large total staff, numbering 560 as of March 1977 (up from Meitinger's estimate of a maximum staff of 500, including 150 academics, in 1973).[297] There were then 214 academic scientists (96 in salaried positions; 33 paid from »soft money«, in the American sense; 75 *Diplom* or doctoral students and postdoctoral fellows; and 10 foreign guest scientists). The rest included an almost equal number of technical staff, plus administrative personnel supplemented by ordinary workers.[298] The importance of having spaces for foreign guests and younger scientists (Nachwuchswissenschaftler) had been part of the discussion from the very beginning of the planning process; indeed, the lack of space for these people had been one of the justifications for moving the biochemical institutes from Munich in the first place.

As noted, Huber was one of the new section leaders who had not been part of the Circle in 1966. Robert Huber (1937–) was a rising star on the faculty of the Technical University of Munich and very well-known to the members of the Martinsried Circle. Born and raised in the city, he had attended school there and then took his *Diplom* in chemistry at the TU in 1960. Under the guidance of Walter Hoppe in his lab at the MPI for Protein and Leather Research, Huber did his *Diplom* research project on the crystallographic structural analysis of the insect metamorphosis hormone ecdysone that had been Peter Karlson's special interest. Thus Huber also did some work in Karlson's laboratory in Butenandt's university institute and published the results in collaboration with Hoppe, Karlson, and others in their laboratories. He continued his structural analysis for his doctorate at the TU under Hoppe's guidance, after which he maintained his affiliation with the TU as an instructor. His next research project, beginning in 1967 with Hoppe's support as well as Braunitzer's from the MPI for Biochemistry, applied crystallographic structural analysis to the insect protein erythrocruorin. The inter-institutional collaborations (LMU-TU-MPI) seen in Huber's training and early career provide additional evidence for the synergies arising from the proximity of institutes in the group designed by Butenandt and Grassmann in central Munich, which the InfraTest survey had documented and the MPG's planners were trying to carry over in an even stronger form to Martinsried (Figs. 21 and 22 above). Given this experience, it was hardly a surprise that the Martinsried Circle nominated Huber in 1971 to head a second section for structural research in addition to Hoppe's. At the same time, Huber, like Butenandt two decades earlier, was offered a position at the University of Basel, and, like Butenandt, he chose to stay with the MPI for Biochemistry.[299]

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296 Trischler, Innovationssystem, 2004, 117–194, 191; Osterwalder, Einführung, 1968, v–viii, vi–vii.

297 Meitinger, Die Neubauten, 1973, 1251–1264, 1260.

298 Max-Planck-Gesellschaft (ed.), *Max-Planck-Institut für Biochemie*, 1977, 79.

299 The Nobel Prize: Robert Huber. Biographical, 1988. https://www.nobelprize.org/prizes/chemistry/1988/huber/biographical/. Last accessed 2/20/2023; Academic Tree (Chemistry): Walter Hoppe – Publications. https://academictree.org/chemistry/publications.php?pid=52143. Last accessed 5/9/2023.

4.12 Internationalism and international collaboration/competition in Martinsried

As noted earlier, despite the evidence of Nazi atrocities partly committed in the name of scientific research, the period after the Second World War did not see an official boycott similar to that following the First World War. Nevertheless, neither German nor Japanese delegates were invited to the first International Congress of Biochemistry at Cambridge, UK, in 1949, and even later there were occasional protests against individual scientists who had engaged in inhumane research under the Nazis.[300] In any case, as Butenandt, Kuhn, and other German scientific leaders had realized, German scientists needed to systematically rebuild relations with scientists in formerly hostile nations. This was a continuing challenge even in the late 1960s, when the Martinsried Circle was planning the MPI for Biochemistry in Martinsried, and indeed it would continue for decades afterwards.[301] The Martinsried Circle evidently agreed with the need to raise their new institute's international profile. One means to this end was to invite international panels of scholars to judge their potential appointments. This was the case with the appointment of Wünsch, noted above; the commission making the recommendation in his case called upon ten distinguished referees, only two of whom were Germans; four were Anglo-American, one Swiss, one Israeli, one Swedish, and one Italian.[302] This was hardly new; the MPG's Scientific Council had followed the same approach with the appointment of the younger members of the Martinsried Circle to their MPIs in Munich prior to the advent of the Martinsried project.[303] Another approach was to appoint foreign scientists as external (or corresponding) scientific members of the institute. This was done with Dean Burk of the NIH in Bethesda in 1972, for example. Burk had previously been a corresponding member of Otto Warburg's institute in Dahlem. After that institute closed in 1972 following Warburg's death and a brief period with Lynen as interim director, the MPI for Biochemistry then offered Burk a similar position, as corresponding member.

It was felt that it would be even better would be to appoint a foreign scientist to their new institute. With one major exception, all of the scientific members of the Martinsried center when it opened in 1973 were Germans who had already been associated with one of the three component institutes (Wünsch, a Sudeten German, was a partial exception). The major exception was

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300 Harmke Kamminga: Internationalizing Biochemistry: The 1949 Congress in Context. *Trends in Biochemical Sciences* 24/10 (1999), 404–408, 404, 406. On the Dutch protests and threatened boycott against the participation of Julius Hallervorden (MPI for Brain Research) in the 5th International Neurology Conference in Lisbon (1953), see Sascha Topp: *Geschichte als Argument in der Nachkriegsmedizin. Formen der Vergegenwärtigung der nationalsozialistischen Euthanasie zwischen Politisierung und Historiographie*. Göttingen: V&R unipress 2013, 247–260; Lawrence A. Zeidman: *Brain Science under the Swastika. Ethical Violations, Resistance, and Victimization of Neuroscientists in Nazi Europe*. Oxford: Oxford University Press 2020, 669–689.

301 Cf. Wissenschaftsrat: *Empfehlungen zur Internationalisierung der Wissenschaftsbeziehungen*. Köln 1992.

302 Kommission: Zukunft der Arbeitsgruppe E. Wünsch, 3. Sitzung der Kommission am 7.2.73, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 13. Notably, the predominance of non-German referees necessitated that Wünsch's vita be translated into English, cf. message by O. Westphal, 17 April 1972, ibid., fol. 83.

303 Documents in AMPG, II. Abt., Rep. 62, Nr. 733, e. g. recommendations for Braunitzer, Hofschneider, and Zillig as scientific members of their MPIs, 1964–66.

Pehr Edman, a Swedish biochemist then working in Australia, who had specialized in developing the first protein sequenator, a device that automated the previously time-consuming and tedious work of »sequencing« or identifying and determining the order of amino-acid components in a protein chain). It was a significant advance over the purely manual sequencing technique first developed by Frederick Sanger in the UK between 1945 and 1951, which led to Sanger's being awarded the Nobel Prize in Chemistry in 1958.[304] Edman's appointment reflected the already-emerging process that would steadily shift the Martinsried center toward the modern techniques and approaches of molecular biology, and from him, the Martinsried Circle hoped to learn some of these techniques. The latter hope was realized, at least for protein sequencing; Edman's device involved a process known as »Edman degradation«, which has remained a common approach to protein sequencing and is still used by TopLab, one of the spin-off companies that developed in the Martinsried area.[305]

Nevertheless, the Circle wanted more from Edman. In view of the significance of nucleic acids in molecular biology, they hoped that Edman would also work on techniques for sequencing nucleic acids, a problem that had not yet found a satisfactory solution, and they also wanted to know how likely he would be to work collaboratively with others in Martinsried. The Circle then invited their international panel of five expert referees to comment on these possibilities in Edman's case. They responded either by not commenting on these points, or they judged the possibilities negatively.[306] In view of these rather pessimistic views, Lynen specifically asked Edman to comment on his future plans, especially his intentions to work in sequencing nucleic acids, as well as on his interest in collaborating with other members of the Martinsried center.[307] Edman's response on both points was noncommittal, suggesting that both might happen, but without being especially enthusiastic about either.[308] Heinz Schuster, a director of the MPI for Molecular Genetics in Dahlem and member of the appointment commission, concluded from the referees' comments and from discussions with members of the Martinsried Circle that Edman would not be a suitable member of the new institute, and he abstained on the final vote to hire Edman.[309] But he was unable to convince his colleagues, and the Circle voted with no

304 Frederick Sanger: Sequences, Sequences, and Sequences. *Annual Reviews of Biochemistry* 57 (1988), 1–28, 4–10; Frederick Sanger: The Chemistry of Insulin. Nobel Lecture, December 11, 1958. *Nobel Lectures, Chemistry 1942–1962.* Amsterdam: Elsevier 1964; Joe S. Jeffers: *Frederick Sanger. Two-Time Nobel Laureate in Chemistry.* Cham: Springer 2017. doi:10.1007/978-3-319-54709-1; Pehr Edman and Geoffrey Begg: A Protein Sequenator. *European Journal of Biochemistry* 1/1 (1967), 80–91. doi:10.1111/j.1432-1033.1967.tb00047.x; S. Miles Partridge and Birger Blombäck: Pehr Victor Edman. 14 April 1916–19 March 1977. *Biographical Memoirs of Fellows of the Royal Society* 25 (1979), 241–265.

305 TopLab: N-Terminal Edman Sequencing. http://www.toplab.de/analytical-methods/edman-sequencing/. Last accessed 3/17/2023.

306 Two examples: Frank W. Putnam (Prof. of Molecular Biology, Indiana Univ.) to Otto Westphal, 27 Aug. 1971; Frederick Sanger to Otto Westphal, 13 Aug. 1971, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 171–173.

307 Lynen to Edman, 5 Nov. 1971, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 153–154.

308 Edman to Lynen, 9 Dec. 1971, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 155–157.

309 H. Schuster to the Berufungskommission, 21 Jan. 1971 [sic: 1972]: Begründung meiner Stimmenthaltung zur Empfehlung der Kommission, Herrn Edman zu berufen, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 149–150.

other abstentions or negative votes to offer a position as scientific member to Edman; this was confirmed by the MPG's Senate, and Edman accepted the offer as effective in 1973.[310]

As it turned out, Edman never worked on the sequencing of nucleic acids; instead, he focused mainly on the central problem of perfecting his protein sequenator, and on the related question of developing techniques for storage, processing, and retrieval of the data derived from the sequenator. Hence, he was a pioneer in the use of computing in biochemistry. His principal collaborator was not anyone in the Martinsried Circle, however, but his wife, Agnes Henschen-Edman, herself an excellent biochemist. It was she who mainly worked with other members of his section on elucidating the structure of the blood protein fibrinogen.[311] After Edman's untimely death in March 1977 at the age of 60, Gerhard Braunitzer became interim director of his section before it was shut down in 1979. It was also in 1977 that Frederick Sanger, working with a team of technicians and biochemists at the Medical Research Council laboratory in Cambridge, developed the »dideoxy« or »Sanger Method« for DNA sequencing – essentially the breakthrough that the Circle had vainly hoped Edman would pursue. The new method made possible an exponential increase in the length of DNA samples that could be sequenced, from around a hundred nucleotide units in the 1960s to five thousand in 1978, to around fifty thousand in 1982, with further increases to come soon after that. In 1980, this earned Sanger a share in his second Nobel Prize in Chemistry.[312]

Edman's death and the closure of his section did not put an end to efforts by the Martinsried biochemists in pursuit of advanced technological devices in their fields, but scientists elsewhere took over leadership in the field of automated sequencing and synthesis of proteins and nucleic acids. At the end of the 1970s, the protein chemist Leroy Hood's research group at Caltech in Pasadena, California (Max Delbrück's institution until his own death in 1981), undertook a coordinated effort to develop devices in all four of these areas. Hood's group had its first success in 1980–1981 with an improved protein sequenator, which was 100 times more sensitive than previous devices such as Edman's and thus allowed work with much smaller samples of rare proteins. At the same time they also began to automate Sanger's methods, producing a workable prototype device by 1985–1986 that significantly accelerated the sequencing of nucleic acids and opened a practical way toward the human genome project, which became possible

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310 Zu Punkt 3. B. a. der Tagesordnung: »Berufung von Dr. Pehr EDMAN Melbourne ...«; Butenandt to Edman, 25 Jan. 1972, AMPG, II. Abt., Rep. 62, Nr. 732, fol. 147.

311 Partridge and Blombäck, Pehr Victor Edman, 1979, 241–265, 257–258. Henschen-Edman had five collaborators on her fibrinogen research in 1976, whereas Edman had only two for his Sequenator project. *Max-Planck-Institut* für *Biochemie. Forschungsgebiete*, 1976, 64. After 1979 she worked in Braunitzer's section until moving to the USA around 1987.

312 Sanger, Sequences, 1988, 1–28, 21–26. Sanger recalls here that in 1969–70 he collaborated with Hans Kössel of Freiburg, who had worked with Gobind Khorana at the University of Wisconsin and there learned to synthesize a crucial polynucleotide needed at an early stage of Sanger's project. These techniques were still very cumbersome, slow, and difficult compared to the more automated methods that developed by the 1980s and beyond. Notably, Kössel was a former student of Butenandt and assistant of Zillig at the MPI in Munich. Cf. Peter Sitte: Obituary: Hans Kössel (1934–1995). In: Hainfried E. A. Schenk et al. (eds.): *Eukaryotism and Symbiosis*. Berlin: Springer 1997, 119–121. doi:10.1007/978-3-642-60885-8_8.

after 1989 with the creation of »the first robust DNA sequencer that could be used routinely in most laboratories.« That, however, required significant investments and a constant focus on technological development that was only possible in a commercial setting. It is worth noting that even in the technology-friendly US, they encountered initial resistance. Reviewers of their applications for NIH support in the early 1980s responded with shortsighted comments dismissing automated methods as »impossible« or claiming, »Graduate students could do it less expensively.« Hood was also rejected by all nineteen instrument and device companies that he approached in his first attempt at commercial development of his protein sequenator. Ultimately, he and his associates partnered with a venture capitalist to found Applied Biosystems (ABI), which became the most successful producer of automated biotechnological sequencing and synthesizing devices during the 1980s and 1990s.[313] There appears to have been nothing comparable to this in Germany at the time.

Another area in which there was trans-Atlantic competition was in peptide, protein, and nucleic acid synthesis, aiming at the goal set for a chemical biology by Emil Fischer in 1915. As noted earlier, Erich Wünsch had achieved fame as a peptide chemist with the total synthesis of glucagon in 1967–1968, which was clearly a step toward Fischer's goal. But the synthesis, like most syntheses of complex biological molecules at that time, required long hours of painstaking, precise organic-chemical work. Bruce Merrifield of the Rockefeller University was meanwhile taking a different approach with the solid-phase peptide synthesis he had developed in the early 1960s. Working with John Stewart, a Rockefeller collaborator, he devised a programmable machine that would automate the steps in the synthesis. »With the new machine I could realistically think about the synthesis of a protein,« Merrifield later recalled, taking up this goal with explicit reference to Fischer as his inspiration.[314] By 1969, Merrifield's machine could synthesize a protein of known sequence such as the ribonuclease A enzyme, with a fairly limited length of 124 amino-acid units. Nevertheless, as Wünsch and others argued in criticism of Merrifield's methods, even after considerable purification, the machine-produced synthetic enzyme exhibited a much more limited biological activity than the natural molecule. To overcome these flaws, Merrifield and his colleagues, along with teams in several other institutions, worked successfully to improve purification and related techniques, in the belief that an automated approach was the only way to accomplish the practical synthesis of large, biologically active molecules, which could open the path to modern biotechnology. In 1970 they had already achieved an activity level of 78% for one preparation of synthetic ribonuclease A.[315] In recogni-

313 Leroy E. Hood: A Personal View of Molecular Technology and How It Has Changed Biology. *Journal of Proteome Research* 1/5 (2002), 399–409, 401.

314 Bruce Merrifield: Recollections. The Chemical Synthesis of Proteins. *Protein Science* 5/9 (1996), 1947–1951, 1948.

315 Bernd Gutte and Bruce Merrifield: The Synthesis of Ribonuclease A. *Journal of Biological Chemistry* 246/6 (1971), 1922–1941, 1929 (Table III). doi:10.1016/S0021-9258(18)62396-8. Gutte, a German postdoctoral fellow and assistant professor at the Rockefeller University, worked with Merrifield in 1969–1971, later moving to the Institute for Genetics in Cologne and then the MPI for Biophysical Chemistry in Göttingen before taking a professorship in Zurich in 1980. University of Zurich: Prof. Emeritus Bernd Gutte. Universität Zürich 2022. http://www.bioc.uzh.ch/en/research/research-groups/former-groups/gutte/cv.html. Last accessed 11/20/2022.

tion of the successful and broad applications of his method for protein synthesis, Merrifield received the rare honor of an unshared Nobel Prize in Chemistry in 1984. At that time, he admitted in his Nobel lecture that the results he had so far obtained were »encouraging, but much more needs to be done to assure that even small proteins can be synthesized readily in high yield and purity.«[316] Looking back a decade later at the rapid progress that was being made in the field with the synthesis of many proteins of around one hundred amino-acid units and the potential for much larger synthetic molecules, and noting that improved methods of purification had finally made it possible to achieve 100% biological activity in synthetic ribonuclease A, Merrifield concluded with a final nod to Fischer. »I do not think he would be surprised, but would be enormously pleased.«[317] Nevertheless, given Wünsch's skepticism, Merrified's automated approach to synthesis was not introduced at Martinsried during the era under discussion in this paper.

4.13 The modified Harnack Principle at work: Transitions in staff and sections, 1975–1991

Back in Martinsried, the closure of Edman's section reflected the continuing operation of the Harnack Principle within the larger structure of the institute. This became standard practice: Dannenberg had already died in 1975; Ruhenstroth-Bauer took over as interim director after Dannenberg before the section was shut down in 1979, when Dieter Oesterhelt of Würzburg was appointed as director of a new section for membrane biochemistry. Following Lynen's retirement and death in 1979, his section too was closed. In 1980, Heinz Ludwig Sänger moved from Giessen to direct a new section for plant virology research. Following Ruhenstroth-Bauer's retirement in 1981, his section for experimental medicine was also closed at the end of 1982.[318] In that year, in view of Hoppe's forthcoming retirement in 1985, part of his section for structural research was allocated to a new working group (Arbeitsgruppe) in molecular structural biology under Wolfgang Baumeister, whose expertise in high-resolution electron microscopy complemented the Hoppe group's x-ray crystallography. The remainder of Hoppe's section was closed when he retired, but Baumeister became a scientific member and his group became a regular section in 1986.[319]

Thus, the years 1979–1982 brought the first major reshaping of the MPI for Biochemistry since its opening years in 1972–74. The next major change occurred in 1989, near the end of the period under consideration, with the opening of Axel Ullrich's section for molecular biology. The

316 Bruce Merrifield: Solid Phase Synthesis. Nobel Lecture, 8 December 1984. In: Tore Frängsmyr and Bo G. Malmström (eds.): *Nobel Lectures, Chemistry 1981–1990*. Singapore: World Scientific Publishing 1992, 149–175, 173, 149–175, 173. https://www.nobelprize.org/uploads/2018/06/merrifield-lecture.pdf. Last accessed 17 March 2023.

317 Merrifield, Recollections, 1996, 1947–1951, 1951.

318 Henning and Kazemi, *Handbuch*, Vol. 1, 2016, 211–213.

319 Cf. discussions in AMPG, II. Abt., Rep. 62, Nr. 737, fol. 88–211.

appointments of Baumeister and Ullrich both continued the transition in Martinsried toward a greater emphasis on molecular biology. In the case of Ullrich, who had received his doctorate in Heidelberg but then did postdoctoral research at the University of California in San Francisco before taking a position on the research staff of Genentech in San Francisco in 1979, the appointment also represented a move toward genetic engineering (following up on the Gene Center that opened in 1984, as discussed below) and a reversal of the German-American brain drain. This also involved a narrowing of focus in the institute, however, necessitated by the MPG's limited resources; not every scientific member at the MPI was replaced, so that by 1990 the number of regular sections had fallen to ten, from the original twelve. In particular, Hannig's research on biochemical methods had come to an effective end, so that his section could close on his retirement, and Braunitzer's protein sequencing, while valuable to the institute, could be largely automated within a working group and thus no longer appeared to require the resources of a full section. The space freed up was used by groups of junior scientists (Nachwuchsgruppen), including those of the Gene Center opened in 1984, as discussed below and working groups led by scientists who were not yet members of the institute, as well as (from 1977 to 1983) a section for neurochemistry of the Theoretical Institute of the MPI for Psychiatry.[320] Overall, the changes in scientific members may be summarized in Table 7 below. It should be noted that the MPI during this period was administered by a managing director and two associate managing directors, elected by the collegium of scientific members to two-year terms.

320 In accordance with a decision made in 1979, from 1981 to 1984 the MPG built and opened a new building for the Theoretical Institute of the MPI for Psychiatry on the site hitherto reserved for the third laboratory building of the MPI for Biochemistry. After moving into its new building, the Theoretical Institute then transferred the northern (L, M) wing of the larger laboratoratory building back to the MPI for Biochemistry to be used for Nachwuchsgruppen of the Gene Center, which the Theoretical Institute co-hosted with the MPI for Biochemistry. Eckart Henning and Marion Kazemi: *Handbuch zur Institutsgeschichte der Kaiser-Wilhelm-/Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1911–2011. Daten und Quellen.* Vol. 2. Berlin: Archiv der Max-Planck-Gesellschaft 2016, 1386–1388. On closing Hannig and Braunitzer's sections: Ergebnissprotokoll der Sitzung der Kommission »Max-Planck-Institut für Biochemie« am 13. 3. 1986 im Institut in Martinsried, AMPG, II. Abt., Rep. 62, Nr. 737, fol. 63–71. On Axel Ullrich (b. 1943) see Leopoldina. Nationale Akademie der Wissenschaften: Curriculum Vitae Prof. Dr. Axel Ullrich. https://www.leopoldina.org/fileadmin/redaktion/Mitglieder/CV_Ullrich_Axel_D.pdf. Last accessed 3/17/2023. During the appointment process, several American referees pointed out Ullrich's significant contributions to Genentech's industrial research, and one in particular ascribed the academic nature and successes of Genentech's research program to Ullrich's influence. Philip Leder to H. G. Wittmann, 3 April 1986, AMPG, II. Abt., Rep. 62, Nr. 737, fol. 58.

TABLE 7

Scientific Members and Fields at the MPI for Biochemistry, Martinsried, 1973–1990.[321]

Member's Name	Area of research	Comments on disposition of section
Founding Generation (1973)		
a) Died or retired in first decade, to 1982		
H. Dannenberg	Organic chemistry, spectroscopy	(d. 13. 2. 1975, section under Ruhenstroth-Bauer to 1979, then closed)
P. Edman	Protein chemistry I	(d. 19. 3. 1977, section under Braunitzer to 1979, then merged into his section)
F. Lynen	Enzyme chemistry, metabolism	(ret. 30. 4. 1979, section closed)
G. Ruhenstroth-Bauer	Experimental medicine	(ret. 30. 6. 1981; section closed end 1982)
b) Died or retired in 1980s		
W. Hoppe	Structural research I	(ret. 31. 3. 1985; section closed)
K. Hannig	Biochemical methods	(ret. 1988, section closed)
G. Braunitzer	Protein chemistry II	(d. 27. 5. 1989; section closed; reopened under R. Timpl from 13. 3. 1992)
c) Continued until 1990s or beyond		
W. Zillig	Molecular biology of gene expression	(ret. 1993, section under Baumeister until closed 1996)
R. Huber	Structural research II	(ret. 2005)
P. H. Hofschneider	Virus research	(ret. 1997)
E. Wünsch	Peptide chemistry	(in Munich until 1976, then Martinsried until ret. 1991; section closed)
K. Kühn	Connective tissue research	(ret. 1995)
Second Generation (appointed 1979–1988)		
G. Gerisch	Cell biology	(April 1979–1999)
H. L. Sänger	Viroid Research (plant virology)	(opened 1. 9. 1980 with greenhouse)
D. Oesterhelt	Membrane biochemistry	(est. 1. 5. 1979, full operation Feb. 1980)
A. Ullrich	Molecular biology	(appt. WM Nov. 1986, sect. open 1989)
W. Baumeister	Molecular structural biology	(Arbeitsgruppe 1982; WM 9. 6. 1988)

321 Compiled from Henning and Kazemi, *Handbuch*, Vol. 1, 2016, 188–189, 209–214; cross-checked with the Jahrbücher of the MPG, 1973–1991.

4.14 Enhancing MPI-university relations. The Gene Center

In old Dahlem (and in Heidelberg), the KWG had initially met with some hostility from the universities, some of whose professors resented the break with the classic German scholarly tradition of the unity of teaching and research. The KWG's leaders had sought to bridge the institutional divides by having their directors and members appointed at least as honorary professors in the universities, but this had not always been successful, and under Hahn the MPG's policy opposed Butenandt's idea of having a director of an MPI simultaneously hold a university professorship. The idea was that a member of a research institute should primarily do research, not teach. Nevertheless, until he retired from his university professorship to assume the presidency of the MPG, Butenandt had regularly lectured in the medical faculties of Tübingen and Munich, justifying this with the argument that it was a temporary expedient to assist the recovery of those faculties' prestige in the aftermath of National Socialism and war, and that Butenandt would withdraw from teaching once his participation no longer seemed required.[322] Of course he had designed his institutes in Munich in such close conjunction that it was extremely difficult for him simply to give up the teaching institute while continuing to direct the MPI, aside from the time lost to his presidential duties; this probably contributed to the decision to move to Martinsried.

In Martinsried, there was initially no university institute on-site, but many of the scientific members continued to hold professorial positions and engage in teaching research, initially to postdocs (often international) and LMU or TU students carried over from their university teaching institutes. As noted earlier, by 1976 the MPI had further institutionalized the teaching connection by creating three junior scientists' groups (Nachwuchsgruppen). A new stage in the process began when the biochemist Ernst-Ludwig Winnacker, who had had extensive foreign training (in Zurich, Berkeley, and Sweden) moved from Cologne to Munich as professor of biochemistry at the LMU in 1977.[323] In the early 1980s, Winnacker proposed creating a Gene Center as a collaborative effort between the LMU and the MPG. It would eventually have its own building, but initially it would be housed in a wing of the MPI for Biochemistry, with regular research laboratories on an upper floor and three Nachwuchsgruppen below. Winnacker expected to obtain support primarily from the Federal Ministry for Research (as part of a broader program of fostering centers of excellence in biotechnology) and the Bavarian government, supplemented by industrial support through a funding association of the type that had once supported the KWI for Chemistry in Dahlem.

To this end, Winnacker (who was to head the industrial funding association) hoped to enlist the major pharmaceutical and organic-chemicals firms, and he also persuaded the MPG presi-

322 Cf. Hahn to Butenandt, 3 Nov. 1953, AMPG, III. Abt., Rep. 84/1, Nr. 689, fol. 94–95; annual Vorlesungsverzeichnisse show Butenandt lecturing in collaboration with a couple of associates from his institute.

323 On Winnacker see Human Frontier Science Program: Profile of Secretary General Ernst-Ludwig Winnacker. https://web.archive.org/web/20120317065548/http://www.hfsp.org/about-us/governance/secretariat/secretary-general. Last accessed 11/30/2011.

dent, Reimar Lüst, to contact such firms directly, except for the Hoechst AG, for which Winnacker took responsibility as his father had been chairman of the board of directors until 1969. It appears that Hoechst AG did agree to contribute one million DM per year for the first five years, but other firms seem to have been reluctant to provide substantial amounts, if any. Thus, the bulk of the initial seven million DM budget for the Gene Center came from the German federal government. Although the MPG was reluctant to supply its own funds for this project, it did agree to provide the necessary space and apparatus in the L, M wing of the MPI in return for rental fees (200,000 DM per year) paid by the university. In response to Winnacker's urgent request to the MPG for additional financial support to allow the center to expand its activities from 1988, the MPG agreed to supplement the center's apparatus budget by 200,000 DM, but only for two years. The Gene Center remained in the MPI for a decade (1984–1994), until its own building was completed at the LMU Großhadern campus. This arrangement offered opportunities for collaboration in teaching and research between scientists of the MPI for Biochemistry and the Theoretical Institute of the MPI for Psychiatry, housed in a new building at Martinsried and opened in 1984 just prior to the opening of the Gene Center, and the LMU. As coordinator of the MPI's Nachwuchsgruppen, Dieter Oesterhelt played a leading role with Winnacker, and his position also offered Oesterhelt opportunities to pursue interdisciplinary research on the interaction of light with protein-pigment complexes, opening a variety of fascinating scientific and technical possibilities.[324] In principle the Gene Center also offered opportunities for firms of the chemical and pharmaceutical industry to give their scientific staff advanced training in new genetic engineering techniques, as well as to recruit newly trained staff from the Nachwuchsgruppen.[325] The documentation I have seen so far unfortunately provides insufficient evidence to judge the degree of direct corporate participation in the center.

A new wrinkle in Martinsried's relations with the universities emerged in 1988, when a typescript by Benno Müller-Hill and his student Heinrich Herbertz appeared on Robert Huber's desk. Huber was then the managing director, and he had previously agreed to Müller-Hill's request to allow a student to visit Martinsried to obtain some data about the size and staffing of its laboratories for a study he was supposedly doing as part of his *Diplom* thesis. Huber was not pleased to find some of this data appearing in an article submitted to the prestigious journal *Nature* and containing an unflattering comparison between Huber's large, well-funded research institute in Martinsried and Müller-Hill's crowded university institute for genetics in

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324 Since the 1970s Oesterhelt had been studying the molecule *Bacteriorhodopsin* and was able to demonstrate that it acted as a molecular pump converting »light energy into chemical energy for the cell – essentially a new form of photosynthesis«, which was the starting point for studies of potential technical applications. Oesterhelt died on 28 Nov. 2022 at the age of 82. Max Planck Institute of Biochemistry: Dieter Oesterhelt (1940–2022), 12/5/2022. https://www.biochem.mpg.de/dieter-oesterhelt-deceased. Last accessed 12/3/2022. As discussed below, Oesterhelt's department also worked with Robert Huber on the research leading to the Nobel Prize. See also Dieter Oesterhelt and Mathias Grote: *Leben mit Licht und Farbe. Ein biochemisches Gespräch.* Diepholz: GNT-Verlag 2022. Excerpt with above quotation in English at Max Planck Institute of Biochemistry: Life with Light and Colour: A Biochemical Conversation, 9/24/2021. https://www.biochem.mpg.de/7833015/news_publication_17585131_transferred?c=6519101. Last accessed 12/3/2021.

325 Documents and correspondence on the Gene Center and the MPI for Biochemistry, AMPG, II. Abt., Rep. 1, Nr. 634, fol. 202–491; Especially: Staab to Winnacker, 9 Feb. 1988, fol. 211–213; Marsch: Vermerk, 16. 10. 1987, fol. 215–216. For support by Hoechst see Rolf Sammet (Hoechst AG) to Wulf Steinmann, LMU, 7 Oct. 1983, BayHStA, MK 77610.

Cologne. Müller-Hill's data for the scientific efficiency and productivity of the two institutes were based on the Science Citation Index, a reasonably reliable source, but drawn from a narrow chronological period (1980–1981), in which (as noted above and as Huber pointed out to Müller-Hill in response) Martinsried had been undergoing a major turnover in its leadership. Such a situation is commonly associated with reduced productivity.[326] In any case, *Nature* chose to publish only a brief but equally critical »commentary« by Müller-Hill in 1991, calling for the government to increase support for university research and »slow down or even freeze further growth« of the MPIs, and the two co-authors later published an expanded analysis of their original paper in a less prestigious journal.[327]

4.15 Legitimizing the idea of a »new Dahlem« – the Nobel Prize in Chemistry, 1988

Huber, still stinging from what had appeared to be unfair criticism, must have been especially gratified later that year when he learned that he and two younger former colleagues (Johann Deisenhofer and Hartmut Michel, by then at positions in Dallas, Texas, and the MPI for Biophysics in Frankfurt a. M., respectively) were to share the 1988 Nobel Prize for their use of x-ray crystallography to determine the three-dimensional structure of the photosynthetic reaction center from the purple bacterium *Rhodopseudomonas viridis*.[328] Nobel Prizes bring not only welcome funding, but perhaps unwelcome attention. In this particular case, however, they also served to legitimize the link of Martinsried with its ancestral scientific community, Dahlem. More particularly, by thus recognizing a fundamental contribution of the Martinsried center to our understanding of the chemical nature of a fundamental biological process, the Nobel Prize also served to highlight Martinsried's role in carrying forward the mission of interdisciplinary collaboration between chemists and biologists that Emil Fischer had set forth for Dahlem more than seven decades earlier. In the light of the purposes of this paper, one may recall that Huber had been an adept collaborator across institutional lines from the beginning, as discussed ear-

326 Heinrich Herbertz and Benno Müller-Hill: University versus Research Center. An Attempt to Evaluate Funding of Science in West Germany. Institut für Genetik, Universität zu Köln, no date [cover letter to Huber dated 23. 9. 1988]), AMPG, III. Abt., Rep. 84/1, Nr. 684, fol. 48–72; Huber's response, 28 Sept. 1988, ibid.

327 Benno Müller-Hill: Funding of Molecular Biology. *Nature* 351/6321 (1991), 11–12. doi:10.1038/351011a0, which (evidently in response to Huber's criticisms) included one more institution (the MPI for Molecular Genetics in Berlin) and a slightly longer chronological period (1980–1984). The later, much more broadly comparative study is Heinrich Herbertz and Benno Müller-Hill: Quality and Efficiency of Basic Research in Molecular Biology. A Bibliometric Analysis of Thirteen Excellent Research Institutes. *Research Policy* 24 (1995), 959–979. doi:10.1016/0048-7333(94)00814-0.

328 A »photosynthetic reaction center« is a technical term referring to a complex structure of multiple proteins, pigments, and other substances whose interactions transform the incoming light energy of a photon into a form usable by a green plant, algae, or bacteria. It is one of the phenomena that makes life possible on earth. For details see the Nobel lectures and Nobel biographies of Robert Huber, Hartmut Michel, and Johann Deisenhofer, online at: The Nobel Prize: Robert Huber. Facts. https://www.nobelprize.org/prizes/chemistry/1988/huber/facts/. Last accessed 2/20/2023; Johann Deisenhofer and Hartmut Michel: The Photosynthetic Reaction Centre from the Purple Bacterium *Rhodopseudomonas Viridis*. In: Tore Frängsmyr and Bo G. Malmström (eds.): *Nobel Lectures, Chemistry 1981–1990*. Singapore: World Scientific Publishing 1992, 526–565. https://www.nobelprize.org/uploads/2018/06/deisenhofer-michel-lecture.pdf. Last accessed 2/20/2023; Huber Biographical, 1988; The Nobel Prize: Hartmut Michel. Biographical. https://www.nobelprize.org/prizes/chemistry/1988/michel/biographical/. Last accessed 2/20/2023; The Nobel Prize: Johann Deisenhofer. Biographical. https://www.nobelprize.org/prizes/chemistry/1988/deisenhofer/biographical/. Last accessed 2/20/2023.

lier. In his Nobel autobiography, he explicitly stressed that he continued this practice throughout his career: »Most of these structural studies were collaborative undertakings with other laboratories, many of them from foreign countries.«[329] Huber elaborated on this point in the acknowledgments to his Nobel lecture, stressing that the reaction center project had begun as a collaboration with a neighboring section in Martinsried: »J[ohann] Deisenhofer's and my interest in structural studies of the photosynthetic reaction centre of Rps. viridis was raised by the establishment of D[ieter] Oesterhelt's department in Martinsried in 1980; he brought with him H[artmut] Michel, with whom a fruitful collaboration on the analysis of the crystal structure of this large protein complex began.« Huber then listed a large number of collaborators he had worked with in Martinsried, as well as »collaborative undertakings with biochemists in Switzerland (H. Zuber, W. Sidler), USA (M. L. Hackert) and Italy (M. Bolognesi, A. Marchesini, A. Finazzi-Agro.«[330] In short, Huber viewed his scientific work as inherently collaborative, interdisciplinary, and international, which is hardly surprising in view of the complex and interdisciplinary nature of modern biochemistry and molecular biology in general.

In May 1992, the journal *Science Watch* published a table of the fifty most productive research institutions in molecular biology, ranked according to citation impact during the decade 1981–1991 as calculated from the ISI database of science indicators. The MPI for Biochemistry in Martinsried was fifth on the list, right behind the EMBL in Heidelberg (Table 8 below). To Butenandt, this information no doubt confirmed that he had been correct in seeking to create a new Dahlem in Martinsried, and it showed that German scientists were finally catching up with the Americans (who occupied the first three positions on the list).

329 Huber Biographical, 1988.

330 Robert Huber: A Structural Basis of Light Energy and Electron Transfer in Biology. In: Tore Frängsmyr and Bo G. Malmström (eds.): *Nobel Lectures, Chemistry 1981–1990*. Singapore: World Scientific Publishing 1992, 574–616, 610.

TABLE 8

Top 50 research institutions in molecular biology, ranked by citation impact, 1981–1991 (based on those publishing more than 200 papers).[331]

Rank	Institution	Papers 1981-91	Citations 1981-91	Citations Per Paper	Rank	Institution	Papers 1981-91	Citations 1981-91	Citations Per Paper
1	Cold Spring Harbor Laboratory	248	10,261	41.38	26	Max Planck Inst. Mol. Genet., Berlin	243	4,645	19.12
2	MRC, Lab. Molecular Biology, Cambridge	453	18,281	40.36	27	Univ. Calif., San Francisco	779	14,854	19.07
3	*Univ. Minnesota, St. Paul-Minneapolis	485	18,572	38.29	28	SUNY Stony Brook	338	6,283	18.59
4	European Molecular Biology Lab., Heidelberg	536	17,283	32.24	29	Institut Pasteur	798	14,554	18.24
5	Max Planck Inst. Biochemistry, Martinsried	349	11,023	31.58	30	Princeton University	232	4,212	18.16
6	Hutchinson Cancer Res. Ctr., Seattle	223	7,016	31.46	31	University of Oxford	482	8,676	18.00
7	University of Basel	302	9,141	30.27	32	Univ. Calif., Berkeley	843	15,025	17.82
8	Max Planck Inst. Plant Breeding, Cologne	242	7,033	29.06	33	Univ. Washington, Seattle	832	14,795	17.78
9	Rockefeller University	415	11,417	27.51	34	University of Paris VII	283	4,916	17.37
10	Salk Inst. Biological Studies	280	7,657	27.35	35	University of Uppsala	397	6,803	17.14
11	German Cancer Res. Ctr., Heidelberg	266	6,901	25.94	36	University of Zurich	364	6,040	16.59
12	Caltech	288	7,222	25.08	37	University of Hawaii	216	3,450	15.97
13	MIT, including Whitehead Institute	781	19,400	24.84	38	Yale University	1,019	16,255	15.95
14	Univ. Calif., San Diego	557	13,838	24.84	39	Univ. Colorado, Boulder	219	3,449	15.75
15	Fac. Med. Strasbourg, Inst. Chim. Biol.	236	5,862	24.84	40	University of Leicester	221	3,384	15.31
16	University of Geneva	307	6,875	22.39	41	University of Cambridge	537	8,213	15.29
17	National Inst. Medical Research, London	250	5,594	22.38	42	University of Freiburg	304	4,613	15.17
18	Swiss Fed. Inst. Tech.-Zurich (ETH)	338	7,417	21.94	43	Nagoya University	263	3,962	15.06
19	Weizmann Institute of Science	394	8,638	21.92	44	Columbia University	689	10,306	14.96
20	Harvard University	1,551	33,840	21.82	45	University of Dusseldorf	207	3,097	14.96
21	Stanford University	927	19,610	21.15	46	Northwestern University	267	3,977	14.90
22	John Innes Institute, Norwich	259	5,317	20.53	47	Kyoto University	527	7,608	14.44
23	University of Heidelberg	321	6,577	20.49	48	Univ. Wisconsin, Madison	1,023	14,748	14.42
24	NIH, National Cancer Institute	1,168	23,300	19.95	49	University of Sussex	274	3,921	14.31
25	Imperial Cancer Research Fund	570	11,363	19.94	50	University of Munich	375	5,340	14.24

331 Table from *Science Watch* (May 1992); data from ISI Science Indicators Database, 1981–1991, with detailed clarifications in the original (not reproduced here). In AMPG, III. Abt., Rep. 84/1, Nr. 684, fol. 311.

5 Concluding Reflections on New Dahlems vs. the Old

5.1 Kuhn vs. Butenandt as postwar innovators in biochemical research and institutions

Richard Kuhn and Adolf Butenandt came out of the Second World War as leaders of German biochemical research, with their research groups largely intact and with no significant damage to their institute buildings. Admittedly, Butenandt had to make a new start by recreating his Dahlem KWI for Biochemistry in Tübingen, while Kuhn had to deal with the American confiscation of half of his KWI for Medical Research (albeit not the part that contained most of his facilities). Both men had successfully adapted to the National Socialist system, contributed to its scientific successes, and collaborated in morally questionable projects in ways that they afterwards sought to downplay or obfuscate, and both men dealt effectively with the immediate problems raised by the postwar occupation of Germany. But their future, and the future of a divided and impoverished Germany, remained uncertain. Toward the end of the 1940s, both men faced fundamental questions about how to proceed with their careers: should they accept attractive offers to move their research groups abroad? Or should they remain in Germany, continuing to work with the new MPG and finding additional resources to modernize their scientific enterprises in order to keep up with the global research front? Butenandt's decision was straightforward and entailed close collaboration with Heinrich Hörlein to unite three corporate partners in the so-called Triple Agreement of Bayer, Schering, and Hoffmann-LaRoche, which enabled him to adequately equip his institute in Tübingen and thus to remain there until he could negotiate an even larger and more modern institute complex in Munich. He presided over a highly productive research organization (submitting all of its results to his industrial partners for prior approval before publication) and also continued to teach physiological chemistry in the medical faculty until being elected as president of the MPG in 1959. In this new position, Butenandt was poised to transfer the MPG's administration to Munich, successfully leading it into a new world of big science and collegially organized research institutes, including the MPI for Biochemistry in Martinsried. Throughout, he retained his profitable and congenial connection with industry until his retirement, including a position on Bayer's supervisory board.

In contrast, Kuhn seemed less loyal to the new MPG or to its German context in 1949. While Butenandt remained the consummate insider, Kuhn acted more as an outsider to his German colleagues, resuming his Austrian citizenship and entering into consulting relationships with Sandoz in Basel and Wyeth / American Home Products in Philadelphia while turning down Hörlein's offer to organize support for him in German industry. Although Kuhn did establish a consulting connection to BASF in 1950, at the same time he nearly accepted a permanent professorship at the medical school of the University of Pennsylvania, before abruptly changing it to a guest professorship for two years. During this time, he collaborated on a significant study of human milk and its components, particularly the oligosaccharides, with his former associate there, Paul György, in the research facilities set up for him by Wyeth. Although he might well have continued these arrangements, he appears to have been increasingly dissatisfied and

brought the collaboration to a close, returned permanently to direct the MPI at Heidelberg, and shifted his industrial consultantship from AHP to BASF (whose chairman, Wurster, also gave Kuhn a leading role on the supervisory board). As Butenandt had done in Tübingen and Munich, Kuhn modernized the institute with new apparatus, and Karl Hermann Hausser began his pioneering work on NMR and EPR spectroscopy under Kuhn's leadership. Urged by associates in the MPG to oppose Butenandt for the presidency in 1959, he made only a half-hearted attempt, initially again styling himself as an ineligible outsider due to his Austrian citizenship, then changing his strategy and trying to campaign in earnest, but unsuccessfully, for the position. While there is no question that Kuhn was a brilliant organic chemist who could be personally charming, he seems to have lacked Butenandt's qualities of administrative leadership and tact, and others saw him at crucial moments as playing a double game that undermined their trust in him.[332] Nevertheless, one must acknowledge that at a key moment for the future planning of institutes in the MPG, Kuhn as vice president spoke strongly in favor of an American-style, collegially-managed departmental structure for the Martinsried biochemical center during a critical senate meeting in March 1965. Yet Kuhn had thus far strongly opposed the re-establishment of a collegial organization in his own MPI in Heidelberg, and one might suspect that Butenandt used Kuhn's own words regarding Martinsried to persuade him to finally accept the idea of a transition to a collegial system in Heidelberg, which began during the last year of his life.

5.2 Martinsried as a »new Dahlem«?

This paper has presented evidence that the idea of creating the biochemical center / MPI for Biochemistry in Martinsried as a »new Dahlem« originated with Adolf Butenandt. Moreover, the preceding analysis is based on the assumption that the idea of a new Dahlem in Martinsried was not a purely rhetorical notion, but that there are many traits in common between the origins and early development of Martinsried and the original Dahlem as scientific communities. One must, of course, consider that Martinsried was a single large institute complex, rather than a group of separately administered institutes as was the original Dahlem. But at the same time, I believe that it was absolutely necessary for Butenandt to invoke the spirit of Dahlem in justifying the creation of the complex in Martinsried. Only by invoking Dahlem, the almost mythical (but by 1945 mostly lost) scientific paradise of pre-war Germany, could Butenandt overcome the objections to creating large-scale institutes that would otherwise have blocked not only Martinsried, but several other projects that developed during his presidency. In other words, Martinsried was a test case for Butenandt's new policy of fostering a particular type of »big science« institute in the Max Planck Society. As a hybrid of a new large-scale, collegially managed institute with the traditional small-scale, individually-directed Dahlem institutes, Martinsried had achieved considerable success by 1990, despite an extremely long gestation period and many setbacks after it opened in 1973. In the process, Martinsried also became a test case for the

332 Cf. Warburg to Meyerhof, 12 Aug. 1951, UPA, UPT50 M613, Box 1, Folder 36.

modified Harnack Principle that Butenandt had set forth in 1965. No longer would the MPG build an institute around a single scientist, as had been the general practice in Dahlem and elsewhere under the KWG. With perhaps some minor exceptions where small institutes would still be appropriate, the operative unit was now the section of a larger institute, and the new principle was that if a scientist retired or died, their section would be closed, unless another outstanding researcher might be available in that same field. Otherwise, the space left open would either be merged with another section, or a specialist in a different field would be appointed to shape their new section as they might find appropriate. The MPG took a conservative approach here by not replacing all of the scientific members who departed by 1990, so that the total number fell from twelve sections to ten, opening space for Nachwuchsgruppen and other organizations such as the Theoretical Institute of the MPI for Psychiatry and the Gene Center of the LMU. The limits on growth for the biochemical MPI also took architectural form, as the third laboratory »star« building for biochemistry foreseen in the original plans for Martinsried was instead built in the early 1980s as the new building of the Theoretical Institute for Psychiatry (which admittedly entailed elements of biochemical research in its program,[333] so that the proximity of the two institutes in Martinsried could potentially foster interdisciplinary research as already noted in the case of the Gene Center and Oesterhelt's section).

5.3 The problem of international competition in Dahlem and in postwar West Germany

The »old Dahlem« emerged under the sign of international scientific competition, as expressed by Adolf Harnack's memorandum to the Kaiser in 1909, which led to the establishment of the Kaiser-Wilhelm-Gesellschaft (KWG) under Harnack's presidency in 1911. Harnack had stressed the danger of Germany's falling behind other nations in science, if it failed to establish a group of research institutes in critical fields.[334] While this claim was exaggerated to some extent, it nevertheless applied to certain areas, particularly interdisciplinary fields that did not fit effectively into the system of university faculties and institutes. In particular, as discussed earlier, despite the pioneering work of Imperial German scientists in physical chemistry and physiological or biochemistry, these fields were limited to mostly peripheral positions and smaller institutes, whereas the classical organic chemists dominated the larger university institutes for general chemistry. Hence it was logical for the planners of the KWG to focus on compensating for the weaknesses of the universities by developing research institutes for the allegedly

333 Henning and Kazemi, *Handbuch*, Vol. 2, 2016, 1387.

334 The rhetorical strategy of claiming that a country is »falling behind« in science, necessitating drastic institutional reforms and increased public support, goes back at least to Charles Babbage: *Reflections on the Decline of Science in England.* London 1830; for another example in German scientific institutions see Helmuth Trischler: The Syndrome of Falling Behind. Resource Constellations and Epistemic Orientations in the Natural and Engineering Sciences. In: Mark Walker et al. (eds.): *The German Research Foundation 1920–1970. Funding Poised between Science and Politics.* Stuttgart: Franz Steiner Verlag 2013, 98–114.

neglected fields; or to be more precise, by appointing outstanding leaders in these fields who could not find university positions commensurate with their abilities and achievements.[335]

Was biochemistry in the Federal Republic of Germany still »behind« its international competitors in the early 1960s, and could this be linked to problems in the university system? As Butenandt reluctantly conceded in his 1963 presidential address, there are good reasons for believing that, fifty years after the founding of the KWG, one could make more than a simply rhetorical case for this issue. It has been demonstrated, for example, that despite the achievements of Butenandt and Kuhn, together with the continuing strength of German biochemists in certain fields (mainly the analysis and synthesis of smaller biological and medically-active molecules such as hormones and vitamins), the overall impact of the National Socialist era on biochemistry was negative. This resulted not only from the extensive purges of politically and »racially« unacceptable researchers, followed by the destructive impact of the war, but also (aside from a few exceptions) from a decade or so of relative intellectual isolation after the war, despite the absence of an official boycott of German scientists by international institutions, as had occurred after the First World War.[336] This impact was intensified by a decisive mid-century shift from German to English in the language of chemistry, so that German work tended to be less recognized on the international level, as opposed to biochemical innovations in English-speaking countries. Whereas before the war German biochemists might have expected foreign students to learn German in order to learn the most advanced techniques in German universities, the situation after the war was largely reversed, and German biochemists were learning English to study in the UK and USA. Moreover, although Butenandt, Kuhn, and Lynen had declined calls from universities abroad, some of their younger colleagues found such positions to be more attractive than staying in Germany.[337] This »brain drain« continued to be an issue into the early 1980s. The German Federal Minister for Research and Technology emphasized the need to attract back from abroad the qualified scientists who were needed to lead research groups that were to be established in research and teaching centers such as the Gene Center of the LMU, which, in 1984, opened in the second laboratory building of the MPI for Biochemistry in Martinsried, occupying one floor of the north (sections L and M) wing.[338]

Biochemistry had thus become one of the fields highlighted by the recently formed German Council of Science (Wissenschaftsrat) in its report of 1960 as in need of new chairs and institutes. Günther Weitzel, Butenandt's successor in the chair of physiological chemistry in Tübin-

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335 I am of course omitting here the social prejudice that confronted the Jewish scientists who tended to be overrepresented in the early KWG. Kuhn's mentor Richard Willstätter and Butenandt's Dahlem predecessor Carl Neuberg are the two men most relevant to the present discussion. Arguably, however, institutional and social conservatism were linked in the Imperial German universities.

336 Deichmann, Emigration, 2002, 449–471.

337 Deichmann, Emigration, 2002, 449–471, cites several examples.

338 Protokoll des Ministergespräches Biotechnologie am 17. 2. 1983, AMPG, II. Abt., Rep. 1, Nr. 634, fol. 520. The Gene Center moved to a new building of the LMU on its Großhadern campus in 1994. Henning and Kazemi, *Handbuch*, Vol. 1, 2016, 212.

gen, made effective use of these recommendations in advocating the creation of a new course of study for the *Diplom* in physiological and biochemistry, which other universities could adopt. He pointed out that the Council of Science had called for doubling the number of chairs in physiological chemistry (mainly in the medical faculties), and for establishing independent chairs in biochemistry in all natural-science faculties (as late as 1960, only a handful of such chairs and institutes existed).[339] The Tübingen course of studies, introduced with twenty students in 1962, graduated its first five certified biochemists (*Diplom-Biochemiker*) in 1966. By this time it had 400 students and had to turn many applicants away, reflecting the rapidly growing interest in the field.[340] In view of the similarities in the fields of physiological chemistry and biochemistry, and in order to minimize the institutional barriers between faculties, the West German Conference of Rectors [i. e., university presidents] also recommended »a reciprocal representation of chairholders in the faculties« in order to foster collaboration across faculty lines.[341]

These measures served to promote a rapid development of biochemistry in the Federal Republic of Germany during the 1970s and 1980s, a process that was no doubt aided by the growing prestige of the Martinsried complex. When compared with the small number of chairs of biochemistry and physiological chemistry in the West German universities immediately after the war, the numbers on the eve of reunification four decades later were truly impressive.[342] On the other hand, to what extent did the rapid growth of biochemistry (and molecular biology) help to fulfil the earlier vision of Emil Fischer that the collaborative interdisciplinary efforts of chemists and biologists in Dahlem (or by extension, in German research institutes overall) would produce a new, synthetic-chemical biology? This is a question that unfortunately cannot be fully answered in the present paper, but the evidence presented here does suggest that the Martinsried researchers were moving in that direction, even if they were still far from the ultimate goal.

5.4 The role of industry in promoting the postwar recovery of science in Germany

The extent to which Butenandt benefited from direct industrial subsidies in the postwar era beginning in 1949 has not received as much attention in the literature as it deserves. That may be in part because Butenandt took great care to avoid publicizing his close connection with

339 Günther Weitzel: Memorandum zur Einrichtung des Studiums der Physiologischen Chemie und Biochemie an der Universität Tübingen (Februar 1962), 7–8, citing »Empfehlungen des Wissenschaftsrates« (1960), UAT, 117E/1310.

340 G. Weitzel: Begründung für die Aufstellung einer »Rahmenordnung für die Diplomprüfung in Biologischer Chemie«, Tübingen, 31 Aug. 1967, UAT, 117E/1310.

341 President Rüegg, Westdeutsche Rektorenkonferenz, to Deans of the medical and natural-science faculties in the BRD and West Berlin, Bad Godesberg, 23 Oct. 1967, UAT, 117E/1310.

342 Arbeitsgemeinschaft der Lehrstuhlinhaber von Unterrichtsinstituten für Chemie an Hochschulen der Bundesrepublik Deutschland and Gesellschaft Deutscher Chemiker, Chemie und Biochemie in der Bundesrepublik Deutschland: *Institute, Personen, Arbeitsgebiete, Publikationen.* Weinheim: VCH 1989; this had 673 pages.

industry through the Triple Agreement of Bayer, Schering, and Hoffmann-La Roche. It should be noted that after the initial three-year term in which he received a total of 150,000 DM for his institute, Butenandt continued to receive 50,000 DM per year for his institute until he retired, and the institute was transferred to Martinsried. Butenandt's industrial subsidies came at a price, requiring the submission before publication of all papers coming out of his institute to his three corporate benefactors for their potential utilization as the basis for patents. To what extent might this have affected the way in which Butenandt managed research in his institute? There is no doubt that he did not want the subsidies to be publicly known; none of the papers appears to contain any acknowledgement of industrial support, and many or most of those who worked in the institute may even have been unaware of it. This was rather different from the beginning of the KWG, in which the Reich and Prussian governments invited industry to play a highly public role.[343] The phenomenon deserves more detailed analysis than has been possible here, but the potential interest of the problem is suggested by the brief discussion above of the role of the Triple Agreement firms in supporting ecdysone research, along with the issues arising from the competing priorities of academic publication vs. patenting and commercial development. Similarly, the present study covers only the rough outlines of Kuhn's consulting relationship with Wyeth, Sandoz, and BASF. To what extent did such relationships develop at Martinsried? At present, there is no evidence that after 1973, any of the scientific members of the Max Planck Institute for Biochemistry in Martinsried received the same sort of industrial subsidy that Butenandt or Kuhn had received, although Wünsch's peptide section had benefited from support by the Hoechst and Bayer firms when it was still part of the old MPI for Protein and Leather Research, and Walter Hoppe was apparently included as a consultant for the Triple Agreement firms as part of his work on the structure of ecdysone. It is possible, but not yet clear from the evidence, that these industrial subsidies were terminated when these scientists moved to Martinsried.[344] If so, the Martinsried institute was fundamentally different from Butenandt's institutes or, for example, the KWI for Chemistry, which opened in Dahlem in 1912. Once must admit that the same stricture does not apply to the Gene Center, for which, as noted earlier, its founder Winnacker obtained industrial subsidies, but these came mainly from the company, Hoechst AG, with which he had a special relationship due to his father's leading role there. Other outside sources of funding for work at Martinsried included charitable foundations, such as the Deutsche Krebshilfe e. V., which in 1983 contributed 0.7 million DM to support a junior scientists' group specializing in cancer research.[345] Toward the end of the 1980s, the MPI began listing specific numbers of scientists not on the permanent staff who received support from miscellaneous outside sources (Drittmittelbeschäftigte), some of which presumably included industrial sources such as the Chemical Industry Fund, sponsored by the German Chemical Industry Association (Veband der Chemischen Industrie) with the principal purpose of sup-

343 Johnson, *The Kaiser's Chemists*, 1990.

344 See correspondence and documents in BayHStA, MK 71240, esp. Dr. E. Wünsch: Betr.: Fortbestand der Abteilung für Peptidchemie am Max-Planck-Institut für Eiweiß- und Lederforschung, München (11. 10. 1966).

345 Generalverwaltung der Max-Planck-Gesellschaft (ed.): Tätigkeitsbericht 1983. *Max-Planck-Gesellschaft Jahrbuch 1984.* Göttingen: Vandenhoeck & Ruprecht 1984, 111–137, 135.

porting doctoral and postdoctoral fellowships for training in research, as well as innovative teaching methods; while only 33 scientists fell into this category in 1973 when the MPI opened, in 1989 there were 105, and 137 in 1990, in addition to the regular group of scientific guests and postdocs (121 and 151 in 1989 and 1990 respectively).[346]

5.5 The need to promote interdisciplinary collaboration through appropriate organization

Whereas the foregoing discussion highlights the problem of crossing disciplinary and faculty lines in the universities, fostering such interdisciplinary collaboration was a founding idea in Dahlem. As previously discussed, Emil Fischer, who as vice president was the most influential natural scientist in the leadership of the KWG, had paid particular attention to the possibilities of collaboration between chemists and biologists in Dahlem.[347] Adolf Butenandt had continued to believe in the importance of interdisciplinary collaboration, and as discussed earlier, he had personally and institutionally collaborated with his friend and colleague Alfred Kühn of the KWI/MPI for Biology in Dahlem, and to some extent later in Tübingen, by establishing a joint section for virus research. In postwar Tübingen, however, the biology institute withdrew from formal collaborative management, and the research group became a section within Butenandt's institute, which in 1954 became the MPI for Virus Research in Tübingen. This was in effect to compensate Tübingen for the loss of the MPI for Biochemistry. In moving to Munich, however, Butenandt effectively restricted prospects for a renewed collaboration with biologists in the MPG. Instead, interdisciplinary collaboration became somewhat more narrowly defined as between various subdisciplines of biochemistry, and, to some extent, physiology. The institute complex that Butenandt created in central Munich, after he had brought in Grassmann's MPI for Protein and Leather Research, was well-suited to this type of collaboration. Butenandt clearly hoped for a similar collaboration in Martinsried, albeit among a dozen different subdisciplines of biochemistry and the emerging molecular biology, just as Fischer had originally hoped for Dahlem. Both Fischer and Butenandt were motivated by perceived limitations in the German university system as they had experienced it, and they saw Dahlem and (for Butenandt) the various »new Dahlems« in Tübingen, Munich, and Martinsried as offering superior opportunities for collaboration across (sub)disciplinary lines, even if this meant breaking to some extent with the proud university tradition of the unity of teaching and research that they both continued to embrace. Here again, Martinsried became a test case, as shown in the next section.

346 Generalverwaltung der Max-Planck-Gesellschaft (ed.): *Max-Planck-Gesellschaft Jahrbuch 1990*. Göttingen: Vandenhoeck & Ruprecht 1990, 114. Cf. Fonds der Chemischen Industrie im Verband der Chemischen Industrie e. V.: *Förderprogramm des Fonds der Chemischen Industrie 2023/2024* (17 April 2023). https://www.vci.de/fonds/downloads-fonds/publikationen/2021-04-foederprogramm-des-fonds.pdf. Last accessed 6/18/2023.

347 Fischer, Die Kaiser-Wilhelm-Institute, 1924, 796–809 [1915].

5.6 The problem of finding an optimal form of management for a »big science« (*Großforschung*) institute: Directorial or collegial, or some sort of hybrid?

Of course, Martinsried as a single large institute complex had to be organized differently from Dahlem's community of institutes, where the Harnack Principle of »building an institute around a director« had largely prevailed. It is nevertheless clear that despite what appear to have been initial reservations about »big science« in the collegial or »democratic« form of a »department institute«, Butenandt went to great lengths to foster a spirit of egalitarian collegiality among the leading Martinsried scientists. That began with the unique notion of a »Martinsried Circle« including the future directors of all the expected sections. This was somewhat like an American-style departmental system, rather than the hierarchical structure of the traditional German institute, and the collegial direction was supposedly a way of avoiding the perils of a »mammoth institute«, which I have identified with the »quasi-collegial« management system critiqued by Osterwalder in 1968. I have also argued that the KWI for Medical Research in Heidelberg presented an important precedent for the new form of collegial management in a large institute, even though the system had broken down almost immediately and instead became effectively a quasi-collegial system under Richard Kuhn. This fact, and the fact that he resisted the restoration of collegiality in his institute, makes it all the more ironic that Kuhn so strongly advocated collegial management for the large institute in Martinsried. Aside from that, however, Martinsried became an excellent test case for the planning, construction, and organization of a non-device-centered »big science« institute in the MPG. As shown in the guidelines of the architectural competition for the Martinsried center and the subsequent choice of a winner, the design of the building complex and the architecture itself was expected to play, and in this case probably did play, a critical role in fostering a spirit of community, unity, and interdisciplinary collaboration. In 1977, Butenandt certainly believed that it had done so, asserting that »since the opening of the institute, two or more sections have collaborated on the solution of no fewer than eighteen research problems«. This, he believed, was a »sign of the correctness of the basic conception.«[348] Moreover, the annual reports of the various sections of the Martinsried MPI during the mid-1970s confirm that the directors of each section put considerable emphasis on their collaborative efforts, not only with various sections of the MPI itself but also with the common facilities. Collaboration with external institutions, especially universities in Germany or abroad, was also a major theme. Thus, in 1976, Klaus Kühn of the section for connective tissue research reported at least four different collaborative projects: the three-dimensional sequencing of the structure of collagen, with the computer center; studies of blood corpuscles, with Ruhenstroth-Bauer's experimental medicine section; studies of collagen, with a scientist from the US National Institutes of Health; and research on the role of procollagen in the genetic regulation of immune response, with D. Götze of GSF (Gesellschaft für Strahlen- und Umweltforschung, Munich-Neuherberg).[349] Nevertheless, Butenandt's conclusion

348 Butenandt, Geschichte, 1977, 21; (also cited in Heßler, *Die kreative Stadt*, 2007, 222).

349 Klaus Kühn: Abteilung Bindegewebsforschung. In: Generalverwaltung der Max-Planck-Gesellschaft (ed.): *Jahrbuch der Max-Planck-Gesellschaft 1976*. Göttingen: Vandenhoeck & Ruprecht 1976, 79–83, 79.

must remain somewhat tentative for the longer-term development of the Martinsried complex. Even Robert Huber, who repeatedly demonstrated the advantages of collaboration across institutional, disciplinary, and even national lines both in Munich and in Martinsried, remained somewhat skeptical of the extent to which architecture could promote or enhance that collaboration.[350]

Just as the planners of Dahlem sought from the beginning to attract foreign scientists to the new community while promoting international scientific exchanges, the same was true in Martinsried, where it was recognized that advancing to the forefront in the most modern fields of biochemistry and molecular biology would require not only attracting foreign guest scientists who could exchange ideas with the permanent staff and postdoctoral fellows who might transfer ideas and techniques from Martinsried to other institutions abroad. The success of this effort could be seen in the large numbers of these two groups. Except for two years, 1974 and 1978, the numbers of guests and postdocs ranged from 118 to 180, significantly exceeding the number of regular scientific staff (in the mid-nineties).[351] But from the beginning, the scientists at Martinsried also sought to recruit to the permanent staff specialists from abroad who were familiar with the latest techniques. In the case of Martinsried, the planners targeted the Swedish scientist Pehr Edman, then in Australia, who became the first foreign addition to the Martinsried Circle. It appears, however, that they were more concerned with getting Edman into the group, as an internationally top-rated specialist in protein sequencing, than with contributions he might (but did not) make for them in the sequencing of nucleic acids. This may be considered an example of the new version of the Harnack Principle at work, i. e., once recruited, the scientist was allowed to »build a section around himself«, given complete freedom of choice of research topic, and therefore did not respond to the efforts of his Martinsried colleagues to change his priorities to suit their own wishes. Creative personalities were more important than the field they represented. The other side of the Harnack Principle was that once a director was dead or retired, their institute or section would be closed. This of course would not occur to the entire institute in Martinsried, but it was applied several times to particular sections, which the MPG's leadership closed, rather replacing their departed directors with ones working along similar lines. The closed laboratory sections generally went to new scientific members who established research groups in new, often quite different fields. The new members then became part of the continuing collegial structure of the institute, but, in the process, it appears that the initial impulse toward collaboration between sections may have lost some of its earlier impetus, which may also help to account for Huber's skepticism as mentioned above. That is, the death or retirement of a member would have naturally disrupted previously established collaborative relationships, particularly those incorporated into the initial planning of the institute (recall the impact of Karl Wilhelm Hausser's death on the KWI for Medical Research in 1933). New members entering vacant laboratory sections might, like Edman (or Bothe in Heidelberg),

350 Personal communication from Robert Huber, 4 Oct. 2017.

351 Numbers found in the *Jahrbücher* of the MPG, 1974–1991.

not find it appropriate to collaborate with their neighbors as they developed their own research programs.[352]

5.7 Relations between university and research institutes in Dahlem and Martinsried

One of the continuing issues in Dahlem, Heidelberg, and elsewhere had been the disadvantages arising when researchers did not have university professorships, which made it very difficult to recruit younger scientists in training. In any case, the physical remoteness of Dahlem and Martinsried from the university buildings in the city centers in Berlin and Munich worked against arrangements whereby a member of a research institute could simultaneously hold a regular university professorship, as Butenandt did in Tübingen and in Munich (an honorary professorship was of course another matter). In Dahlem the postwar era greatly mitigated this problem through the creation of the Free University, ironically however by transforming many of the former Kaiser Wilhelm Institutes (including Butenandt's old institute for biochemistry) into university buildings. The Free University restored the possibility for joint appointments that would enhance the training of younger scientists, but only in those few Max Planck Institutes still in Dahlem. Thus, the original scientific community of Dahlem itself could not be recreated in postwar Dahlem. Butenandt had tried to avoid this problem by simultaneously directing a university institute and an MPI in the city of Munich, but he soon discovered that the lack of space interfered with opportunities for establishing a Dahlem-like community of scientists, and his assumption of the MPG presidency put a definite end to his expensive, carefully designed institute complex. Moving the research complex out to Martinsried made it initially impractical (as it had been in Dahlem) for any member of the new MPI to duplicate Butenandt's former joint appointment. This began to be mitigated as the university moved out to Martinsried, which the MPG had planned on from the outset, but the process took a long time and did not benefit the original Martinsried Circle.

In 1984 the founding of the LMU Gene Center in the Martinsried institute complex itself ostensibly facilitated training younger scientists, as seen with the creation of three Nachwuchsgruppen. It also appears, however, that the arrangement produced administrative headaches for the MPG. In the broader context of the German university system, there was also some tension between the Cologne University Institute of Genetics and Martinsried, reflecting resentment in the universities of the (correctly perceived) better-funded, permanently-employed research scientists. Benno Müller-Hill even sought to demonstrate that his institute was scientifically more efficient and thus, relatively speaking, more productive. As shown here, however, that impression was countered not only by the Nobel Prize of 1988, but also the fact that by the early 1990s the Martinsried MPI for Biochemistry was rated among the top five institutions for molec-

352 This is a problem that would require more systematic investigation than was possible in the current project.

ular biology. This of course reinforced the image of Martinsried as an elite institution on par with the more recently established EMBL – which had originally been expected to be at Martinsried, a plan that had clashed with the expectations of the non-chemist molecular biologists circa 1970. That Martinsried, even without the EMBL, became a successful center for a diverse range of research projects in molecular biology as well as classical biochemistry, is again strong evidence for the superiority of its original design as a means to create a lively and open scientific atmosphere, not unlike that of an elite American department as Rudolf Mössbauer had depicted it. Thus, by 1990, the MPI could rightly describe itself as a »center for biosciences« incorporating fields extending beyond the disciplinary framework of biochemistry »in the strict sense«. These expanded research areas included cell biology, biophysics, and advanced data processing. Molecular biology, the study of »nucleic acids as carriers of genetic information«, occupied a more prominent position in Martinsried's research goals than it had in 1973, and it was now supplemented by federally-funded genetic engineering research groups collaborating with the LMU's Gene Center. Nevertheless, a »primary goal« of the MPI for Biochemistry had not changed over the decades: »understanding the function of molecules on the basis of their structure«.[353]

353 Generalverwaltung der Max-Planck-Gesellschaft (ed.): MPI für Biochemie, Martinsried bei München. *Max-Planck-Gesellschaft Jahrbuch 1990*. Göttingen: Vandenhoeck & Ruprecht 1990, 87–131, 87.

6 Epilogue. The Advent of Chemical Biology – Fulfilling Fischer's Dream?

Martinsried's success in molecular biology in comparison with prewar Dahlem recalls the quote with which this paper began, from Emil Fischer's lecture of 1915 calling for »synthetic chemical biology«. Here one might consider some of the most recent developments on the disciplinary borderlands between chemistry and biology. Germany's former Society for Physiological Chemistry (founded in 1947) became the Society for Biochemistry and Molecular Biology (GBM) in 1996. Its former journal of physiological chemistry is now called *Biological Chemistry*. More recently, there has also been a movement in the other direction. In January 2021 the GBM's Joint Professional Group for Chemical Biology (shared with three other organizations) awarded its first Richard Willstätter Prize for Chemical Biology at an online conference »Advances in Chemical Biology.«[354] Given the foregoing analysis, the prize might also be called the Emil Fischer Prize, but there can be no question that Willstätter justly deserves this recognition from the German scientific community. In any case, the prize went not to a scientist in Martinsried but to one in Dortmund, Herbert Waldmann, director of the laboratory for Chemical Biology at the Max Planck Institute for Molecular Physiology founded after the merger of two older institutes in 1993. According to their website, »Our research focuses on the development of methods and strategies for the production of a whole collection of active substances that are closely related to individual natural products.«[355] In other words: Waldmann's research group are doing synthetic-chemical biology, in a way not too different from that envisioned more than a century ago by Emil Fischer. But investigating how they came to be doing that, and the extent to which the resulting knowledge might disrupt the living world, will have to be a task left for another paper.

354 Gesellschaft für Biochemie und Molekularbiologie e. V.: Richard-Willstätter-Preis für Chemische Biologie, 11/27/2020. https://gbm-online.de/news-details/richard-willst%C3%A4tter-preis-f%C3%BCr-chemische-biologie-188.html. Last accessed 3/10/2021.

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11 Index of persons

12 List of abbrevations

ABI	Applied Biosystems, Inc.
ACS	American Chemical Society
AG	Aktiengesellschaft / corporation
AHP	American Home Products
AMPG	Archives of the Max Planck Society (Archiv der Max-Planck-Gesellschaft) for the Advancement of Science, Berlin-Dahlem
AOP	Ausserordentlicher / extraordinarius (associate) Professor
ATP	Adenosine triphosphate
BAL	Bayer Archives, Leverkusen
BASF UA	BASF SE, Unternehmensarchiv / Corporate Archives, Ludwigshafen a. R.
BayHStA	Bayerisches Hauptstaatsarchiv / Bavarian Central State Archives, Munich
CalTech	California Institute of Technology, Pasadena, California
CERN	Conseil Européen pour la Recherche Nucléaire / European Organization for Nuclear research
CSU	Christlich-Soziale Union in Bayern / Christian Social Union in Bavaria
DESY	Deutsche Elektronen-Synchrotron / German Electron Synchrotron
DFG	Deutsche Forschungsgemeinschaft / German Research Foundation
DM	Deutsche Mark / German mark
EMBC	European Molecular Biology Conference
EMBL	European Molecular Biology Laboratory
EMBO	European Organization for Molecular Biology
EMP	Embden-Meyerhof-Parnas Pathway
EPR	Electron Paramagnetic Resonance
ETH	Federal Technical University, Zurich
EUCOM	European Command, US Army
EURATOM	Europäische Atomgemeinschaft / European Atomic Energy Community
FIAT	Field Information Agency Technical
FRG	Federal Republic of Germany
GBM	Society for Biochemistry and Molecular Biology
GKSS	Gesellschaft für Kernenergieverwertung in Schiffbau und Schifffahrt / Society for Nuclear Energy Utilisation in Shipbuilding and Shipping
GP	Gemeindearchiv / Municipal Archives, Planegg
GSF	Gesellschaft für Strahlen- und Umweltforschung / Society for Radiation and Environmental Research
HMO	Human milk oligosaccharide
IG Farben	Interessengemeinschaft Farbenindustrie AG / Syndicate of the Dyestuffs Industry, Inc.
IPP	Institute for Plasma Physics
KWG	Kaiser Wilhelm Society
KWI	Kaiser Wilhelm Institute

LMU	Ludwig Maximilian University of Munich
LVA	Landesversicherungsanstalt / National Insurance Office
MPG	Max Planck Society for the Advancement of Science
MPI	Max Planck Institute
NARA	National Archives and Records Administration, College Park, Maryland
NIH	National Institutes of Health
NMR	Nuclear Magnetic Resonance
NSDAP	Nationalsozialistische Partei Deutschlands / National Socialist German Workers' Party
NS	Nationalsozialistisch / National Socialist
OP	Ordentlicher/ordinarius (full) Professor
RM	Reichsmark
SchA	Schering Archives, Berlin
SS	Schutz-Staffel
TU	Technical University
UAM	University Archives, Munich (LMU)
UAT	University Archives, Tübingen
UNESCO	United Nations Educational, Scientific and Cultural Organization
UPA	University of Pennsylvania Archives, Philadelphia
USAHEC	US Army Heritage and Education Center, Carlisle, Pennsylvania

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