CONTINUITY IN GERMAN SCIENCE, 1937-1972:
GENEALOGY AND STRATEGIES OF THE
TMV/MOLECULAR BIOLOGY COMMUNITY

DISSERTATION

Presented in Partial Fulfillment of the Requirements for
the Degree Doctor of Philosophy in the Graduate
School of The Ohio State University

By
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* * * * *

The Ohio State University
2002

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This dissertation is a case study in the social and cultural factors that foster and constrain the production of scientific knowledge. Its focal point is the history of a group of German scientists who began research on tobacco mosaic virus (TMV) in 1937 with the intent of using it as a model organism to understand basic problems in biology and genetics. They began their work in Berlin-Dahlem, and, because of the Second World War they were evacuated to the south German city of Tübingen in 1943. With minimal interruption, given the circumstances, they worked on TMV until the 1960s. Their research contributed to the solution of a fundamental problem—the relationship between proteins, which make up living tissue, and the genetic material that carries biological inheritance, now known as the Genetic Code. Since scientists must reconcile their professional identities as members of an international community of scholars with their identities as members of specific national communities, this study of science in the modern world is illustrative of the tensions generated by the clash between national and international interests. This group is unusual in that it experienced dramatic change in both its political and scientific contexts. The impact of National Socialism allows for a study of how political and social factors can limit the scientific enterprise, and the postwar revolution in biological thought allows for an examination of the process of rapid scientific change and its social impact. Most significantly, an analysis of this group allows for an understanding of German history outside of the political chronology imposed by National Socialism and the Second World War.
Dedicated to Karen
ACKNOWLEDGMENTS

Since I began this project several years ago I have received an extraordinary amount of support, both personal and professional, without which the overall quality of this study would surely have suffered. It is my great pleasure to thank the following people and organizations for their assistance.

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Hubert Dobson, Keith Green, and Nathan and Shonda Kohlhoff provided the friendship I needed during a very exciting but demanding year.

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On the personal side, my close friends John Hartshorne, Jessica Evans, Jason Adams, Derek Dauchy, Rick Bradley, and David Bosko are largely responsible for helping me to grow and mature as a person. Flash, Isaac, Grace, and Manni have made the writing of the manuscript much more interesting than it otherwise would have been. David Fincher has provided both entertainment and insight into being an author. Last but certainly not least, my family has been a source of constant support. I would like to thank my parents, Marcia and Lee Marquis, my in-laws, Ed and Betty Keefer, and my sisters, nieces, and nephews, for supporting my foray into the academic world. The love and support of all these people, though, pales before that given to me over the last ten years by my wife Karen. I am not enough of an author to express my gratitude to her in words, so I hope that sincerely dedicating this work to her will suffice.
VITA

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ABBREVIATIONS USED IN THIS STUDY

BRD: Bundesrepublik Deutschland (Federal Republic of Germany)

DAAD: Deutscher Akademischer Austauschdienst (German Academic Exchange Service)

DDR: Deutsche Demokratische Republik (German Democratic Republic)

DFG: Deutsche Forschungsgemeinschaft (German Research Society)

DFR: Deutsche Forschungsrat (German Research Council)

DM: Deutschmark

DNA: Deoxyribonucleic Acid

ERP: European Recovery Program (Marshall Plan)

FRG: Federal Republic of Germany

GDR: German Democratic Republic

JMB: Journal of Molecular Biology

KWG: Kaiser-Wilhelm-Gesellschaft (Kaiser-Wilhelm-Society)

KWI: Kaiser-Wilhelm-Institut

MPG: Max-Planck-Gesellschaft (Max-Planck-Society)

MPI: Max-Planck-Institut

PNAS: Proceedings of the National Academy of Sciences

RAC: Rockefeller Archive Center

REM: Reichserziehungsministerium (State Educational Ministry—Nazi Period)
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<td>RM:</td>
<td>Reichsmark</td>
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<td>RMEL:</td>
<td>Reichsministerium für Ernahrung und Landwirtschaft (State Ministry for Food and Agriculture—Nazi Period)</td>
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<td>RNA:</td>
<td>Ribonucleic Acid</td>
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<td>SAT:</td>
<td>Stadtarchiv Tübingen (Tübingen City Archive)</td>
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<td>SPD:</td>
<td>Sozialdemokratische Partei Deutschlands</td>
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<td>TMV:</td>
<td>Tobacco Mosaic Virus</td>
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<td>UAT:</td>
<td>Universitätsarchiv Tübingen (Tübingen University Archive)</td>
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WEST GERMAN SCIENTISTS IMPORTANT TO THIS STUDY

Anderer, F. Alfred. Studied biochemistry, worked with Gerhard Schramm in the MPI für Virusforschung in the late 1950s; significant figure in sequencing of TMV coat protein; director of Institut für Virusforschung after 1967.

Bergold, Gernot. Field of study insect virology. During World War II worked in satellite lab of Arbeitsstätte für Virusforschung in the city of Oppau. Emigrated to Canada in 1948; in 1959 to Venezuela.

Braunitzer, Gerhard. Began working with Adolf Butenandt in biochemistry in 1947. Collaborated with Gerhard Schramm on end-group analysis of TMV in early 1950s. Became one of the most successful protein sequencers in Germany, eventually became director of a division of the Max-Planck-Institut für Biochemie in Martinsried.

Bresch, Carsten. Began studying phage genetics due to assistance from Max Delbrück in 1947; ran his own small phage group in Göttingen in the early 1950s. Went to Cologne in late 1950s, became one of the first faculty members of the Institut für Genetik. In late 1960s took over leadership of genetics institute at Freiburg University.


Danneel, Rolf. Field of study zoology. Joined Alfred Kühn in Dahlem and became Kühn’s student in the Arbeitsstätte für Virusforschung. In 1945 moved to Göttingen, became professor of zoology at Bonn University in 1949.

Delbrück, Max. Studied physics and worked in Dahlem at the Kaiser-Wilhelm-Institut für Chemie in the early 1930s under Lise Meitner. Emigrated to the United States in the late 1930s where he became one of the most important figures in molecular biology after World War II. Worked to encourage modern biology research in West Germany as well.

Gierer, Alfred. Studied physics in Göttingen with Karl Wirtz in early 1950s. In 1954 joined Freksa’s division in Tübingen. One of Tübingen’s most successful researchers, co-published on infectious RNA with Gerhard Schramm, on nitrous-acid TMV mutants with Karl-Wolfgang Mundry, as well as own research on structure of TMV RNA. Director in Institut für Virusforschung after 1960.


Kühn, Alfred. Studied zoology under August Weismann in Freiburg; became director in Kaiser-Wilhelm-Institut für Biologie in 1937; co-published research on physiological genetics with Adolf Butenandt. One of three co-founders of Arbeitsstätte für Virusforschung.

Melchers, Georg. Studied botany, joined Fritz von Wettstein in Dahlem in 1937, was Wettstein’s student in Arbeitsstätte für Virusforschung. Succeeded Wettstein as division leader of Kaiser-Wilhelm/Max-Planck-Institut für Biologie. Became important public figure in West German science and also sponsored excellent TMV research in his division.


Schramm, Gerhard. Began studying organic chemistry with Butenandt in 1933; became Butenandt’s student in Arbeitsstätte für Virusforschung, eventually director of Institut für Virusforschung. Co-published infectious RNA work with Alfred Gierer, as well as dozens of other scientific papers from 1930s-1960s. One of the most visible and prolific members of Tübingen TMV group.


Starlinger, Peter. Studied medicine in Kiel and Tübingen before becoming a member of Freksa’s division in Institut für Virusforschung in 1954. In 1956 moved to Cologne University, began studies of bacterial genetics. Became one of first faculty members of Cologne’s Institut für Genetik in 1960.
Trautner, Thomas. Studied bacterial genetics with Carsten Bresch in mid-fifties. Worked with Salvador Luria and Arthur Kornberg in United States. Faculty member of University of California, Berkeley, before becoming director in MPI für molekulare Genetik in mid 1960s.

Weidel, Wolfhard. Began career under Butenandt in late 1930s; doctoral work on physiological genetics. Joined Melchers’ division in 1950, became world leader in microbiology; became division leader of Institut für Biologie in 1956.


Wittmann-Liebold, Brigitte. Studied protein sequencing under Gerhard Braunitzer in Munich; joined Wittmann’s group in 1960, became important contributor to TMV comparative sequencing research. Joined MPI für molekulare Genetik in mid 1960s.

Zachau, Hans Georg. Began studying chemistry with Butenandt in 1952. Became one of first faculty members of Cologne’s Institut für Genetik in 1960; later became first person correctly to sequence transfer RNA.

Zillig, Wolfram. Began studying chemistry with Butenandt in 1950; instrumental in helping refine phenol technique by which TMV RNA was isolated from protein; eventually became division leader in MPI für Biochemie in Martinsried.
Preface:
The Connection to Auschwitz

On June 7-8, 2001, the Max-Planck-Gesellschaft zur Förderung der Wissenschaften (Max-Planck-Society for the Advancement of Science, MPG), Germany’s leading non-university research organization, hosted an unusual meeting in the capital city of Berlin. Entitled “Biological Science and Human Experiments in the Institutes of the Kaiser-Wilhelm-Society: The Connection to Auschwitz,” the two-day symposium brought recognition to recent scholarly work that linked some of the most prestigious scientists from the MPG’s predecessor, the Kaiser-Wilhelm-Gesellschaft zur Förderung der Wissenschaften, to atrocities committed during the Nazi era. Most damning was evidence indicating that analysis of blood from Auschwitz inmates subjected to medical experiments had been analyzed in the Kaiser-Wilhelm-Institut für Biochemie (Kaiser-Wilhelm-Institute for Biochemistry) in the Berlin suburb of Dahlem. But apart from education, the symposium had a second purpose, that of reconciliation. Several survivors of Nazi medical experiments carried out in the Auschwitz concentration camp were invited to participate in the meeting, and at one point during the symposium, Hubert Markl, the President of the Max-Planck-Gesellschaft, publicly apologized to the victims. He apologized first for the criminal activities of scientists in
Nazi Germany, and, second, for the unwillingness of MPG officials to acknowledge an institutional responsibility to deal with the dark periods of German science.¹

An event such as this, in a country that took decades to come to terms with its recent past, was itself long in the making. For years, few in West Germany were anxious to acknowledge the complicity of German scientists with the Nazi government.² In fact, the first scholarly effort to deal with the activities of the biomedical community during the Third Reich, Tödliche Wissenschaft, written by Cologne geneticist Benno Müller-Hill, was not published until 1984. Even then, Müller-Hill’s book did not spark a healthy discussion about Germany’s past—for the most part, the public of West Germany paid little attention to the book. Privately, many scientists and their relatives were angry at Müller-Hill for broaching the subject.³

Several years later this climate began to change. Historians in West Germany and abroad began to produce a body of critical scholarship on the relationship between the German biomedical community and National Socialism. In early 1995, Adolf Butenandt, a leading researcher during World War II, died shortly before his 92nd birthday. In the postwar years, Butenandt, a Nobel Prize Winner, had become one of the most prominent scientists in West Germany and served two terms as president of the Max-Planck-


² In 1946, Max Weinreich published his study of the complicity of a broad range of scholars, including biological scientists, historians, anthropologists, and philosophers, in the Nazi persecution and destruction of the Jews. However, his book, published in Yiddish in the journal of the Yiddish Scientific Institute in New York, was a powerful critique by an outside, not an effort by Germans themselves to deal with the problem of scholarly complicity in Hitler’s Germany. The book has been reprinted as Max Weinreich, Hitler’s Professors: The Part of Scholarship in Germany’s Crimes Against the Jewish People, Second edition (New Haven: Yale University Press, 1999).

³ Benno Müller-Hill, Tödliche Wissenschaft. (Reinbeck bei Hamburg: Rowohlt Taschenbuch Verlag GmbH, 1984). The book was not reviewed by the German press until the author copied an English language review from Nature and sent it to the major West German newspapers; of these, only the Frankfurter Allgemeine Zeitung then published a review. Interview with Benno Müller-Hill, Cologne, June 21, 2000.
Gesellschaft. After his death, questions from the public regarding Butenandt's activities during the war began to accompany the accolades of his peers. His membership in the Nazi party, denied by him during his lifetime, became widely known, and his place of honor among German public figures became contested.\footnote{Butenandt's ties to individuals who had taken part in medical crimes was documented by Ernst Klee in his \textit{Auschwitz, die NS-Medizin, und ihre Opfer} (Frankfurt am Main: S. Fischer Verlag GmbH, 1997). Butenandt's unwillingness to admit the extent of his cooperation with the National Socialist government in the postwar era were noted by Nobel Prize winner James Watson in “Leichte Schatten über Berlin. Die Deutschen und ihre Genetiker: Anmerkungen eines amerikanischen Nobelpreisträgers,” \textit{Frankfurter Allgemeine Zeitung}, 19 July, 1997. The publicity generated about Butenandt became so negative that teachers in a school named after him began to question whether or not he should be honored in such a manner. \textit{Weser-Kurier} (Bremen), 31 August, 1998.}

Noting this, and noting the almost complete lack of critical history regarding the institutes of the Kaiser-Wilhelm-Gesellschaft during the Nazi time, President Markl decided to fund a group of independent scholars to research the history of the Max-Planck-Gesellschaft.\footnote{The Presidential Commission, consisting of several independent historians, guest scholars, and doctoral students, began work on the research project “Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus” in March, 1999. A brief report on the commission is Allison Abbot, “German science starts facing up to its historical amnesia,” \textit{Nature} 403 (February 3, 2000): 474-5. A great deal has been written about the Kaiser-Wilhelm/Max-Planck-Societies, but prior to the nineties, almost nothing dealt with the history of the society under the Third Reich. For example, in a massive Festschrift published in 1990, only fifty of a total of 1000 pages examined the experience of the Society under Nazism, and offered a chronicle of events rather than a critical history. See Helmuth Albrecht and Armin Hermann, “Die Kaiser-Wilhelm-Gesellschaft im Dritten Reich,” in \textit{Forschung im Spannungsfeld von Politik und Gesellschaft: Geschichte und Struktur der Kaiser-Wilhelm-/Max-Planck-Gesellschaft}, ed. Rudolf Vierhaus and Bernard vom Brocke (Stuttgart: Deutsche Verlags-Anstalt, 1990), 356-406. Kristie Macrakis provided the first in depth history of the political relationship between the MPG and the Nazi government. Kristie Macrakis, \textit{Surviving the Swastika: Scientific Research in Nazi Germany} (Oxford: Oxford University Press, 1993).} While admirable, critics such as Müller-Hill argued that researching and understanding the past were not enough—the MPG as an institution needed to take responsibility as well. He and others urged Markl to demonstrate their responsibility by apologizing to the few remaining survivors of Josef Mengele’s human experiments before it was too late.\footnote{Müller-Hill made this point in a paper given at the 1999 conference that inaugurated the work of the MPG Presidential Commission; it has been printed as Benno Müller-Hill, “Das Blut von Auschwitz und das Schweigen der Gelehrten,” in \textit{Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus: Bestandsaufnahme und Perspektiven der Forschung, Band 1}, ed. Doris Kaufmann (Göttingen: Wallstein}
One of the most critical articles regarding Butenandt, Ernst Klee’s “Augen aus Auschwitz,” was published in the influential weekly newspaper Die Zeit in January, 2000, as I was beginning research on this project in Munich. By coincidence, I had scheduled an appointment with a member of the general administration of the Max-Planck-Gesellschaft, located in Munich, to discuss my project shortly after the article appeared. I had hoped to discuss a number of subjects, including Butenandt’s role in pathbreaking biology research that had taken place in Germany from the 1930s to the 1960s. My appointment was delayed slightly while my host responded to inquiries from individuals regarding Klee’s article. At that point, I realized that I was very fortunate to be working on a topic about which many people cared. Only later did I begin to realize the responsibility that accompanies discussing a topic that is emotionally charged for many people.

History as a discipline concerns trends and changes that transcend individual human lives. Nevertheless, it is ultimately the study of people. Although many of the scientists who are at the heart of this study are no longer alive, many others are and are still active in the research community. All of them have friends, family, and co-workers who are concerned about the way in which historians will treat the German scientific community of the last century. Many of these people have taken time to sit for interviews, answer questions, and provide me with sources, assisting me without knowing what form the final product will take.

My experiences in researching this study have shown me the need to treat the historical actors discussed herein with respect and to try to understand their decisions as they themselves understood them, not just with the self-assurance produced by hindsight.

The central figures of this study—particularly those, such as Adolf Butenandt, Georg Melchers, and Gerhard Schramm, who endured the Third Reich—were extraordinarily gifted, exceedingly complex people. As this work has progressed, I have found myself admiring their intelligence, agreeing with some of their decisions, and disagreeing with others. Therefore, in producing a narrative of their scientific lives, I have tried to avoid over-simplification and easy answers in favor of the messy complexity that I believe characterizes the human world. I hope the results are of value to all of those who care about this subject and these people.
Chapter 1: Introduction

Historian Robert Proctor has demonstrated that National Socialist Germany produced outstanding research on cancer, particularly in correlating environmental factors, such as cigarette smoking, with specific types of cancer. The Nazi government then implemented public health measures to reduce the incidence of cancer in the German population. The Nazi regime, which many to this point have understood as being strongly anti-intellectual, seems to have been an unlikely sponsor for such innovative research. Yet it is clear that in this case, National Socialist science policy did not inhibit research, but instead actively promoted it and scientists responded enthusiastically. This has led Proctor to raise the general question, "What was science that it so easily adapted to fascist politics?"\(^1\)

This study answer's Proctor's question by using a case study to examine science in the broader perspective of twentieth-century German history. I show that science was a social activity that produced useful knowledge about the natural world, knowledge that transcended cultural and national boundaries. This intellectual quest, however, was performed by human beings who were themselves the product of unique cultural contexts, which made the process of knowledge production culturally specific. In the

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\(^1\) Robert N. Proctor, *The Nazi War on Cancer* (New Haven: Yale University Press, 1999), quote from 17, also see 76, 249, 278.
history of German science, the human/cultural element has, to this point, dominated scholarly discussion, generating a body of literature framed by the pivotal years 1933 and 1945. There is, however, an intellectual perspective as well, which I shall privilege in my narrative. By treating science as a human activity while framing my narrative around the knowledge produced by one particular scientific community, I offer a complementary perspective on German history in the twentieth century. Against a background of war, inhuman cruelty, and reconstruction, I shall trace the history of a group of researchers as they became part of a new scientific discipline, molecular biology. This discipline emerged in the 1930s, became institutionalized in the 1950s and 1960s, and produced a cosmopolitan subculture in nations around the globe.

The following is therefore a work of European cultural history, one that takes as its subject a very specific cultural elite—German biological researchers. It examines the work and influence of a small community of scientists, never more than a couple of dozen in number, who began their research in National Socialist Germany in the 1930s and worked continuously until the 1960s. Of central interest to me are the ways in which this elite subculture related to the worlds around them—the natural world that was the subject of their inquiries, and the human world of which they were a part. I therefore investigate the interaction of logic and empirical evidence with social and cultural factors in the production of scientific knowledge. The researchers discussed within began to study tobacco mosaic virus (TMV) in 1937 with the conscious intent of using the virus as a model organism to understand basic problems in biology and genetics. They established a laboratory in Berlin-Dahlem, but because of the war were evacuated to the south German city of Tübingen in 1943. Their research eventually contributed to the solution of a fundamental problem in biology—the relationship between proteins, which make up living tissue, and the genetic material that carries biological inheritance. This relationship is now known as the Genetic Code.
A detailed case study such as this can provide valuable insights into the ways in which scientists grapple simultaneously with the natural and human worlds of which they are also a part. Scientists form a clearly defined elite subculture with a strong sense of professional identity. This identity is rooted in internationalism, since the phenomena of the natural world transcend national boundaries. Nevertheless, scientists as people are located within a larger social structure, one that has been organized nationally since the rise of modern science. Scientists therefore have many roles that they must attempt to fulfill simultaneously. These obligations may be mutually exclusive, making it impossible for scientists to reconcile satisfactorily their professional identities as members of an international community of scholars with their identities as members of specific national communities. The study of science in the modern world is therefore well suited for exploring the tensions produced by the clash between national and international interests.²

Historian of science Loren Graham has written, “The study of the extreme is often thought to enlighten our understanding of the normal.”³ If he is correct, then the story of the German tobacco mosaic virus (TMV) investigators should be particularly enlightening, for these people experienced extreme change in both their national and international environments. As Germans, they experienced National Socialism and the lost war firsthand. Nazi science policy, as I shall demonstrate, was quite beneficial to them. They had a high level of intellectual freedom and were well funded by both government and industry, although the devastation of the war and the isolation from their colleagues abroad certainly compromised the quality of their work. After the war ended,

² This is a theme of Jeffrey Allan Johnson’s *The Kaiser’s Chemists: Science and Modernization in Imperial Germany* (Chapel Hill: UNC Press, 1990), esp. 201-209.

they were able to make the adjustment from dictatorship to democracy with ease, and the relative openness of the Federal Republic of Germany enabled them to succeed to a much greater extent than had the closed world of the Nazis. As members of the international community of science, they experienced the dramatic change of the molecular revolution in biology. Between the 1940s and the 1960s, investigators mapped out the precise mechanisms of biological heredity for the first time. The TMV researchers made several significant, even fundamental, contributions to this change in biological thought. My work therefore provides an unusually vivid historical example of how scientists cope with rapid social and scientific upheaval.

Since this study examines biological research as well as the broader social and cultural context in which this knowledge was produced and interpreted, I make use of a variety of source materials. For the content of scientific work, I have consulted the most relevant published scientific papers, review articles, and conference proceedings. Whenever possible, I have supplemented this public record with unpublished sources such as professional correspondence. Piecing together the social environment in which these researchers operated required the synthesis of many different published and unpublished sources. From archives in Germany and in the United States, I have examined sets of correspondence from some of the important figures as well as administrative and financial records for the pertinent research institutes. I have also used published and unpublished reports produced by government and related organizations concerned with science in the Federal Republic of Germany. Newspaper articles, speeches, and essays both by and about the TMV researchers form the rest of my contemporary source material.

To supplement these materials, I have also conducted interviews with many of the persons directly or indirectly involved in German biological research in the postwar era, including some of the TMV researchers. Naturally, interviews can be problematic as
primary sources in a historical study. Memories are not historical records, but rather contemporary reconstructions of past experiences. In my research, I have attempted to use them in this manner—as contemporary, expert interpretations of the events under study. Used in this way, my interview material has provided me with a wealth of insights and has helped me to organize my own ideas and interpret the events revealed in the documents. I am grateful to all of those who have generously given their time and thoughts to assist me in my research.

I have used the word “genealogy” in the title of this study to indicate the importance of interpersonal relationships for intellectual and professional success in the German TMV community, especially the bond between mentor and student. Senior researchers influenced the intellectual development of their students and were instrumental in helping the younger scientists secure academic positions. The patterns of friendship and patronage produced through the teacher/pupil relationship provided much of the coherence within the German TMV community. Since professional support was such an important element of these networks of relationships, I prefer the organic image of a genealogy to the more conventional image of schools of thought centered on a particular individual. As a metaphor, the familiar relations traced in a genealogy carry the entailment of inheritance, which in this case refers to the intellectual legacy handed down from instructor to pupil. Furthermore, families grow, take in outsiders, and interbreed. Sometimes individual members sever ties between one another. All of these behaviors characterized the intellectual and professional life of the TMV community. Finally, family as a metaphor also carries an entailment of support, care, and patronage. The senior researchers of the TMV group took this obligation very seriously. Intellectual training and professional support were essential parts of the nurturing of the careers of young scientists, and both inevitably marked the mature careers of these individuals.
Following these intellectual lineages is an integral part of understanding how the German TMV researchers functioned as a community of scholars.

1.1 The Nature of Science

1.1.1 Constructivism and its Limitations

Germany's scientific past has become adversarial in recent years at least partially because German scientists long tried to portray themselves as free of the burden of National Socialism. Like many scientists, they interpreted their research as a rational, apolitical, objective activity. According to this definition, the scientific excellence of the work produced in the research institutes of the Kaiser-Wilhelm-Gesellschaft proved the apolitical character of the scientists themselves and therefore absolved them of complicity in Nazi Germany's program of domination and genocide. However, as historians have developed a clearer picture of the behavior of German scientists during National Socialism, their conclusions have undermined the claims of neutrality professed by the scientific community. The tension in Germany regarding the political activity of scientists is illustrative of a problem that exists to varying degrees in all the scientifically advanced regions of the world: reconciling the accurate, useful knowledge that scientists produce with scientists themselves as human beings. The fact that scientific knowledge transcends cultures and societies so readily supports the idea that science is an apolitical quest for the truth, but it is an illusion that results from conflating the knowledge produced by science with the process of science itself.

Scientific knowledge is indeed objective in that it applies to all cultures in the world equally, but the environment that produces the knowledge and the ways in which it is interpreted and applied are both culturally specific. Data becomes scientific knowledge only when a significant segment of the relevant scientific community accept those data as such. The transition from speculation to established fact is often quite contested and occasionally subject to revision. Furthermore, the point of interpretation,
when accepted fact regarding the natural world becomes a strategy for understanding and
dealing with that world, is heavily laden with specific cultural values. Interpreting
science historically, as something that people in the past did, requires understanding the
entire process, from the culturally specific ways that individual researchers engaged the
natural world to the ways that the knowledge they produced became part of the culture of
their particular societies.

The first widespread, systematic effort to address the shortcomings of the vision
of science as an objective, apolitical enterprise began in the 1960s and is inextricably
bound up with the publication of Thomas S. Kuhn’s The Structure of Scientific
Revolutions. Kuhn argued that science does not proceed through the accumulation of
objective facts but rather in a discontinuous manner, through conceptual revolutions in
which old ideas were discarded when they ceased to be productive. For Kuhn, the
socialization and education of scientists was as important as nature of the phenomena
they studied. In the 1970s and 1980s, Structure became the most influential book on the
nature of science, having an impact and generating interpretations far beyond what the
author had originally intended.4 By providing an entirely new avenue for historians,
sociologists, and philosophers of science to pursue their inquiries, Kuhn’s book
precipitated a new way of understanding science, one that I shall loosely label
constructivist, as opposed to the objective realist position characteristic of many scientists
themselves.5

of Chicago Press, 1970). For Kuhn’s reflections on the impact of his work, see Thomas S. Kuhn, The Road

5 Here I take the objective realist position to be one that accepts the idea of the existence an independent
natural world, and that physical theories developed by humans can approximate certain elements of this
world. The goal of the physical sciences is then to develop theories that are applicable to the world to a
greater degree of approximation under more varied circumstances. While intuitive, realism as a
philosophical concept does have a number of shortcomings, not the least of which is the tendency to see
scientists themselves as being as objective as the natural world they study. See A.F. Chalmers. What is
In his recent study, Jan Golinski defined “a ‘constructivist’ outlook” as one “which regards scientific knowledge primarily as a human product, made with locally situated cultural and material resources, rather than as simply the revelation of a pre-given order of nature.” Obviously the term can and does mean many things to many people. When moderately presented constructivism serves as a corrective to the shortcomings of the objective realist position. However, some writers have developed an extreme interpretation of constructivism, one in which all knowledge claims are fundamentally the same. Viewed from this perspective, science becomes one way among many of interpreting the world, on a par with myth and religion. In such extreme interpretations, the social and cultural factors constraining science receive attention at the expense of the knowledge itself, which is understood as representing the special interests of scientists as a social group, not the natural world. Such studies have downplayed the

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7 An extremely relativistic form of social constructivist critique, generically referred to as postmodernism, has grabbed a great deal of attention in public years, and is regarded by many as representing the problems inherent in taking constructivism too far. This was demonstrated by the “Sokol Hoax” of the 1990s. Alan Sokol, a professor of physics, submitted an article entitled “Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity” to the avowedly postmodern journal *Social Text*, which published the article with minor revisions. The article was a hoax, which Sokol had written as a stream of gibberish pieced together from bits of borrowed jargon. Its publication generated a storm of controversy between scientists and their critics. The article is reprinted with a commentary and Sokol’s reflections on it, in Alan Sokol and Jean Bricmont, *Fashionable Nonsense: Postmodern Intellectuals’ Abuse of Science* (New York: Picador USA, 1998); for a broader perspective see the following collections of essays: Robert Klee, ed., *Scientific Inquiry: Readings in the Philosophy of Science* (New York: Oxford University Press, 1999), esp. Section 5 “Social Constructivism and Feminism,” 237-312; and Noreta Koertge, ed., *A House Built on Sand: Exposing Postmodernist Myths about Science* (Oxford: Oxford University Press, 1998). While the main point of Sokol’s critique is valid, some scholars have questioned the usefulness of his strategy—rather than bridge the gap between scientists and their critics, it has exacerbated tensions between them. See John Krige’s critical review “Cannon Fodder for the Science Wars,” in *Physics World*, December 1998, 49-50, as well as Sokol and Bricmont’s reply, “Postmodern Critics Hit Back,” *Physics World* February 1999.

8 There is no doubt that scientists are a special interest group that works collectively to ensure its funding and privilege in society; this was true of the German TMV workers and it is also true of scientists more
fact that scientific knowledge often does provide us with usefully accurate representations of the natural world.\textsuperscript{9} Most scientists, therefore, have been reluctant to accept this critique in any but the mildest form.\textsuperscript{10}

The history of the tobacco mosaic virus research group provides a case study in the genesis, work, and dissolution of a successful scientific community, which by example demonstrates that many factors were required to foster scientific excellence. Neither strong constructivism nor objective realism is sufficient to understand fully these investigators and their work. Social and cultural factors such as money, institutions, freedom, and communication were necessary, but not sufficient, as they interacted with empirical evidence, logic, and scientific rigor, which of course had their own limitations.\textsuperscript{11} Scientific knowledge, such as that produced by the TMV researchers, is therefore the product of a complex mutual interaction of limiting and enabling factors. Nevertheless, the fit between scientific theory and the world as observed can be extraordinarily precise. In place of absolute truth, science offers a systematic, non-

\textsuperscript{9} Golinski's generally sympathetic account does not subscribe to this extreme interpretation of constructivism; see pages xi, 8.

\textsuperscript{10} Furthermore, some scholars have noted that constructivism as a philosophy lacks the rigor necessary to produce a meaningful understanding of science because of its lack of reflexivity. That is to say, if one applies the principles of an extreme interpretation of constructivism to constructivism itself, it falls apart. The logic is as follows: if, as constructivism maintains, scientific knowledge is largely the product of local cultural and material resources, it stands to reason that the constructivist study of science is also the product of the local cultural and material resources of the constructivists themselves. It therefore offers no privileged viewpoint from which to examine science. See Golinski, \textit{Making Natural Knowledge}, 190. My interpretation is based largely on Ronald N. Giere's discussion of the problem of reflexivity for the philosophy of science in his \textit{Science Without Laws} (Chicago: University of Chicago Press, 1999), 20-21.

accidental correlation with reality. This correlation is what makes science so useful and effective. Alfred Gierer, reflecting upon a career as a scientist, much of it spent working on TMV, characterized science as follows: “Natural science as a cultural achievement shows in its history and inner structure a convergence of thought and reality, which binds human understanding with the hidden order of nature. However, it also shows the boundaries that constrain scientific thought and reminds us of the informal preconditions of knowledge about nature and humanity.”\(^{12}\) The quality of science depends upon the ability of its practitioners to approximate the external truths of the natural world in spite of the impossibility of complete, perfect objectivity.

1.1.2 Perspectival Realism

To understand the Tübingen TMV community historically, it is necessary to temper the social constructivist position with that of the realist position. Such a modified realism has been developed by the philosopher Ronald N. Giere, who argues for realism bounded by an awareness of individual human limitations, which he calls *perspectival realism*.\(^{13}\) According to perspectival realism, the natural world is infinitely complex and continually variable. Scientists can therefore engage only certain elements of the world at any given time; their own particular cognitive, spatial, and temporal perspectives limit their ability to grasp reality. The knowledge they generate is therefore not universal, but partial and imperfect. Scientific theories are human constructs that “fit” the world around us. The better the theory, the better the fit. Many scientists already envision their work in such a fashion.

Giere suggests that we can think of scientific theories as maps that help us to understand and find our way through the world around us. Maps are human constructs
that vary in detail, scale, and subject. Maps are always partial, and by their nature they are place specific. Yet they do represent something, so they can be understood realistically. They do not correspond perfectly with what they represent, but they are still exceedingly useful in helping us to get around. They help us to understand where we have been and to anticipate where we are going. Like maps, scientific theories allow us to understand the world around us. They allow for the anticipation or prediction of events, and through their incompleteness, indicate interesting new directions for further exploration.\footnote{Giere, Science without Laws, 81. Physicist John Polkinghorne has also discussed the connection between science and map-making. He has written: “Scientists are mapmakers of the physical world. No map tells us all that could be said about a particular terrain, but it can faithfully represent the structure present on a certain scale. In the sense of an increasing verisimilitude, of better approximations to the truth of the matter, science offers us a tightening grasp of physical reality.” John Polkinghorne, Beyond Science (Cambridge: Cambridge University Press, 1996), 8. Quoted in John Polkinghorne, Faith, Science, and Understanding (New Haven: Yale University Press, 2000), 79.}

1.1.3 History and Biology

Interdisciplinary collaboration shall be one of the most important themes of this study. Since both history and biology interact throughout this case study in the history of biology, I would like to use Giere’s image of science as a mapmaking process to provide a heuristic for understanding the relationship between these two disciplines. Many scholars recognize a significant difference between the natural sciences and humanities such as history. For example, Richard J. Evans, a historian, has recently argued that history is not a science because “It [history] can identify, or posit with a high degree of plausibility, patterns, trends, and structures in the human past. In these respects it can be legitimately be regarded as scientific. But history cannot create laws with predictive power.”\footnote{Richard J. Evans, In Defense of History (New York: W.W. Norton and Company, 1999), 52.} He himself does not define what science is, but rather takes the end product, predictive laws, to be representative of the process as a whole. He does not explore how
people create these laws, although his underlying assumption seems to be that simple laws must be the products of a simple process. For Evans, this is unlike history, which deals with life in all its messy, irreducible complexity. "Life," says Evans, "unlike science, is simply too full of surprises" to allow for simplicity and predictability.\textsuperscript{16} He implies that history is about life, and science is about something else.

As far as the natural sciences are concerned, biology, at least, is about life, and I would argue that there is much common ground between history and biology.\textsuperscript{17} As disciplines, they both study unique phenomena formed through the accumulation of chance occurrences over time. In Stuart Kauffman's eloquent phrasing, "History arises when the space of possibilities is too large by far for the actual to exhaust the possible."\textsuperscript{18} He was speaking of the evolution of biological molecules, but his words hold true for any

\textsuperscript{16} Evans, \textit{In Defense of History}, 53. On 62, Evans does go on to acknowledge that there are weaker sciences that cannot generate laws, and that history might be considered to be such a weak "science," in the sense of the German word \textit{Wissenschaft}.

\textsuperscript{17} Physics dominated the history and philosophy of science until recent decades. Consequently, much of the philosophy of science has been modeled on a scientific approach in which systems are isolated from their context, broken down into their simplest components, and analyzed with the intention of producing universal laws and very accurate predictability about the future behavior of the systems in question. This approach is not as useful in biology, where systems depend upon complex mutual interactions and cannot be isolated or dissected without destroying the phenomena under investigation. This had led many philosophers of biology to question whether or not physics and biology are both "sciences" in the exact same meaning of the word. Alexander Rosenberg has argued that there is a significant difference in the approach of these two disciplines. He begins with two interpretations of science—science as an enterprise that aims to reveal the structure of nature (realism), and science that provides a practical means through which humans control their environment (instrumentalism). He then argues that due to the complexity of biological systems the knowledge produced in biology is more a collection of heuristic devices and rules of thumb than universal laws. He therefore concludes that biology is relatively a more instrumental science than is physics. Alexander Rosenberg, \textit{Instrumental Biology or the Disunity of Science} (Chicago: University of Chicago Press, 1994), 1-16, and \textit{The Structure of Biological Science} (Cambridge: Cambridge University Press, 1985), 1-68. Also see Ernst Mayr, \textit{Toward a New Philosophy of Biology: Observations of an Evolutionist} (Cambridge MA: Belknap/Harvard, 1988), 8-21. Kenneth Schaffiner has argued that the biological sciences produce "theories of the middle range," which he describes as being between the universal laws of chemistry and the universal mechanisms of Darwinian evolution. He discusses the problems and literature regarding laws and generalization in biological explanation at length. Kenneth F. Schaffiner, \textit{Discovery and Explanation in Biology and Medicine} (Chicago: University of Chicago Press, 1993), Chapter 3, "Theories and ‘Laws’ in Biology and Medicine," 64-128.

system that has multiple possible outcomes at any given moment in its existence. Systems like these, including the human phenomena studied by historians, proceed through the elimination of alternatives and are therefore inherently historical. Max Delbrück, one of the founders of molecular biology and a physicist by training, believed that “every biological phenomenon is essentially an historical one, one unique situation in the infinite total complex of life.” “You cannot,” he said, “hope to explain so wise an old bird in a few simple words.” The methodology of biology therefore requires an element of historical narrative. Even in molecular biology, the most precise of the biological sciences, most discoveries were actually descriptions, their explanatory strategies narrative. While this approach differs from philosophies of science that take physics as their model, it may be the only appropriate approach for explaining unique occurrences.

The human process of science, detailed at length throughout this study, should seem familiar to those with knowledge of the profession of history, and the knowledge claims produced by both are cognitively similar. The greatest distinction between the two is in their respective subjects of inquiry. History is to the past as biology is to nature. Both the human and natural worlds consist of unique events that have developed over time, but the human world has the added complication of self-conscious choice shaping and driving it, while the selective processes of the biological world

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20 Ernst Mayr, This is Biology: The Science of the Living World (Cambridge: Belknap/Harvard University Press, 1997), 113. Kenneth Schaffner is more critical of historical explanations, noting their importance but describing them as “data-deficient” in relation to explanations from physics and chemistry. I see the difference between these explanations as resulting from an excess, rather than a lack, of data in phenomena that must be explained historically. Again, Schaffner provides a stimulating and through discussion of the problem and the literature dealing with it. See Discovery and Explanation in Biology and Medicine, Chapter 7, “Historicity, Historical Explanations, and Evolutionary Theory,” 325-361.

21 I owe the phrasing of this particular insight to Alan Beyerchen, who is fond of saying, “History is to the past as science is to nature.”
depend upon a combination of physical properties and chance. Self-awareness coupled with chance and contingency, make the phenomenon studied by historians even more indeterminate than those studied by biologists, and historians’ conclusions are accordingly more tenuous.

To return to Giere’s ideas, the difference between history and biology is in the scale of the maps that they produce. Biology, particularly molecular biology and biochemistry, produce very fine-grained, detailed maps about relatively small areas. For example, several of the German TMV investigators spent decades studying a cylindrical virus roughly three one-hundred-thousandths of a millimeter (roughly 3000 Å, or $3 \times 10^{-7}$ meters) in length. The resolution of discoveries, or “maps,” such as these, is therefore exceedingly sharp. One can liken them to blueprints—very detailed and specific, but lacking in context. Whole organism biology might be seen as a street map of a specific area, lacking some of the details of the former, but in compensation showing the interrelationships of smaller scale objects. Subjects like zoology can then be seen as city maps, large enough to create a coherent pattern lost in the fine details of the former two. Historical maps then become much broader and correspondingly course grained. They show continents and oceans—on them, the teeming complexity of the cities appears as mere dots. The usefulness of each map depends upon the needs of the explorer—a blueprint will not help one cross the country, and a globe will not help one find an address two blocks away. It also, of course depends upon its relationship to reality. Poor maps are as useless as biological discoveries that are not empirically verifiable or historical narratives that do not examine sources rigorously and critically.

The empirical rigor of scholarship unifies the disciplines across their varying scales of complexity, and the mapping heuristic suggested by Giere arranges them in a logical, non-hierarchical manner. The scholarly disciplines are not subordinate to one another, but rather nested seamlessly within one another. Crossing from one to the other
can be rather easy; for example, the exact point at which the biology of human evolution ends and anthropology, archaeology, and ancient history begin, is impossible to draw with precision. This is an interpretation of the relationship of the scholarly disciplines that I believe is truer to the meaning of the German word *Wissenschaft*, which is often translated as "science" when it would be more accurately translated as scholarship. In developing these ideas, I have been influenced by the philosophical and historical works of Alfred Gierer as well as others of the TMV community. I therefore believe that the interpretation of science I have developed here corresponds very closely with that of the TMV researchers, and is therefore an important pre-requisite for understanding these men and their careers. It is a story of people making choices, constrained by the complexities of both the human and the natural world.

1.2 Historians and German Biology

1.2.1 Science and National Socialism

Since the late 1970s, the scholarly interest in science in Nazi Germany has exploded, producing a very thorough and searching body of historical literature. To situate this study historiographically, I shall begin by reviewing recent surveys of the literature by leading scholars. I identify three promising points of departure for further research—the behavior of scientists, the shortcomings of a political chronology for the history of science, and the importance of the institutional setting of science, and then describe how I pursue these lines of inquiry in my own research and discuss the literature that has helped me to do so.

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22 In this study, I have often translated *Wissenschaft* as "science" simply to follow convention; for example, I translate Max-Planck-Gesellschaft zur Förderung der Wissenschaften as "Max-Planck-Society for the Advancement of Science" because that is the usual translation. A glance at the actual institutes in the society—it contains institutes for history and the history of science—suffices to confirm that the translation "Max-Planck-Society for the Advancement of Scholarship" is a more appropriate translation.
In his 1992 review, "What We Now Know About Nazism and Science," Alan Beyerchen noted that recent scholarship had shown that the Nazi government consistently supported excellent scientific research, and that this support (measured in financial terms) increased throughout the war years.\(^{23}\) He also found that this very promising trend was often marred by the preoccupation of historians with assigning blame to historical actors or meting out scholarly justice. A natural complement to such an agenda has been the identification of heroic resisters, resulting in a simple moral dichotomy that fits poorly with the complex character of scientific research as revealed by empirical historical investigation. Other historians, including Mark Walker and Jonathan Harwood, have agreed that such a "saints or sinners" historiography is likely to sidestep the important task of explaining why scientists responded (or did not respond) to National Socialism in the manner in which they did. In a more recent review, Harwood summed up the concerns of historians of German science by saying "Their point is, instead, that an explanatory historiography is essential if we are to learn the political and moral lessons of the Nazi episode. It would be a sign of this field's maturity if the next decade of research were to move in this direction."\(^{24}\)

In a different review essay, Mark Walker noted the persistence of another trend in the scholarly literature, the tendency to compartmentalize the history of German science within a political chronology. This has the unfortunate effect of obscuring continuities

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\(^{23}\) Alan Beyerchen, "What We Now Know About Nazism and Science," *Social Research* 59 (1992): 624-628. Since Beyerchen's essay was published, further research has contributed to our image of a Nazi Germany that was very friendly to scientific research. For an overview, the interested reader should see the following collections of essays: Margit Szollosi-Janze, ed., *Science in the Third Reich* (Oxford/New York: Berg Publishers, 2001); both published volumes of the MPG Presidential Commission, *Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus: Bestandsaufnahme und Perspektiven der Forschung*, ed. Doris Kaufmann (Göttingen: Wallstein Verlag, 2000), as well as the ongoing publications of the Commission; and Monika Renneberg and Mark Walker, eds., *Science, Technology, and National Socialism* (Cambridge: Cambridge University Press, 1994).

between Weimar Germany and Nazi Germany, or between Nazi Germany and the Federal Republic. Walker wrote, "Separation of the Third Reich from what came before it and from what followed it can facilitate or perpetuate the simplistic model of a 'golden age' of German science before 1933, a heroic reconstruction after 1945, and an unfortunate and atypical interval in between." Such is often the case even if it is not the author's intention. Walker suggests that the best way to "normalize" National Socialism as a topic in the history of science is to transcend the typical boundaries of 1933 and 1945.

Jonathan Harwood has suggested a third direction for future research, one that focuses on the institutional context of science. To explain the reasons for excellence in scientific research in Imperial Germany, Harwood focused on what he calls Third Sector Institutes, that is, research institutes that were neither wholly academic nor industrial in character, such as the Kaiser-Wilhelm/Max-Planck-Gesellschaft. For him, the interesting question is the kind of impact these institutes had on the modernization of Germany. This question becomes even more interesting if one combines it with Walker's advice to carry the history of German science past 1945. Given that the Federal Republic of Germany became an economic powerhouse dependent upon the export of manufactured goods, to what extent did West German biological science, especially that of the Max-Planck-Gesellschaft, contribute to the Economic Miracle or the modernization of West German society?


This study endeavors to address all three of these pressing issues. First of all, I have attempted to produce an explanatory history of biological research in twentieth century Germany, one that is less concerned with evaluating blame than with the larger narrative by means of understanding the actions of the scientists. The people at the heart of this study were in fact neither saints nor sinners. For them, as for most people, the choices they made in the years 1933-1939 regarding the Nazi government were not nearly as clear as they appear in the retrospective analysis of the historian. Explaining that these choices were logical and even sensible, given the social climate of the time, does not imply that they were the correct choices to make in a universal, moral sense. Yet, if they were the wrong decisions, they were wrong because of the local, selfish perspective of the individuals making them, not because they were rooted in an exterminationist desire to conquer the world. They were the kind of “wrong” decisions that characterize everyday life. One of the important findings of this work is that the exact same pattern of decision making that allowed scientists to work with the Nazi government prevailed when they worked in the Federal Republic. In this sense, there was nothing inherently Nazi about the TMV program.

Recent biographies of important scientists have begun to illustrate the difficult choices presented to individuals who were less than enthusiastic about the Nazi Party. John Heilbron’s study of Max Planck is especially interesting, as it illustrates how Planck’s feelings of duty toward his science and the German state led him to remain at his post even though he had strong personal misgivings. David Cassidy’s biography of Werner Heisenberg shows that the polycratic nature of the Nazi state created an ambiguous relationship with the scientific elite. Individual scientists who were threatened could usually find sponsorship in another quarter, with deeper ties to the
regime ironically resulting from this initial desire to maintain a degree of independence. Speaking of scientists more generally, Herbert Mehrtens has written that the idea of professional values, particularly the vision of science as a pure, apolitical activity, allowed scientists to distance themselves morally from the Nazi regime, claiming independence while in reality they depended upon it and lent it credibility and prestige. In his research on the aerospace industry, Helmuth Trischler develops the idea of the "self-mobilization of science." In Imperial, Weimar, and Nazi Germany, scientists combined a strong feeling of responsibility toward the state with the expectation that the state would support them in return. Budget cuts during the Weimar period disillusioned scientists to an extent, and so when the Nazis demonstrated that they were willing to support scientific research, the scientists responded enthusiastically. There was no need for the political mobilization of scientists.

Since biological heredity was at the heart of the TMV research, it is important to note that it was not motivated by the racial ideology of Nazi Germany as a whole and that the scientists did not use their research to support eugenics or racial discrimination. Since TMV was a plant virus, the researchers sought support from the state by presenting

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27 John L. Heilbron, *The Dilemmas of an Upright Man: Max Planck as Spokesman for German Science* (Berkeley: University of California Press, 1986); David Cassidy, *Uncertainty: The Life and Science of Werner Heisenberg* (New York: Freeman, 1991). As the titles of the two books show, Heisenberg’s relationship with the regime was much more ambiguous than was Planck’s. Very recently (February, 2002) the family of Niels Bohr have published family documents regarding Heisenberg’s visit to Bohr during World War II that show that Heisenberg’s dedication to the German atomic weapons program was stronger than many had previously thought. The publication of the letters sparked an interesting discussion in the *New York Review of Books*, 28 March, 2002.


their work as agricultural work that would contribute to the achievement of self-sufficiency in food and fodder crops. The lack of agricultural self-sufficiency had become a crucial problem during World War I, when Germany was unable to produce enough food to feed its population adequately. After the war, achieving independence in agriculture became a national policy objective. TMV research thus was part of a long-standing national security issue that became a central tenet of military expansion under the Nazis. This finding is not incompatible with the work of scholars that details the participation of the biological and social scientists in Nazi programs.\(^{30}\) The researchers to be discussed here certainly knew of Nazi atrocities and worked side-by-side with enthusiastic participants—there was overlap between different scientific groups. There is at this point, though, no evidence indicating that the TMV researchers used the results of their work to lend scientific credence to sterilization, euthanasia, or extermination.\(^{31}\)


\(^{31}\) The work of the TMV researchers makes an interesting contrast with that of bacteriologists who identified Jews with typhus and used the eradication of the illness as a justification for eradicating the Jews who were supposed to harbor it. This clear example of microbiology research supporting the racial view of Nazi Germany is the subject of Paul Weindling’s Epidemics and Genocide in Eastern Europe, 1890–1945 (Oxford: Oxford University Press, 2000). In her study of biology under Nazism, Anne Bäumer has found considerable variation in the responses of biologists toward the regime. At the one extreme were enthusiastic supporters of the regime who hoped to create a kind of “German biology” analogous to the effort by Philipp Lenard and Johannes Stark to create a “German physics.” At the other end were researchers whose work was uninfluenced by Nazi ideology. Anne Bäumer, NS Biologie (Stuttgart: Wissenschaftliche Verlagsgesellschaft mbH, 1990).
The second historiographic issue that my work addresses is chronology. My beginning and end points are based upon events in the history of the German TMV community, not the political history of Germany. The group was founded in 1937 as the result of a confluence of unpleasant events. The death of one director and the dismissal of two others on "racial" grounds brought three broad-minded researchers to the Kaiser-Wilhelm-Gesellschaft in Dahlem at the same time that groundbreaking TMV research was occurring in the United States. Given an opportunity to extend their work in new directions, the three decided to pursue the virus research that had been started in the US, and in 1937 they launched one of the most ambitious virology programs in the world. My end point of 1972 is somewhat more arbitrary. By that time, several of the older TMV researchers had died, many of the younger researchers had left the group for other positions, and TMV research had all but ceased in the Max-Planck-Gesellschaft.

I know of no other study that examines a research program that began in Nazi Germany and proceeded unchanged into the Federal Republic. The aerospace industry studied by Trischler was in place well before the Nazis. Robert Proctor's fine study The Nazi War on Cancer, clearly shows that the Nazi regime encouraged first-rate biomedical research and that the results produced stand up even today. Proctor does not follow this work into the postwar era, although undoubtedly the continuities in personnel and research programs were significant. The few studies of the postwar era that exist, such as those by Thomas Stamm and Maria Osietzki, focus on the establishment of the West German research infrastructure after the war. The few cultural histories of science in West Germany, such as Cathryn Carson's study of Werner Heisenberg's postwar career,

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are clearly intended to analyze the public role of science in the Federal Republic, rather than to track a continuous research program to which 1945 was largely irrelevant.\textsuperscript{34}

Of the scholars who have written on the subject of science under National Socialism, the one whose work most closely intersects with mine, both in subject matter and chronological scope, is Ute Deichmann. Deichmann was the first to show that quality biological research continued in the Third Reich and that government support for research increased steadily throughout the war. She has concentrated her attention on the Third Reich but has also tackled the question of how Nazism affected biological research in the Federal Republic of Germany.\textsuperscript{35} My research pursues a similar agenda, only with the chronological priorities inverted.

Since the bulk of this study examines biological research in the Federal Republic of Germany, its conclusions contribute to our understanding of the third issue, posed by Harwood—the extent to which nonacademic research institutes served as progressive forces in German society. In his own work on the German genetics community before Nazism, Harwood argues that the kind of science practiced in a given institute—the problems chosen and the techniques used to explore them—depended at least partially upon institutional structure and available resources.\textsuperscript{36} In particular he noted how cultural

\textsuperscript{34} Cathryn Leigh Carson, "Particle Physics and Cultural Politics: Werner Heisenberg and the Shaping of a Role for the Physicist in Postwar West Germany," (Ph.D. diss., Harvard University, 1995). Also see Richard H. Beyler, "From Postivism to Organicism: Pascual Jordan’s Interpretations of Modern Physics in Cultural Context," (Ph. D. Diss., Harvard University, 1994). Research on anthropology and medicine in the postwar era has usually been linked to its involvement in the Nazi government; for example, see Hans-Peter Kröner, \textit{Von der Rassenhygiene zur Humangenetik: Das Kaiser-Wilhelm-Institute für Anthropologie, menschliche Erblehre und Eugenik nach dem Kriege} (Stuttgart: Gustav Fischer, 1998). For psychiatry, see Mitchell Ash, \textit{Gestalt Psychology in German Culture, 1890-1967: Holism and the Quest for Objectivity} (Cambridge: Cambridge University Press, 1995).


differences between Germany and the United States shaped research institutes and therefore research programs, allowing for much more narrow specialization in the US, while German geneticists tended to define the field more broadly.

Such a comparison works equally well for different kinds of institutes within Germany. The same tendencies in the German university system that Harwood noted persisted into the postwar era, making the university research style very different from the style of research practiced in the United States or even in Germany’s Max-Planck-Gesellschaft. In the universities, disciplines were broadly defined, yet boundaries were strictly enforced, making the cooperative networking of specialized researchers that was vital for American success difficult. Recognizing the situation, many German biologists wished to “modernize” their own research infrastructure by making similar it to that of the United States.\textsuperscript{37} For them, modern biology and America had become synonymous. This effort at fundamental reform was not undertaken grudgingly, but instead optimistically, in imitation of a system that many German biologists found to be superior to their own, similar perhaps to the American emulation of German universities in the late nineteenth century.

1.2.2 The History of the History of Molecular Biology

The history of molecular biology, a relatively new branch of the history of science, has already produced an impressive body of critical scholarship. In the late 1960s and early 1970s, historians and scientists themselves began by focusing on the contributions to the new biology made by physicists or through techniques imported from physics. Max Delbrück, a German émigré physicist who took up biology in the 1930s, and the group surrounding him, received much of the attention in this early work, which

\textsuperscript{37} In this sense, the researchers used the term “modern” to mean “contemporary,” or even “progressive,” the association with America was so strong because American university departments, by the 1950s, were often much more diverse and collegial than their counterparts in Europe.
consequently tended to slight the contributions made by workers in biochemistry to the emerging field of molecular biology.\textsuperscript{38} Among historians, this perspective was stated most forcefully by Robert Kohler, who wrote, “Few biochemists were interested in the great unsolved problems of biology, and fewer still had any sympathy with the swashbuckling molecular biologists who swept down in the 1950s and carried off the richest prizes in molecular genetics, protein synthesis, and cell physiology. As chemists, biologists, bacteriologists, and others began to intrude on their turf, biochemists began to act like embattled defenders of a conservative faith.”\textsuperscript{39} This interpretation became rather powerful for a time, despite the fact that the two best known histories of molecular biology, both of which appeared in the 1970s, acknowledged the importance of biochemistry.\textsuperscript{40}

A complementary narrative that examines the importance of biochemistry has emerged in the 1980s, providing a more nuanced interpretation of the development of molecular biology.\textsuperscript{41} Historians have now addressed the contributions of biochemistry to the understanding of nucleic acids and proteins. In addition, entire fields that were seen

\textsuperscript{38}The trend of seeing molecular biology as exclusive of biochemistry in general began with the collection of essays in \textit{Phage and the Origins of Molecular Biology}, all of which were written by admirers of Max Delbrück in honor of his 60th birthday. Reviews of the book confirmed this; see John Kendrew, “How Molecular Biology Started” and Gunther Stent, “That was the Molecular Biology that was.” Both are included in the expanded edition of \textit{Phage and the Origins of Molecular Biology}, 343-344, and 345-350, respectively.


as molecular biology in the 1960s, but were initially ignored by historians, have begun to attract attention in the scholarly literature as well. These fields include photosynthesis, protein synthesis, and immunology.42 In fact, in recent years, complexity seems to have become the defining characteristic of molecular biology in the secondary literature, so much so that Lily Kay has suggested that it is no longer possible to conceive of a single history of molecular biology:

Given these observations, we might have to come to terms with the realization that there is not one grand narrative of the history of molecular biology, no single privileged Archimedean vantage point from which to tell the scientific story. Instead, we may have to embrace the multiplicity of perspectives and methodologies that collectively illuminate different aspects of a historical manifold as complex as molecular biology.43

The example of the Tübingen TMV research supports such an interpretation of molecular biology, for the participants worked in a molecular biology that was characterized by the importance of crossing boundaries, both disciplinary and national. Most of the TMV work can be seen as biochemical in nature, and therefore reinforcing the importance of biochemistry in the history of molecular biology. Nevertheless, it was biochemistry complemented with botany and physics, turned toward genetic problems, and dedicated toward generating results that would be applicable to all biology. The cooperative networking of specialists characterized the TMV community just as it did the


rest of the postwar biomedical community. After 1956, the diverse researchers in the German TMV community were unified by a common problem, the Genetic Code, more than anything else. The Genetic Code, the correlation between proteins and genetic material, is itself an element of the history of molecular biology that has only recently been given proper attention as part of the diversification of molecular biology scholarship.

In most histories of molecular biology, scientists from the Federal Republic of Germany are conspicuous by their absence. Investigators working in the United States and Britain, and to a lesser extent in France, dominate the historiography. There are two explanations for the lack of a strong German presence. First, the contributions of the other three countries were greater than those of the Federal Republic by whatever criteria for judgment one might choose. The explanation for this disparity will be developed in the body of this study, but for now it is sufficient to note that that lack of attention paid to West Germany by historians reflects a relative lack of scientific productivity in the Federal Republic. The second reason relates to the issue of chronology in the history of German science. As we have seen, because of the importance and challenge of dealing with the Third Reich, few historians of German science have pursued their subject into the postwar era, when the most significant breakthroughs in molecular biology were made. Thus there are two communities of scholars whose work relates to my study, but to this point, other subjects have drawn the attention of both. My study of German TMV

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research therefore fills a gap in both of these bodies of literature and serves as a synthesis, tying the history of Germany to the history of molecular biology.

An interesting element of this community that I have not explored in depth is that of the laboratory itself. A wealth of recent scholarship has revealed the ways in which the materials and tools of laboratories shaped not only the process of knowledge creation within the lab, but also had an influence in the broader environment as well. Social, political, and cultural forces outside the lab do not determine experimental practice inside the laboratory; instead there is a much more dynamic interaction between all of these elements.\textsuperscript{46} The insights of the material culture approach have already been brought to bear on TMV in Angela N.H. Creager's \textit{The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930-1965}.\textsuperscript{47} Creager's work provides a fine-grained analysis of TMV as a model system for biological study, and, unlike many other historians of the biological sciences, she takes full note of the German contributions. Her book therefore provides a complement to my own work.

\subsection*{1.2.3 Biological Research in the German Democratic Republic}

As the title indicates, this study examines research done in the Federal Republic of Germany, although the story does of course include the prewar roots of the TMV research program as well. The decision to exclude the German Democratic Republic from my study was not made arbitrarily. In 1943, when the Allied bombing campaign

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began to reduce the city of Berlin to rubble, most of the scientific institutes of the Kaiser-Wilhelm-Gesellschaft were evacuated to relatively safe areas. From the pattern of evacuation (see figure 1.1), it is clear that the air war was not the only concern of the officials planning the evacuation; every single institute was evacuated westward, out of the path of the advancing Red Army. The two Kaiser-Wilhelm-Institutes most directly involved in the TMV research, those for biology and biochemistry, were evacuated to the city of Tübingen in southwest Germany, which survived the war relatively unscathed. The most important biological research institute to remain in Berlin was the Institute for Brain Research in Berlin-Buch, which became a focal point for biomedical research in East Germany.48

In addition to the loss of the institutes of the Kaiser-Wilhelm-Gesellschaft, there were several other factors unique to the German Democratic Republic that discouraged the pursuit of quality molecular biology research. The other research institutes that remained were for the most part affiliated with the universities of the Soviet Zone; these institutes quickly became the loci of indoctrination of the East German Communist Party, the SED. As early as April, 1947, a party functionary summarized the goals of university education as "...promotion of worker and peasant studies, eliminating fascist teachers and students, political schooling of students."49 There was a dramatic purge of the faculty, especially in the natural sciences, in the postwar years. In the summer of 1947, only 15.8 per cent of professors in mathematics and the natural sciences who had been active in 1944 were still teaching at universities in the Soviet Zone. After 1948, former NSDAP

48 See Chapter 2 for more on the institute in Berlin-Buch.

Figure 1.1: Evacuation of Kaiser-Wilhelm-Instituts Westward

Source: Kurt Ueberreiter, A Statistical Postwar Survey on the Natural Sciences and German Universities, (Library of Congress, European Affairs Division, 1950).
members were permitted to resume their teaching possibilities on a limited basis, but many of the best chose to defect to the West prior to the building of the Berlin Wall.\textsuperscript{50}

A second problem confronting East German biologists in the 1950s was Lysenkoism. Trofim Denisovich Lysenko was a Soviet biologist who argued that the hereditary characteristics of living things, agricultural crops in particular, could be influenced by environmental factors, contradicting the central tenets of Mendelian genetics as understood in the rest of the world. In the late 1930s, Lysenko adroitly managed to fit his own view of biological heredity with dictator Josef Stalin’s political and economic visions. Lysenko suggested that he could increase Soviet agricultural productivity by exposing wheat seeds to conditions of cold and humidity prior to planting them, arguing that this would make the mature plants better able to tolerate the less than ideal growing conditions that prevailed in much of Soviet Russia. He therefore offered Stalin a way out of the agricultural crisis that had afflicted Soviet Russia since the beginning of agricultural collectivization. Beginning in the late 1930s, Lysenko’s scientific views became the only accepted theory of biological inheritance permitted in the Soviet Union and its satellite states.\textsuperscript{51} Classical genetics ceased to be taught in much of the Eastern Bloc in the 1950s. Even if they had previous training in genetics, biologists were required to state publicly their agreement with Lysenkoism, regardless of whether or not they believed it (many did not).\textsuperscript{52}


\textsuperscript{52} For example, Erhard Geissler, a respected virologist from the DDR, recalls that during his training at Leipzig University from 1950 to 1954, there were no classes on genetics offered. He did not receive formal
Eventually, the failure of his agricultural programs to produce results led to Lysenko's fall from grace in the early 1960s, though not before he had received additional support from Stalin's successor Khrushchev. At that point, the Soviet government lifted formal restrictions on teaching genetics and the prospects for biological research brightened. Nevertheless, difficulties remained for biological researchers in Eastern Bloc countries such as the German Democratic Republic. The fortified boundary between East and West, manifested in extreme form in the Berlin Wall, made international travel exceedingly difficult. Even when East German researchers received permission to travel abroad, their lack of access to hard currency made them dependent upon their host countries for support. Access to American and Western European scientific literature was limited as well.\textsuperscript{53} However, since the Party no longer had an official line on genetics or genetic engineering, researchers could discuss these issues quite freely in East Germany in the 1960s and 1970s, making blanket generalizations difficult to sustain.\textsuperscript{54} Clearly, the history of biological research in the German Democratic Republic is a very complex subject that is only now beginning to receive the sustained scholarly inquiry it merits.\textsuperscript{55}

\textsuperscript{53} Erhard Geissler to Max Delbrück, January 2, 1970. Earlier, Delbrück had provided Geissler with nearly a decade's worth of bound copies of \textit{Proceedings of the National Academy of Sciences}, one of the most significant American scientific publications. Geissler was unable to receive it in East Germany through a normal subscription. Max Delbrück to Erhard Geissler, November 21, 1967. Both letters Max Delbrück Papers, Carton 9, Folder 3.


1.3 The Study of Biological Heredity in the Early Twentieth Century

1.3.1 Classical Genetics

The desire to understand the principles underlying the patterns of biological inheritance in humans and other organisms is as old as human society itself. The domestication of plants and animals, which made settled human communities possible, were the successful results of controlled breeding experiments carried out empirically over millennia. Efforts to turn this empirical practice into a systematic body of critical knowledge date back to the Greeks, but it was not until the mid-nineteenth century that an investigator articulated the underlying patterns of inheritance as general rules.\(^{56}\)

The modern study of inheritance, genetics, began in 1900 when three researchers, Hugo de Vries, Carl Correns, and Erich von Tschermak-Seysenegg, working independently and roughly simultaneously, encountered phenomena similar to those discussed by Gregor Mendel in 1866. For the next decade, the study of inheritance was usually called Mendelism, but by 1910, the new term genetics (coined by William Bateson in 1906) became more common. In 1909, the Danish geneticist W.L. Johannsen provided the new science with much of the terminology that is still in use today. Johannsen suggested using the term gene for the unit of heredity. The gene as he imagined it, however, was different from our contemporary conception of the gene. Johannsen did not believe that genes were actual physical entities. For him, genes were a mental tool, a calculating unit (Rechnungseinheit) to be used by geneticists in the analysis of data.

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of hereditary factors. After 1910, the laboratory of Thomas Hunt Morgan dominated genetics. Beginning at Columbia University and then moving to Caltech, Morgan’s group developed the most valuable experimental system ever used in biology, the fruit fly *Drosophila melanogaster*. Breeding experiments using specific mutations as markers allowed Morgan and his co-workers to make a number of significant breakthroughs by the early 1920s.

One of the most important legacies of the Morgan school was to establish chromosomes as the cellular locations of genes, making chromosomes subjects for genetic research. Chromosomes are structures that appear in the nuclei of cells shortly before cell division, then segregate and separate when the cell itself divides. During the rest of the cell cycle, chromosomes exist as long filaments distributed throughout the nucleus; these filaments were not detectable through ordinary light microscopes, and many believed that chromosomes actually dissolved after cell division and were then created anew prior to the next division. By the early 1900s, researchers established the constancy of chromosomes largely through inference rather than direct proof. For example, they noticed that during the process of meiosis (the formation of sex cells with half the normal complement of chromosomes) chromosomal behavior followed the same patterns as did traits of Mendelian inheritance. As early as 1915, Morgan and his colleagues made the connection between genes and chromosomes explicit. This did not

57 W. L. Johannsen, *Elemente der Exakten Erblichkeitslehre* (Jena: Gustav Fischer Verlag, 1909); see discussion in Mayr, *Growth of Biological Thought*, 736-737. Johannsen also introduced the important terms genotype, meaning the entire genetic makeup of an organism, and phenotype, its physical appearance.


mean that they were any closer to understanding the physical nature of the gene, however; even in the mid-1920s Morgan described genes as locations on chromosomes but admitted that the chemical composition of genes was still unknown and that they might not even be distinct chemical molecules.\textsuperscript{60}

1.3.2 Biological Chemistry

Investigating the physical nature of genes would eventually require the chemical analysis of their constituent substances, proteins and nucleic acids. Beginning in the nineteenth century, these molecules (as well as others related to the physiological processes of life) had served as a point of overlap between the fields of biology and chemistry. By late in the century, organic chemists had become so adept at the analysis of complex molecules that a number of organic chemists began to turn their attention toward applying their laboratory techniques to living tissues and processes. The result of this synthesis was a second new discipline that emerged in the early twentieth century at roughly the same time as Mendelian genetics. Originally called physiological or biological chemistry and housed in medical schools, the field has since been institutionalized as biochemistry.\textsuperscript{61}

Researchers have associated the large, complex molecules that we today call proteins with life since these molecules were first recognized. Proteins were commonly described as the building blocks of life, and this was accurate—they do structure all living tissue. But proteins have additional functions as well. Proteins serve as chemical

\textsuperscript{60} Morgan concluded his 1926 work on the gene by writing “When all this is given due weight it nevertheless is difficult to resist the fascinating temptation that the gene is constant because it represents an organic chemical molecule. This is the simplest assumption one can make at present, and since this view is consistent with all that is known about the stability of the gene it seems, at least, a good working hypothesis.” T.H. Morgan, \textit{The Theory of the Gene} (New Haven: Yale University Press, 1926), 310. See the discussion in Mayr, \textit{Growth of Biological Thought}, 769-775.

\textsuperscript{61} For general histories, see Kohler, \textit{From Medical Chemistry}, and Fruton, \textit{Proteins, Enzymes, Genes}. 39
messengers (hormones), they transport and store important substances such as oxygen, and as enzymes, they catalyze the chemical reactions necessary for life.

The term protein was coined in 1838 and was originally used to designate a hypothetical basic chemical formula thought to be shared by many different tissues. Despite this initial inaccuracy, investigators continued to use the term and it was eventually used to describe a diverse set of substances, all containing sulfur and phosphorous, that were found in tissues and biological fluids. By the second half of the nineteenth century, many chemists believed that proteins were the primary component of the “protoplasm” that made up living cells. Some went so far as to postulate that proteins contained the vital energy of life itself and that there were differences in the proteins of living and dead tissue. All agreed that the complexity of these substances defied contemporary methods of chemical analysis.

At the time, the standard method of laboratory analysis was to decompose compounds through exposure to chemical agents and then to identify the products. When proteins were subjected to such analysis, they consistently decomposed into smaller molecules called amino acids. By the turn of the century, chemists had tentatively identified many different amino acids, but by the late 1930s, more reliable techniques had pared this number down to roughly twenty amino acids that were commonly found in proteins.

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62 A much more thorough discussion of this topic can be found in Fruton, Proteins, Enzymes, Genes. The notes that follow are simply meant as reference points to direct the interested reader to the appropriate sections of the book. Robert Olby’s The Path to the Double Helix is more specific, focusing quite naturally on the developments that led to the Watson-Crick model of DNA, but he does provide valuable background on the way in which researchers conceptualized proteins and nucleic acids early in the twentieth century. Graeme K. Hunter’s Vital Forces: The Discovery of the Molecular Basis of Life (San Diego: Academic Press, 2000), is an accessible and reasonably accurate introduction to the topic that is marred, however, by an almost complete lack of documentation of source materials.

63 Fruton, Proteins, Enzymes, Genes, 171, 161, 166.

64 Ibid., 179, 182-183. In 1986, a 21st amino acid was found, and very recently (May, 2002), a group of investigators from the Ohio State University announced that they had identified a 22nd amino acid. See B. Hao, W. Gong, T.K. Ferguson, C.M. James, J.A. Krzycki, and M.K. Chan, “A new UAG-encoded residue
Still, it was by no means clear how amino acids assembled to form the larger proteins. In 1902, the German organic chemist Emil Fischer proposed that a bond linking the carboxylic acid group of one amino acid to the amino group of another, which he called a peptide bond, was the fundamental linkage within the individual protein molecule. He therefore began to refer to chains of linked amino acids as polypeptides in order to reflect the numerous peptide bonds holding them together.\textsuperscript{65} Despite his suggestion, great uncertainty still surrounded the structure of the complete protein molecule. The size of proteins remained unknown until the late 1920s, when experiments using new experimental equipment such as the ultracentrifuge showed that they were truly immense molecules. At that point, the atomic weight of some proteins was determined to be in the tens of thousands, implying that each protein itself was composed of hundreds, if not thousands, of individual atoms.\textsuperscript{66} Clarifying the arrangement of individual amino acids within these immense molecules was a task that eventually required decades.\textsuperscript{67} In fact, many of the most highly recognized achievements in molecular biology after the Second World War—Linus Pauling’s discovery of the alpha helix motif, Frederick Sanger’s sequencing of insulin, and Max Perutz’s determination of

\textsuperscript{65} A more detailed discussion of protein structure and the peptide bond is given below in Chapter 3.

\textsuperscript{66} The atomic weight (the term is usually used interchangeably with the term atomic mass) of an atom gives an approximation of the number of protons and neutrons contained within; the atomic weight of each is very close to one. The atomic weight of a molecule is the sum of the atomic weights of the atoms contained within. Since the atomic weights of most common atoms are in the single or double digits, the immense weights of protein molecules implied that they contained a great many atoms.

\textsuperscript{67} Fruton, \textit{Proteins, Enzymes, Genes}, 186-201; also see Olby, \textit{Path to the Double Helix}, Chapters 3 and 4.
the three-dimensional structure of the protein hemoglobin through X-ray crystallography—were contributions to understanding the structure of proteins.

Another important development of the 1930s was the association of proteins with biological inheritance. In the early 1800s, investigators isolated chemical substances that catalyzed important biological reactions, such as digestion, the conversion of starch to sugar, and the conversion of sugar to alcohol. These substances were initially designated ferments, although by the early twentieth century they were more often called by their current name, enzymes. The ability of enzymes to catalyze, that is, to facilitate biological reactions without directly participating in them, earned them an important place in the search for the chemical basis of life. By the early twentieth century, it was clear that enzymes had a protein component and that this component was probably responsible for the catalytic effect of enzymes. The American chemist John Northrop produced definitive proof of this hypothesis in 1930. Northrop crystallized the enzyme pepsin, and since crystallization was believed to be a property of pure chemical molecules (impurities prevented the formation of a regular crystalline lattice), Northrop and his contemporaries believed that he had shown that pepsin was a protein. He then demonstrated that the crystallized enzyme retained its catalytic effect. His work was a turning point in enzyme chemistry, uniting it with protein chemistry. Catalysis, an important process of living cells, was now firmly linked to proteins.

The last components to be added to this summary are nucleic acids. This class of molecules includes deoxyribonucleic acid (DNA) and ribonucleic acid (RNA).

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68 Due to confusion between the name ferments, as a class of chemical enzymes, and ferment as a verb designating specific processes (such as the conversion of sugar to alcohol), English speaking scientists made the transition from ferment to enzyme rather quickly. In German, the term for the process of fermentation, Gärung, was sufficiently different from the noun Ferment, to allow the latter to be used without confusion. The scientists whom I shall discuss below continued to use the terms Ferment and Enzyme interchangeably until well into the 1940s. Fruton, Proteins, Enzymes, Genes, 148.

69 Ibid., 208; also see Chapter 4, “From Ferments to Enzymes.”
Chemically the two are very similar—both are long chains of four different repeating sub-units, called nucleotide bases, each of which is attached to a five-carbon sugar. The sugar in RNA is called ribose; the sugar in DNA lacks one oxygen atom present in ribose, and is therefore called deoxyribose. These sugar/base complexes are in turn linked together in chainwise fashion by phosphate groups. DNA consists of two such chains wrapped around one another, while RNA is single stranded.

DNA was first isolated from the nuclei of pus cells by the Swiss chemist Friedrich Miescher in the city of Tübingen in 1869. Miescher had originally been interested in studying the composition of the cells themselves by isolating and then purifying the cytoplasm. When his initial experiments were inconclusive, he turned his attention toward the cell nuclei, and eventually isolated a substance that was high in phosphorous. He called the substance nuclein in recognition of its location within the cell. Two years later he found that nuclein was associated with a protein he called “protamine.” Shortly thereafter it was suggested that nuclein was identical with chromatin, the material of chromosomes, establishing the link between nucleic acids, proteins, and heredity.

At the beginning of the twentieth century, the German biochemist Albrecht Kossel spent many years analyzing the non-protein component of nuclein; this became known as nucleic acid. Kossel concluded that, like proteins, nucleic acid was also composed of individual building blocks arranged one after another in a chainlike manner. In nucleic acids, these building blocks are called nucleotide bases. By the 1920s, the subtle differences between the nucleic acids DNA and RNA were detected, and the two were distinguished from one another. Researchers initially thought that DNA was associated with animals and RNA with plants, but they quickly discarded this idea. Both kinds of nucleic acid consisted of four different nucleotide bases that were present in

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roughly equal amounts, which indicated to many experts that their structure was simpler and more regular than that of proteins. The apparent simplicity of nucleic acids suggested that proteins were in fact the carriers of biological inheritance, since their complex structure was better suited for storing and transmitting the information necessary for inheritance.

The inability of classical genetics to deal with the gene on a physical level, coupled with the advances in biological chemistry, created an environment in which researchers could imagine exploring genetic problems with a variety of methods. Enough experimental evidence existed by the mid-1930s to convince many investigators that coupling genetics with chemistry and biology would be a productive strategy for determining the physical character of biological inheritance. Proteins and, to a lesser extent, nucleic acids, were the molecules that promised to be of most use in bringing these previously separate disciplines together.

**Outline of the Argument**

The main narrative of this study will unfold as follows. Chapter 2, “Virus Research in National Socialist Germany, 1937-1945,” details the creation of an interdisciplinary working group for the study of tobacco mosaic virus (TMV) that was created in two institutes of the Kaiser-Wilhelm-Gesellschaft, the Institutes for Biology and for Biochemistry. Immediately following the National Socialist takeover, the German scientific community was decimated by the dismissal of Jewish scientists. Shortly afterward, in America, chemist Wendell Stanley made a dramatic breakthrough in virus research by crystallizing TMV. Stanley’s work opened up the possibility of using viruses such as TMV as model systems for understanding the chemical basis of gene function. This implication of Stanley’s research became immediately clear to three new scientific directors, Fritz von Wettstein, Alfred Kühn, and Adolf Butenandt, who had

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71 Olby, *Path to the Double Helix*, Chapter 6 “Kossel, Levene, and the Tetranucleotide Hypothesis.”
been called to the Kaiser-Wilhelm-Institutes in Dahlem in the wake of the dismissals. With generous support from the regime and from industry, they created one of the most advanced virology labs in the world. The researchers argued that TMV could be used as a model to understand all types of crop-destroying viruses, allowing them to present their work as an indispensable part of the German drive for agricultural self-sufficiency. The chapter concludes with an exploration of the ways in which virus research and National Socialism supported and reinforced one another without necessarily sharing the same goals.

Chapter 3, "Science in a Shattered State, 1945-1950," examines the re-establishment of the infrastructure for scientific research in the years immediately after World War II. Here I emphasize how the unique cultural and historical constraints that were present in postwar Germany shaped biological research. The Kaiser-Wilhelm-Gesellschaft was re-founded as the Max-Planck-Gesellschaft zur Förderung der Wissenschaften, and a de-centralized national funding system was created. The German university system was re-established unchanged in structure from its prewar status. Biology departments continued to be divided between zoology and botany, excluding fields such as genetics, biophysics, and biochemistry, which hindered interdisciplinary research. The TMV research community, still in Tübingen, enjoyed considerable financial and political support in the postwar years. Those scientists who had been members of Nazi organizations were able to deflect attention away from any record of their complicity with the Nazi government and could therefore continue their work with little or no difficulty.

Chapter 4, "From Virus Research to Molecular Biology, 1945-1956" analyzes postwar scientific research in Tübingen, focusing on the importance of the relationship between the German investigators and the rest of the international scientific community. The chapter begins with a discussion of the importance of the scientific community for
the production of scientific research, then examines the ways that this community was strained by twelve years of Nazi rule. Re-integration into this community was the most important challenge facing German scientists in the immediate postwar years. Initially, their research was not of universal significance, but that changed with Alfred Gierer and Gerhard Schramm’s award-winning work on the genetic material of TMV in 1956. Their research established TMV as a model system with relevance to all of biology, but more importantly, it established the Tübingen scientists as members of an international community of researchers whose work was leading to the emergence of the new field of molecular biology.

Chapter 5, “Tübingen and the Genetic Code, 1957-1966,” follows the work of the Tübingen researchers as they became part of a massive international research effort targeted at understanding how genetic material, such as DNA, directs the manufacture of proteins. Specifically, the Tübingen researchers helped to establish the correlation between small sequences of genetic material and individual amino acids, known as the Genetic Code. The chapter demonstrates that the German scientists were thoroughly integrated into the international scientific community and had the respect of their peers. It concludes with an examination of how these researchers presented the new biology to West German society. They made a deliberate effort to bring their work to a broader audience, but they did not discuss the potential application of their work to human beings through medicine and biotechnology. Instead, they were publicly critical of efforts by their colleagues in Britain and the US to articulate a new eugenics based on the medical application of advances in molecular biology.

Chapter 6, “Restructuring Research in the Land of the Economic Miracle: Reform, 1950-1972” returns to the broader social and cultural context of the scientific enterprise by focusing on the institutionalization of the new biology in the Federal Republic. Although the West German economic recovery (called the “Economic
Miracle") was dependent upon export of manufactured goods, outside of chemistry there were few ties between the scientific community and industry. Overall, the Economic Miracle was decidedly low-tech. The continuing lack of a West German presence in the most advanced fields of science and technology became a source of increasing concern for the country’s leadership during the period covered by this chapter. In the early 1960s, the Max-Planck-Gesellschaft created new institutes for molecular biology research, and new funding organizations provided additional support for investigators. Meanwhile, organizational rigidities continued to exist in the university system, hampering the establishment of molecular biology. In the late 1950s, individual researchers and representatives of the major scientific organizations concluded that a major effort to overhaul the universities was necessary in order to alleviate these types of difficulties. In biology, the first attempt at such thorough reform was the establishment of a new Institute for Genetics at Cologne University. After some initial difficulties, the institute succeeded admirably and carved out an important space in the research landscape for the universities.
Chapter 2:

Virus Research in National Socialist Germany, 1937 to 1945

Introduction

In the early twentieth century, Germany was the global center of scientific research. Advances in physics and chemistry helped fuel the country’s industrial growth, allowing Germany to overtake Britain as Europe’s greatest industrial power by the eve of the First World War. Despite the lost war and the isolation of researchers from the international community, many parts of German science survived the war and flourished by the late twenties. Many of the best German scientific talents worked in the institutes of the Kaiser-Wilhelm-Gesellschaft zur Förderung der Wissenschaften (KWG), many of which were located in the Berlin suburb of Dahlem. After the Nazi assumption of power in 1933, Nazi racial laws compromised the German scientific community but did not destroy it. Therefore it was possible that in Dahlem, in the wake of a wave of dismissals, a new interdisciplinary center for virus research could be founded in 1937. Judged by structure, equipment, and choice of problems, the new center was one of the most advanced virology labs in the world. Although the relationship of individual researchers and the Nazi Party varied, as a whole they justified their work and secured financial support by emphasizing the practical results of their research. The idea of science as an apolitical search for the truth lent them distance from the regime and allowed them to continue their work in the postwar era in the rubble left by twelve years of Nazi rule.
This chapter begins with a brief discussion of the local, institutional, and scientific backgrounds of the virus research program. As new scientific directors were called to the institutes of the Kaiser-Wilhelm-Gesellschaft in Dahlem, American chemist Wendell Stanley stunned the world with his research on tobacco mosaic virus (TMV). TMV quickly became the experimental object of choice for Dahlem's new researchers. This program was formalized as a division for virus research, the Arbeitsstätte für Virusforschung, which combined elements of botany, genetics, and biochemistry. The chapter concludes by illustrating how the agricultural applications of TMV research involved the program in Nazi Germany's policy of agricultural self-sufficiency, or autarky, and how the desire of the researchers to pursue their own interests inadvertently gave support and prestige to the National Socialist government.

2.1 Background

2.1.1 Berlin-Dahlem

Berlin's status was the result of a relatively recent turn of events that had allowed German researchers to achieve great success both within and outside the structure of the German university system. Germany's modern university system originated in Prussia and was the result of a thoroughgoing reform by the Prussian government in the wake of defeat in the Napoleonic Wars. Philosophers and scholars directed the reform of the universities, and hoped to acquire the rights of self-government for the universities and privileges for university professors. Based on the ideals of the Prussian reformer Wilhelm von Humboldt, the reformed universities became general educational institutions instead of narrowly focused professional schools. The new universities were to stress the importance of both teaching and research as aspects of scholarship, and to cover subjects ranging from the natural sciences to the humanities. Their aspiration toward universality led the reformers to define individual disciplines very broadly, but the need to confer authority on individual professors led to the drawing and enforcement
of sharp boundary lines between these broad disciplines. The result, by the late nineteenth century, was a curious, fragmented universality. By that time, the emphasis on research meant that the German universities were the most advanced places in the world where students could go for training in how to do scholarly research, particularly in the natural sciences.

In addition to the generally excellent research institutions in its universities, Imperial Germany also had a strong research program in the physical sciences that took place within private industry. After the global economic slump of the 1870s and 1880s, the industrial might of the German Reich expanded dramatically in the 1890s, fueled largely by the technologies of the second industrial revolution—steel, chemicals, and electricity. Unlike the First Industrial Revolution, this wave of industrial expansion depended upon methodical laboratory research as well as tinkering. 

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2 For example, the first modern chemistry lab that trained students for careers as academic chemists was led by Justus von Liebig in Giessen beginning in the 1840s. John Hudson, *The History of Chemistry* (New York: Chapman and Hall, 1992).

3 The First Industrial Revolution, which took place roughly between 1750 and 1850, was relatively independent of laboratory scientific research. Its stimulus was largely ecological and much of its innovations were organizational in nature. The primary technological innovation of the First Industrial Revolution, the steam engine, was developed through a process of trial and error; its workings were not well understood until the middle of the nineteenth century. At that point, James McClellan and Harold Dorn argue that a "Second" scientific revolution took place. At this time, several previously separate fields of inquiry (such as electricity, magnetism, and mechanics) were joined in a synthesis that we today think of as classical physics. As the revolution unfolded, the synthesis was mathematized and used to generate a coherent scientific world picture by the 1880s, which was then coupled to economic processes. Its greatest industrial application came through the generation and transmission of electricity. In chemistry, knowledge
physical sciences led to improvements in manufacturing, which in turn led to advances in knowledge. The entire process in which scientific knowledge was converted into industrial technology was complex and reciprocal, with both sides reinforcing one another. Complex interdependencies such as these were fostered by linking the research and manufacturing processes in a new phenomenon dedicated to institutionalizing technological innovation within the firm—the industrial research lab. In the late nineteenth century, more and more academic scientists who specialized in chemistry and physics were recruited to work in such labs. New firms, particularly in the chemical industry, depended upon the expertise of these individuals. For example, the Bayer Chemical Company began hiring academic chemists in the 1870s; Werner von Siemens, founder of the Siemens electrical company, regarded his formal training in mathematics, physics, and chemistry, received while an engineer in the Prussian Army, as integral to his later success. His firm pioneered innovative work in electrical and communications equipment, and by 1914 his companies had sales of more than RM 314 million and employed more than 81,000 people worldwide. 


The overall excellence of Wilhelminian science cannot be attributed to these two sectors operating independently of one another, but to the fruitful interaction of the "pure" science of the universities with the "applied science" of the industrial world. These two styles of research met in what Harwood has called "Third-Sector Institutions," which were neither industrial nor academic, received a mix of public and private funding, and were geographically and institutionally separate from both firms and universities.⁵

Leaders of Imperial Germany, well aware of the significance of scientific research for industrial and military power, began to support research in the physical sciences very generously by the end of the century. Since the government took such an active interest, the national capital of Berlin became a focal point for the founding of new institutes supported by government money. In 1887, the government founded the Physikalisch-Technische Reichsanstalt (Imperial Physical and Technical Institute) in Berlin. Originally, the institute was meant to be a national laboratory to establish standards for the electrical industry, but theoretical work was done there as well. Research completed in the Reichsanstalt at the turn of the century contributed to Max Planck's groundbreaking work in quantum mechanics.⁶ The Physikalisch-Technische Reichsanstalt served as a precedent for government research initiatives, and its example led chemical industry representatives to lobby for a similar institute. Such an institute was never built. Instead, after many years of negotiations, in October of 1912, Kaiser

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Wilhelm II officially opened two new research institutes that formed the core of the newly created Kaiser-Wilhelm-Gesellschaft in Dahlem.

The Kaiser-Wilhelm-Gesellschaft was a self-governing association of research institutes collectively dedicated to the improvement of German scientific research. Kaiser-Wilhelm-Institutes fulfilled all of Harwood’s requirements for third-sector institutes—they were neither fully academic nor industrial, they received public and private funding, and they were distinct from the universities. Chemistry and biology served as focal points for the society’s research in its early years. The first two institutes founded in Dahlem were the Kaiser-Wilhelm-Institut für physikalische Chemie und Elektrochemie (Kaiser-Wilhelm Institute for Physical Chemistry and Electrochemistry), and the Kaiser-Wilhelm-Institut für Chemie (Kaiser-Wilhelm-Institute for Chemistry), both of which were dedicated on October 23, 1912. In 1913, a third institute was opened in Dahlem, the Kaiser-Wilhelm-Institut für experimentelle Therapie (Kaiser-Wilhelm-Institute for Experimental Therapy). This institute had both biological and chemical divisions, and such was the importance of the interaction of these two areas that they were institutionalized as a hybrid field in 1917 in the Kaiser-Wilhelm-Institut für Biochemie (Kaiser-Wilhelm-Institute for Biochemistry), directed by Carl Neuberg. In 1934, this satellite institute became fully independent. The fourth institute built in the Dahlem area was the Kaiser-Wilhelm-Institut für Biologie (Kaiser-Wilhelm-Institut für Biologie), which opened in 1915.\(^7\)

The institutes of the Kaiser-Wilhelm-Gesellschaft were able to attract the best scientists in Germany, who pursued innovative research programs and achieved

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spectacular results despite the financial hardships of the interwar years and Great Depression. This success was due in large part to the generous support of the Rockefeller Foundation, the influence of which on the emergence of molecular biology early in the twentieth century, was profound. One of the goals of the Foundation’s administration was to improve public health, and in the early years of the twentieth century, they sponsored successful campaigns against communicable diseases in the Americas. From 1932 to 1953, it provided roughly $25 million for molecular biology research in the United States; its support for European biology was lavish as well.\(^8\)

The foundation supported German researchers including biochemist Carl Neuberg and geneticists Richard Goldschmidt and Carl Correns, but was perhaps most remarkable in its support for the institutionalization of new branches of research combining traditional fields.\(^9\) This enthusiasm for innovative research is best illustrated by the support the Foundation provided for Otto Warburg, a biochemist whose research style applied the techniques of physics and chemistry to studies of photosynthesis and respiration, traditionally biology topics. While visiting the United States in 1929, Warburg secured a promise from the Rockefeller Foundation to fund two new institutes to enable him to carry out his research; these became the Kaiser-Wilhelm-Institutes für Zellphysiologie and Physik (Kaiser-Wilhelm-Institutes for Cell Physiology and for Physics). In total, the Foundation donated more than RM 2.73 million (roughly

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\(^9\) In 1932, the Rockefeller Foundation gave RM 232,160 to individual researchers for their own projects. MPG-Archiv, I. Abt., Rep. 1A, Nr. 1094 (10).
$650,000) for the construction of these institutes.\textsuperscript{10} Warburg promptly repaid their
investment by winning the Nobel Prize in 1931. The Rockefeller Foundation also
contributed RM 1,323,000 for the construction of a new institute for Brain Research
under the leadership of Oskar Vogt in Berlin-Buch.\textsuperscript{11}

The location of so many different institutes in close proximity to one another
centually encouraged the interaction of thinkers from different disciplinary backgrounds. In
Dahlem, physicists, chemists, biologists, and geneticists could socialize with one another
and discuss problems of general scientific significance. The universally high caliber of
the individuals involved meant that even casual conversations had the potential to lead to
profound breakthroughs. Georg Melchers, a botanist and central figure in the German
TMV research program, later recalled that German molecular biology really began at the
Kaiser-Wilhelm-Gesellschaft’s swimming pool in Dahlem.\textsuperscript{12} There, Melchers and his
colleagues were able to discuss problems of biology and genetics with physicists and

\textsuperscript{10} Henning and Kazemi, "Dahlem", 16-19; in a letter dated May 1, 1930, the foundation pledged RM
2,735,000 to purchase land and construct the two institutes; another letter in the same file, from Alan Gregg
to Friedrich Glim, dated April 22, 1930, estimated this amount at $651,190. MPG-Archiv, I. Abt., Rep.
1A, Nr. 1094.

\textsuperscript{11} MPG-Archiv, I. Abt., Rep. 1A, Nr. 1613. Also see Jochen Richter, "Das Kaiser-Wilhelm-Institut fur
Hirnforschung und die Topographie der Grosshirnhemisphären. Ein Beitrag zur Institutesgeschichte der
Kaiser-Wilhelm-Gesellschaft und zur Geschichte der architektonischen Hirnforschung," in Die Kaiser-
Wilhelm-Max-Planck-Gesellschaft und Ihre Institute. Studien zu ihrer Geschichte: Das Harnack Prinzip,
ed. Bernhard vom Brocke and Hubert Laitko (Berlin: Walter de Gruyter, 1996), 349-409. Surprisingly, the
institute in Berlin-Buch survived the Second World War, became a center for biological research in the
DDR from the 1940s-1980s, and recently was reorganized as the Max-Delbrück-Institute for Molecular
Medicine under the leadership of Detlev Ganten. See Josef Reindl, "Akadmiereform und biomedizinische
Forschung in Berlin-Buch," in Antworten auf die amerikanische Herausforderung: Forschung in der
Bundesrepublik und der DDR in den "langen" siebziger Jahren ed. Gerhard A. Ritter, Margit Szöllősi-
Janze, and Helmut Trischler (Frankfurt: Campus Verlag, 1999) 339-360. Reindl is also working on a
forthcoming study, The History of Biomedical Research at Berlin-Buch. 1920’s to Today. Also see Rainer
Hohfeld, "Between Autonomy and State Control: Genetic and Biomedical Research," in Science Under
Socialism: East Germany in Comparative Perspective, ed. Kristie Macrakis and Dieter Hoffmann
Institute Berlin-Buch. Beiträge zur Geschichte (Heidelberg: Springer Verlag, 1997).

\textsuperscript{12} Melchers, "Abschrift an die Deutsche Forschungsgemeinschaft," April 8, 1963. RAC, Record Group
1.2, Series 717, Box 6, Folder 68.
chemists. The intellectual climate of Dahlem was as important in stimulating the work of the TMV researchers as was the research infrastructure provided by the Kaiser-Wilhelm-Gesellschaft and the Rockefeller Foundation.¹³

2.1.2 The Purge of the Research Community

Dahlem’s tranquility was shaken in 1933, when the Nazi Party took control of the German government. Nazi racism in general, and anti-Semitism in particular, had an immediate impact on Jewish members of the German research community. In April, 1933, the Nazis set about removing their opponents (defined racially or politically) from positions of authority by passing the “Law for the Restoration of the Professional Civil Service.” This law stated that anyone who did not meet specific requirements could be “retired” from government service. Paragraph 3 of the law specifically indicated that any civil servant who was not of Aryan descent, that is, anyone who had a Jewish grandparent, was to be dismissed. Initially, veterans of the First World War were exempted, but the Nuremberg Laws of 1935 rescinded this exemption. In total, the purge affected perhaps 1 to 2 per cent of the German civil service, between 15,000 and 30,000 persons, but in the higher ranks the percentage of those dismissed may have been as high as 12 per cent.¹⁴

The impact on the high ranks of civil service is particularly important for our story, since in Nazi Germany, university professors were civil servants and thus fell within the jurisdiction of the law. Many professors of all ranks were dismissed in accordance with the wishes of the new regime. The physical sciences were severely affected. The fields of research that suffered the greatest losses were the newest and most

¹³ The environment in Dahlem was similar to that of the best American biology departments, such as the University of Chicago, in the early twentieth century. See Philip J. Pauly, Controlling Life: Jacques Loeb and the Engineering Ideal in Biology (Oxford: Oxford University Press, 1987).

innovative, probably because they allowed the greatest possibility for advancement for those, such as Jews, who had been marginalized in Imperial and Weimar Germany.\textsuperscript{15} Physics, the most innovative of the Weimar sciences, lost perhaps 25 per cent of its members, many of whom emigrated.\textsuperscript{16}

As I indicated in Chapter 1, biochemistry was also a new, innovative field in the 1930s. This novelty and interdisciplinarity were precisely the reasons that the Rockefeller Foundation chose to support biochemical research, like that of Warburg, so generously. Since the field was so new, there were very few institutes dedicated to the study of biochemistry in Germany, and consequently the best biochemists often worked in hospital laboratories or in private institutes. Much of the material published in the main biochemistry journal, \textit{Biochemische Zeitschrift}, came from researchers whose formal training was in disciplines other than biochemistry.\textsuperscript{17} Nevertheless, the overall excellence of German chemical research in the most general sense meant that biochemical research was in fact very strong in Germany until the 1930s, whatever the names of the institutes in which it took place.\textsuperscript{18}


\textsuperscript{16} Alan D. Beyerchen, \textit{Scientists Under Hitler: Politics and the Physics Community in the Third Reich}, (New Haven: Yale University Press, 1977), 47. The exact percentage depends of course on how one defines the positions under study; other scholars, defining the position of physicist more broadly, have reached a smaller percentage. For a lower estimate see Klaus Fischer, “Der quantitative Beitrag der nach 1933 emigrierten Naturwissenschaftler zur deutschsprachigen physikalischen Forschung,” \textit{Berichte zur Wissenschaftsgeschichte} 12 (1988): 83-104.

\textsuperscript{17} Kohler, \textit{From Medical Chemistry to Biochemistry}, esp. p. 6, where he concludes that the institutionalization of biochemistry in America was the result of a thoroughgoing reform of the medical schools that provided the opportunity for the creation of a new discipline that simply did not exist in Britain or Germany. Also see Kohler, “The History of Biochemistry: A Survey,” \textit{Journal of the History of Biology} 8 (Fall, 1975): 286.

\textsuperscript{18} According to Ute Deichmann, “Notwithstanding certain American and British breakthroughs in physical chemistry, Germany was the undisputed leader in chemistry and bio-chemistry until the mid-1930s.” From “The Expulsion of German-Jewish chemists and biochemists and their correspondence with colleagues in Germany after 1945—the impossibility of normalisation?” in \textit{Science and Technology in the Third Reich}, ed. Margit Szöllösi-Janze (Oxford: Berg Publishers, 2000), 243-280, quote from 243. The impact of Nazism on German biochemistry is covered in more depth in Deichmann, \textit{Flüchten, Mitmachen, Vergessen}.
As a new, incompletely institutionalized field, biochemistry was relatively free of the institutionalized anti-Semitism of other academic disciplines. Therefore, before 1933, biochemistry was open to all talented researchers and included a significant percentage of Jews. Consequently, biochemistry was devastated by the Nazi purge. Ute Deichmann has calculated that roughly 26 per cent of researchers whose work can be considered biochemistry were dismissed or chose to retire as a result of Nazi racial discrimination. Of these, a great many emigrated, enriching the scientific communities of nations that would soon be Germany’s adversaries, while simultaneously impoverishing that of the nation that had betrayed them.

Since the Kaiser-Wilhelm-Institutes received a mix of public and private funding, their situation was more complicated. The general rule was that if a particular institute received more than 50 per cent of its funding from the state, its non-Aryan workers were subject to dismissal. In total, 21 of 30 Kaiser-Wilhelm-Institutes were affected. The distribution of Jewish scientists in these institutes mirrored that of the scientific community as a whole, with many more working in newer fields. Fifty per cent of the

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21 Macrakis, *Surviving the Swastika*, 55.
researchers in the Institute for Physics were defined as non-Aryan according to the 1933 law; in the Institutes for Physical Chemistry and Biochemistry, it was roughly 25 per cent.\textsuperscript{22} Not all were immediately removed from their posts, but the climate became so antagonistic that it was exceedingly difficult for Jewish researchers to continue their work. The examples of Carl Neuberg, Fritz Haber, and Otto Warburg, all Kaiser-Wilhelm-Gesellschaft Directors involved in chemistry research, illustrate the spectrum of fates awaiting Jewish scientists in the early years of the Third Reich.

Carl Neuberg had been director of the Kaiser-Wilhelm-Institut für Biochemie since 1922, and as a veteran of World War I, he was initially exempt from dismissal. He attempted to continue working, but a staff member denounced Neuberg and he was forced into retirement in 1934, although he did serve as temporary director of the institute until its reorganization in 1936. Thanks to assistance from some of his colleagues in the Kaiser-Wilhelm-Gesellschaft, he was able to do research in a private lab in Berlin, and he eventually emigrated to the United States.\textsuperscript{23}

Haber, too, was exempt from dismissal thanks to his service in the First World War—he and his lab had been largely responsible for providing the German Army with the tools of gas warfare—but when he was ordered to dismiss two of his technicians who were defined as non-Aryan, he resigned publicly in protest. Historian Henry Harris has suggested that this public act of defiance was less a moral protest than an effort to pressure the authorities into granting Haber and his institute freedom from Nazi racial laws. In essence, Harris portrays the incident as a power play meant to reinforce Haber’s independence from government interference in his work. Haber had miscalculated, however. His Nobel Prize and service to Germany during the war were not enough to

\textsuperscript{22} Helmuth Albrecht and Armin Hermann, “Die Kaiser Wilhelm Gesellschaft im Dritten Reich,” in *Forschung im Spannungsfeld*, 361.

\textsuperscript{23} Henning and Kazemi, *Dahlem*, 112.
overcome the blind hatred of Nazism, and his resignation was accepted. His health failing, he went into exile in England and accepted an unpaid position at Cambridge. He died during travel to Basel in 1934. His year in exile was made particularly bitter by the indifference shown to him by so many of his German colleagues. They had made no public demonstration of support, nor had they attempted to smooth his transition to exile. After Haber’s death, his friend Max Planck, President of the Kaiser-Wilhelm-Gesellschaft, organized a memorial service for him over much opposition from the government. Such was the official repugnance for Haber’s resignation that attendance at his memorial became an act of defiance in itself.  

Otto Warburg’s case was radically different. Instead of facing dismissal and exile, he was able to remain in Germany and work throughout World War II. The Rockefeller Foundation had paid for the construction of Warburg’s institute and continued to provide a significant portion of its budget. Technically, therefore, the institute was exempt from the law, although as Neuberg’s case shows, exemption in no way guaranteed security. Warburg, too, was a veteran, and he had turned his attention toward cancer research, a field that was extremely important to high-ranking National Socialists such as Göring and Hitler. It was probably the combination of all of these factors that enabled him to survive numerous denunciations and continue his research despite the fact that his father was Jewish.  

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24 Henry Harris, “To Serve Mankind in Peace and the Fatherland in War. The Case of Fritz Haber,” German History 10 (1992): 36-37; Albrecht and Hermann, 363-364. For a more personal account by Haber’s godson, the historian Fritz Stern, see “Fritz Haber: The Scientist in Power and Exile,” in Dreams and Delusions: The Drama of German History (New York: Alfred A. Knopf, 1987), 51-76. For an account of the Haber memorial service, see Beyerchen, Scientists after Hitler, 66-69.

25 Due to the deteriorating conditions in Berlin, Warburg spent the last years of the war at his country home on an island in the Baltic Sea. “Report on Educational Conditions in Postwar Germany. Based on the notes made by A. R. Mann during trip to Germany January and February 1947.” RAC Record Group 1.1, Series 717, Box 5, Folder 24, p. 66; also see Hans Krebs, in collaboration with Roswitha Schmid, Otto Warburg: Cell physiologist, Biochemist and Eccentric, trans. Hans Krebs and Anne Martin (Oxford: Clarendon Press, 1981); Macrakis, Surviving the Swastika, 64. The importance of cancer research in Nazi Germany is the subject of Robert N. Proctor’s The Nazi War on Cancer (Princeton: Princeton University Press, 1999).
2.1.3 Wendell Stanley and TMV Research

As one scientist supported by the Rockefeller Foundation struggled to continue his research in the institutionalized anti-Semitism of Nazi Germany, another, working under much less appalling conditions, announced a discovery that would permanently change the focus of biological research back in Dahlem. In the summer of 1935, the American chemist Wendell Stanley, working at the Rockefeller Institute for Medical Research in Princeton, published a brief paper in the journal *Science* on tobacco mosaic virus. Stanley claimed to have isolated and crystallized the virus in its pure form, and his findings immediately had a dramatic impact. His was one of the most significant discoveries to come out of the prestigious Rockefeller Institute, and the news was not confined to the scientific elite—on the same day that Stanley’s formal announcement appeared in the journal *Science*, the *New York Times* ran a front page article on his work.26 Stanley claimed to have isolated a crystalline protein that had the characteristics of TMV, but, in fact, the details of his work were subject to clarification. Later investigators showed that Stanley had not isolated a protein, but rather the virus itself, which was a mixture of protein and nucleic acid. Stanley’s crystals also contained a significant amount of water.27 Nevertheless, these corrections did little to diminish the impression Stanley had made on the scientific community. Since so little was known

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27 For contemporary criticism of Stanley’s results, see the letters from Sir Frederick Bawden, dated Sept. 27, 1937, and Feb. 1. 1938. Stanley Papers, Carton 6, Folder 85.
about viruses, they were an exciting new topic, and Stanley’s work indicated that they could be used to approach the critical problem of biological reproduction.\textsuperscript{28}

In the 1930s, researchers believed that genes were responsible for the transmission of hereditary traits from one generation of living organisms to the next. More than two decades of meticulous work on the patterns of heredity in model organisms, particularly the fruit fly \textit{Drosophila melanogaster}, had revealed rules of biological inheritance and had allowed geneticists to map specific biological traits to exact locations on chromosomes in the nucleus.\textsuperscript{29} Geneticists called the locations “genes,” but they were unsure of the physical nature of these genes, and some even doubted that genes really existed as physical entities. Most investigators were content to treat genes as theoretical entities and to study their transmission from generation to generation rather than the physical nature of the gene itself.\textsuperscript{30}

Chemical analysis had shown that chromosomes, which housed genes, were composed of proteins and nucleic acids, and so these substances definitely played a role in the makeup of genes. Since genes possessed the most important characteristic of life as defined by biologists—an organism’s ability to reproduce itself—research focused on protein, the substance deemed most likely to possess the ability of self-duplication. By the 1930s, as I have explained, researchers knew that proteins were large and complex, but many believed that the structure of nucleic acids was relatively simple in comparison.

\textsuperscript{28} Angela Creager has shown that Stanley’s work was received differently by different research communities, and the responses ranged from skepticism to unbridled enthusiasm. Stanley himself quickly assimilated the criticisms of his initial work and modified many of his own suggestions, especially the key point of autocatalysis. Creager, \textit{Life of a Virus}, 75. Stanley’s willingness to change his own position, and the stunning effect of his first publication, helped account for the continuing importance of his work in the history of molecular biology. Its inspirational effect on the German research community will be discussed later in this chapter.


\textsuperscript{30} Kay, \textit{Molecular Vision of Life}, 105.
Furthermore, certain proteins, called enzymes, had the ability to catalyze their own reproduction. That is, they could facilitate their own reproduction by accelerating the rate of the chemical reactions that had produced them in the first place. Such self-catalysis, or autocatalysis, as it was usually described, was observed in a number of different chemical systems, and many researchers believed it was the solution to the problem of biological reproduction. They thought that the protein in genes was autocatalytic, which accounted for the ability of genes to reproduce themselves. Nucleic acids were given a secondary role in replication.

At the same time very little was known about viruses. They were relatively mysterious biological agents, the physical nature of which was unknown, but there were reasons to think that they might be good model systems for genes. Investigators knew that certain viruses had the ability to reproduce themselves and to mutate, both of which were properties of genes in higher organisms. Viruses also required the presence of living cells to reproduce themselves; they were parasitic, and used the metabolic machinery of host cells for their own replication. These characteristics allowed researchers to view viruses as freely roaming genes, entities that possessed the ability to replicate without the complex metabolism of higher organisms. The combination of self-replication, mutation, and simplicity meant that viruses had the potential to serve as models for genes, and as early as 1921, the geneticist Herman Muller suggested that research into virus-like substances might be one avenue for exploring the gene on the most basic level.

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For many, Stanley's research meant the realization of this goal. Stanley claimed that he had isolated a virus in a crystalline form. Crystallization was a chemical phenomenon, in which identical molecules form precise, repeating geometric patterns. Crystallization could occur in only a pure substance (impurities prevented the formation of the crystal lattice) whose molecules were all chemically identical. When his experiments resulted in needle-like crystals of TMV, Stanley argued that he had found evidence that TMV itself was a self-catalyzing protein. He concluded his paper by stating, "Tobacco-mosaic virus is regarded as an autocatalytic protein which, for the present, may be assumed to require the presence of living cells for multiplication."\(^{34}\) As written, his experiments showed that simple chemical molecules, proteins, could in fact have the most important characteristics of life. He therefore opened up a means by which biological processes could be understood through chemical research: he had bypassed mystical notions of vital forces animating life and equated the important property of reproduction with chemical autocatalysis. His work had an immediate impact, and has since has been described as the symbolic beginning of molecular biology.\(^{35}\)

### 2.1.4 New Arrivals in Dahlem

Stanley published his results just as a major change in personnel took place in Dahlem. In February, 1933, Carl Correns died. Correns had been one of the three rediscoverers of Mendel’s work in 1900 and later became the first director of the Kaiser-Wilhelm-Institut für Biologie. After his death, the leaders of Kaiser-Wilhelm-

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\(^{34}\) Stanley, "Isolation," 645.

\(^{35}\) Kay, *Molecular Vision of Life*, 111.
Gesellschaft moved quickly to replace him, hoping to minimize interference from the new government. Within a few months, they found a suitable candidate in Friedrich (Fritz) von Wettstein. Wettstein’s father had been a renowned botanist from Vienna; the son, born in 1895, quickly followed in his father’s footsteps. Fritz von Wettstein studied botany and became an assistant in Correns’ lab in Dahlem in 1919. Wettstein remained there until 1924, when, at the relatively young age of thirty, he became a professor of botany specializing in plant genetics at Göttingen University. He remained in Göttingen until 1931, when he accepted a position in Munich.

Despite his qualifications, his appointment was delayed by more than a year. His colleagues and students in Munich did not wish to lose him, nor did the Ministry of Education of the state of Bavaria. Complicating matters was desire of the new regime to create a national ministry of education. Such a centralized institution for education was unprecedented in German history. Since the nineteenth century reform of the universities, their intellectual freedom had been protected by autonomy at many levels. Independent German states had pursued their own educational policies, the individual universities themselves retained the rights of self-governance, and full professors within each university had an extraordinary amount of personal freedom and power. Academics of all backgrounds therefore had grave reservations about the transfer of authority from the local to the national level that such a ministry entailed. Nevertheless the Reichserziehungsministerium (Reich Ministry of Education—REM) was established in 1934, and on May 1 of that year, Bernhard Rust became the first minister of education.38

36 Macrakis, Surviving the Swastika, 111-118.


38 Beyerchen, Scientists under Hitler, 54-56.
The relationship between the previously existing Prussian Ministry of Education (which oversaw appointments to the KWG) and the REM was not immediately clear. The leadership of the Kaiser-Wilhelm-Gesellschaft had no desire to have such an important position filled for them by an agency created and staffed by the National Socialist government, and so they continued to lobby for Wettstein’s appointment, which was finalized later that year.39

Further complicating matters at the Kaiser-Wilhelm-Institut für Biologie in the mid-1930s was the fact that the director of its second division, Richard Goldschmidt, was Jewish. Goldschmidt himself was not immediately dismissed, but his subordinates began to question his authority. He therefore began his own preparations for emigration. He was dismissed in 1935, after which he emigrated to the United States and accepted an appointment as Professor of Genetics and Cytology at the University of California, Berkeley.40 Thus shortly more than one year after his own appointment, Wettstein was faced with the task of finding a suitable replacement for Goldschmidt.

It did not take him long to decide on the zoologist and geneticist Alfred Kühn. Born in 1885, Kühn had studied zoology and physiology at the University of Freiburg. In the First World War he served as a medical orderly, after which he became professor of zoology at Göttingen.41 There, he and Wettstein became close friends and colleagues. The two respected one another’s expertise and understood that biology would best be


served by breaking down the university’s division of biology into the independent subjects of zoology and botany. They took turns teaching the standard genetics course and required their students to understand genetics in a general sense, not just from a botanical or zoological perspective. After Wettstein left Göttingen, Kühn began research on pattern formation in the wings of the flour moth *Ephestia kuniella*, work that was inspired by that of Goldschmidt, the recently departed director. By the mid-1930s, Kühn’s international reputation was so high that his friend Wettstein was able to secure his succession to the Dahlem directorship despite opposition from the REM.

The third major change at the Dahlem biological institutes was, like the replacement of Goldschmidt, also the consequence of Nazi racism. As I mentioned earlier, the director of the Kaiser-Wilhelm-Institut für Biochemie, Carl Neuberg, was forced out of the institute on racial grounds in 1934. The Kaiser-Wilhelm-Gesellschaft took some time to replace him, but after considerable debate, they chose Adolf Butenandt in 1936. Butenandt was born in Lehe, near Bremerhaven, on March 24, 1903, the son of a self-made middle-class merchant. He began his university education at Marburg University in 1921, where he had a great deal of difficulty in choosing his main field, since he was strongly interested in both chemistry and biology. He chose chemistry, but

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44 Rheinberger, “Ephestia,” 556.

45 The most thorough biography of Butenandt was written by one of his former students, Peter Karlson, and has been criticized for omitting or softening details concerning Butenandt’s relationship with the Nazi Party. A much more critical picture has recently been given by the American historian Robert Proctor. As part of the Presidential Commission on the history of the Kaiser-Wilhelm-Gesellschaft under National Socialism, Proctor has had unprecedented access to Butenandt’s files in the archives of the Max-Planck-Society. See Peter Karlson, *Adolf Butenandt: Biochemiker, Hormonforscher, Wissenschaftspolitiker* (Stuttgart: Wissenschaftliche Verlagsgesellschaft mbH, 1990), 11-26; Robert Proctor, “Adolf Butenandt (1903-1995): Nobelpreisträger, Nationalsozialist, und MPG-Präsident. Ein erster Blick in den Nachlass,” *Forschungspogramm “Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus Ergebnisse* 2 (2000).
he trained extensively in biology as well and combined his knowledge of both throughout his career. In 1924, he continued his studies at the University of Göttingen, where his advisor was the Nobel Prize winning organic chemist Adolf Windaus. After passing his doctoral exams in 1927, Butenandt remained in Göttingen, where he began his successful research isolating sex hormones. He also began taking on his own doctoral students and running his own lab. He had his first notable scientific success in 1929, when he and Erika von Ziegner, his future wife, successfully isolated the female hormone oestrone. In 1933, he accepted an offer to move to the Technical University of the city of Danzig, at that time a free city administered by Poland as part of the territorial settlement after the First World War. He spent three successful years in Danzig following up the work on sex hormones.

In 1935, Butenandt made a lengthy trip, funded by the Rockefeller Foundation, to several leading laboratories in the United States, including those at Harvard, Caltech, and Berkeley. While in America he was deeply impressed with the openness, collegiality, and interdisciplinarity of the best biological institutes. He returned to Germany that summer, and the following fall he received an offer to become a faculty member at Harvard. He declined the offer, and the following spring his choice to remain in Germany was amply rewarded when he received the offer to head the Kaiser-Wilhelm-Institut für Biochemie in Dahlem. At roughly the same time that he received the offer, he joined the Nazi Party.

Butenandt’s career choices in 1935-1936 offer a unique opportunity to examine how perfectly normal, rational behavior led to complicity with an irrational regime. The sequence of events is straightforward. In late 1934, Carl Neuberg was dismissed from his

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46 In addition to Windaus, Butenandt was able to study with many of the other great minds at the university as well; for example, the physicist James Franck was also on his doctoral exam committee. Karlson, *Adolf Butenandt*, 26, 35.
position as director of the KWI für Biochemie, and shortly thereafter a search for his successor began. In June of 1935, Butenandt was named as one of three possible successors. After returning from his trip to America, Butenandt received the job offer from Harvard. At the time, the financial situation of the Technical Institute of Danzig was uncertain and there was talk of moving the entire school to the city of Königsberg. In early fall, 1935, amidst this atmosphere of uncertainty, Butenandt went to Berlin, where he used the Harvard offer to negotiate a more secure position for himself in Europe. No one wanted to lose him to the United States, and representatives from the chemical industry assured him that they would support his research should he fail to find a suitable academic position. In any case, the following month the financial crisis surrounding Butenandt’s Danzig position was resolved, and he declined the offer from Harvard. By February, 1936, leaders of the Kaiser-Wilhelm-Gesellschaft favored Butenandt as Neuberg’s replacement, and in April, he was unofficially notified of his appointment. Immediately thereafter, he joined the Nazi party, despite having no prior affiliation with the party or any of its satellite organizations. In early May, he received the official offer from the KWG. In the fall of 1936, he moved his research group from Danzig to Berlin.

Clearly, the decision to offer the position to Butenandt was made on his merits as a scientist, not his political attractiveness. Similarly, Butenandt’s decisions were based

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48 The financial difficulties of Butenandt’s Danzig institute and his efforts to negotiate a position for himself are discussed in a letter from W.E. Tisdale to Warren Weaver, both of the Rockefeller Foundation, dated October 5, 1935. Butenandt explained his reasons for refusing the offer in a letter to Tisdale, November 7, 1935. Both letters RAC, Record Group 1.1, Series 700 D, Box 21, Folder 150.

49 Tisdale to Butenandt, April 21, 1936. RAC, Record Group 1.1, Series 700 D, Box 21, Folder 150.

50 Macrakis, Surviving the Swastika, 115-118. Butenandt’s close relationship with the chemical industry was probably another factor in his selection. During the Weimar and early Nazi years, business interests gained a significant influence in the Kaiser-Wilhelm-Gesellschaft, including having a say in the
on professional and personal, not political considerations. After the war, Butenandt declared that he had rejected the offer from Harvard because he was dismayed at the exodus of German scientists and remained in order to continue to contribute to German science as well as out of a feeling of obligation to his students. His reasoning sounds very self-serving in retrospect, but these were actually the same reasons that he gave for staying in the fall of 1935.\footnote{51} Loyalty to close friends and colleagues characterized Butenandt throughout his life, and personal loyalty should not be disregarded as a motivating factor in rejecting the Harvard offer.

Practically speaking, the offer from the Kaiser-Wilhelm-Gesellschaft was a dream job for a young, moderately nationalist German biochemist with three children, whose current position was outside the borders of the German nation. In the summer of 1935, the KWG began to seek money to rebuild the Institut für Biochemie as a precondition for attracting a first-rate candidate. By 1936, considerable funds were available and throughout the summer Butenandt had the institute completely rebuilt according to his own criteria; cost was no object. The KWG even built a large, comfortable house for Butenandt and his family, complete with stables for his horses. Such financial security was unknown to him in Danzig, where he had had to petition the Rockefeller Foundation for money to equip his lab.\footnote{52}

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\footnote{51} Butenandt explained his reasons for remaining in Germany in a 1995 interview, “Adolf Butenandt im Gespräch,” in Forschung und Technik in Deutschland nach 1945, 187-196. According to the report of officers of the Rockefeller Foundation, he gave similar reasons in October, 1935, before he even rejected it. See letter, W.E. Tisdale to Warren Weaver, October 5, 1935. RAC, Record Group 1.1, Series 700 D, Box 21, Folder 150.

\footnote{52} In 1934, Butenandt’s budget from the free city of Danzig was insufficient to cover his research costs, he appealed to the Rockefeller Foundation and was awarded roughly $3,000 for equipment. Adolf Butenandt, Research Report, August 7, 1934, and H. R. Robinson, Chase National Bank, to Rockefeller Foundation, August 14, 1934. RAC, Record Group 1.1, Series 700 D, Box 21, Folder 150.
With his scientific credentials firmly established, the only possible obstacle between Butenandt and the most attractive biochemistry position in the Reich was his political reliability. By late 1935, political considerations had begun to influence decision making at the highest levels in German science organizations. In fact, one of the men with whom Butenandt had spoken in the fall of 1935 was Johannes Stark, a convinced Nazi and appalling anti-Semite, who at that time was attempting (unsuccessfully, as it would turn out) to “gather the reins of German science into his hands.”\(^\text{53}\) Since the party had closed admission, Butenandt had to be specially invited to join in early 1936. Given the circumstances, he apparently found the invitation difficult to decline.

2.2  *The Arbeitsstätte für Virusforschung*

2.2.1  Establishing the Research Program

Göttingen was the common denominator among the three new directors. Butenandt had arrived there in 1924, shortly before Wettstein’s departure. In addition to studying chemistry, Butenandt also learned biology with Alfred Kühn, and although more than twenty years Kühn’s junior, the two established a very cordial relationship, more of senior and junior colleague than of teacher/pupil. Kühn had become interested in the genetic control of eye color and pattern formation in insects. He had come to genetics rather late in his career, after spending considerable time on physiology.\(^\text{54}\) He thus brought a developmental perspective to genetics—he was not concerned with just the transmission of genetic traits, but in understanding the chain of chemical reactions through which genetic information was converted into the proteins that make up living

\(^{53}\) Beyerchen, *Scientists under Hitler*, 117-122.

tissue.\textsuperscript{55} Tracing this chain of reactions required coupling his own research in physiological genetics with Butenandt’s expertise in biochemistry.

Such interdisciplinary cooperation was exceptional in Europe. In fact, such collegiality was so rare and so promising that the Rockefeller Foundation continued to fund Kühn and Butenandt on a year-to-year basis even after discontinuing much of its support of German science as a consequence of National Socialism. In a 1934 report to Warren Weaver, the head of the biological section of the foundation, European representative W.E. Tisdale wrote, “there is nowhere on the continent or in England that we can find chemists, embryologists and geneticists willing to cooperate among themselves, as are these German scientists.”\textsuperscript{56}

On November 1, 1936, Butenandt officially became director of the Kaiser-Wilhelm-Institut für Biochemie. In his first report to the Board of Trustees of the Kaiser-Wilhelm-Gesellschaft in April, 1937, he stated that his goal was to build a research center that was capable of dealing with problems in biological chemistry in the broadest sense. He closed the report by declaring his wish to establish a close exchange of ideas between the Kaiser-Wilhelm-Institutes for biochemistry and biology. He cited the fruitful work in biochemical genetics, and promised that this field was ripe for further investigation.\textsuperscript{57} Although he had brought with him nearly his entire research group from Danzig, their research was delayed since the rebuilding of the institute lasted until early fall of 1937.

\textsuperscript{55} Rheinberger, “Ephestia,” is the best study of developmental genetics as studied by Kühn and his colleagues.

\textsuperscript{56} Letter, Tisdale to Warren Weaver, August 8, 1934. RAC, Record Group 1.1, Series 717 D, box 13, Folder 123. The number of Rockefeller Fellowships to German scientists was cut from 20 in 1932 to 8 in 1933; it dropped to 3 per year in the late 1930s, reflecting increased caution on the part of the Rockefeller Foundation regarding the use of their grant money. Weindling, “The Rockefeller Foundation and German Biomedical Sciences,” 129.

As the time passed, though, the pieces fell into place that allowed Butenandt and his colleagues to establish an outstanding virology laboratory.

Butenandt had first heard of Stanley’s results during a conference on virus research in September, 1936. Although some in the audience were skeptical, Butenandt was quite impressed and thought that virology had been “promoted” from medicine to basic biological science. Viruses could now be studied as model systems for problems in general biology, not just as mysterious pathogens. He immediately began discussing the possibilities for such research with his colleagues. \(^{58}\) Hans Gaffron, a biochemist then working in Wettstein’s group, wrote that Stanley’s ongoing work was of “universal significance to all biology” and even suggested that viruses could be compared with genes. \(^{59}\) This comparison, echoing that of Muller in 1921, connected TMV to the biochemical genetics that Butenandt and Kühn had already been studying. TMV seemed to offer the two researchers a new way to pursue their research on gene expression. \(^{60}\)

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59 Hans Gaffron, “Über das Tabakmosaikvirus,” *Die Naturwissenschaften* 25 (1937): 496. Gaffron was a biochemist who had worked with Carl Neuberg. When Butenandt took over the Institute, Gaffron was temporarily left without a position. Wettstein took him into his division and even helped him secure opportunities to do research in the US. Gaffron emigrated to America in 1937 and later became a member of James Franck’s photosynthesis research team at the University of Chicago. MPG-Archiv, I. Abt., Rep. 1A, Nr. 1538 (1).

In April, 1937, Kühn arrived. In July, Hans Friedrich-Freksa, a zoologist with training in biophysics, joined Butenandt’s group, bringing a fresh perspective and facilitating cooperation with the institute for Biology. Freksa was born in Munich in 1906 and studied zoology, physics, and physiological chemistry in Königsberg and Tübingen. After participating in a zoological expedition to the Dutch East Indies, he worked as a research assistant at the zoological institutes in Tübingen and Frankfurt am Main before joining Butenandt’s group. Shortly after his arrival in Dahlem, workers completed construction of the new building and labs for the institute.\textsuperscript{61}

Since the field of virus research was new and to that point had not been institutionalized in Germany, Butenandt, Kühn, and Wettstein had no model to follow, though they were influenced by the Rockefeller Foundation’s policy of combining traditional fields of research. During the summer of 1937, they decided to proceed in a three-step manner.\textsuperscript{62} The first step was to create an informal community of researchers in order to become familiar with virology and its requirements. The second step was to secure autonomy for this research group within the Kaiser-Wilhelm-Gesellschaft. The third and final goal was to establish a freestanding Kaiser-Wilhelm-Institute for virus research.

One of the most important early decisions was to entrust most of the research to younger scholars. In doing so, Butenandt, Kühn, and Wettstein provided a new generation of investigators with the training and experience that would allow them in turn to have a profound impact on the training of biologists in postwar Germany. The intellectual family tree of postwar German biological research took root in Dahlem in 1937. Wettstein and Butenandt each chose valued co-workers with whom they had

\textsuperscript{61} Butenandt, “Historical development of virus research in Germany,” 936.

\textsuperscript{62} Ibid., 937.
strong personal and scientific relationships. Wettstein selected Georg Melchers. Melchers had begun his studies in Freiburg, then moved to Kiel, and finally to Göttingen, where he completed his doctoral work on microevolution of alpine plants under the direction of Wettstein. When Wettstein moved to Munich, Melchers accompanied him as a scientific assistant and followed him to Dahlem in 1934 as well.\textsuperscript{63} For the biochemical aspects of the virus research, Butenandt chose Gerhard Schramm. Schramm began his university study at Göttingen in 1929. He spent 1930-1932 at the university of Munich (where one of his professors was the Nobel laureate Heinrich Wieland) before returning to Göttingen. After completing his dissertation, Schramm joined Butenandt as a scientific co-worker when the latter moved to Danzig and from there to Dahlem.\textsuperscript{64}

The laboratory training that Schramm received under Butenandt’s direction was a crucial element of the next phase of implementing virus research—equipping the laboratory. Protein was clearly an important part of TMV, so any lab group that hoped to work with TMV had to master the latest techniques in protein chemistry, which differed significantly from traditional organic chemistry. To a large extent, the separation and preparation of compounds in organic chemistry had been achieved by various chemical means, but this strategy was not as effective when dealing with massive protein molecules. Separation by physical characteristics, such as weight and net charge, remained as the most likely ways to purify proteins for analysis.

By the 1930s, new laboratory apparatus made the separation of protein molecules by weight and charge possible. The two most important machines were the ultracentrifuge and the electrophoresis apparatus, both of which were originally built in


\textsuperscript{64} Schramm, Melchers, and Friedrich-Freksa were the central figures in the postwar TMV research; see Chapter 3 for more biographical information.
Sweden. The ultracentrifuge, designed by Theodor Svedberg, used extremely high rotational speeds to simulate intense gravitational pulls. Proteins in solution placed in the ultracentrifuge settled according to their mass; this behavior could be observed and used to estimate precisely the mass of the protein.\(^{65}\) The electrophoresis apparatus, designed by Arne Tiselius, separated molecules according to differences in their electrical charges. Proteins in solution were subjected to an electric field, and the rate at which they migrated toward either a positive or negative pole indirectly revealed their net charge.\(^{66}\) Both machines were large and expensive. Since they measured different qualities, they complemented one another nicely, and both became indispensable parts of a first rate protein chemistry lab.

In 1938, Schramm spent several months in Uppsala learning how to use the new machines properly from Tiselius and Svedberg. He made an excellent impression on his hosts while he was there, and the Swedes remained in close scientific contact with the Dahlem researchers from that point onward, even during the war. More importantly, Schramm acquired the tacit knowledge necessary to make use of the new equipment. When dealing with large, complex apparatus such as the two mentioned above, there was really no training manual to which one could refer—the requisite skills could be developed only through experience, and Schramm’s visit to Sweden can be seen as a kind of brief apprenticeship. Upon his return to Dahlem, the lab purchased a Tiselius electrophoresis apparatus. Since no ultracentrifuge was commercially available,

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Schramm combined his own knowledge with the available published literature to develop an air-driven model that in many ways was superior to that of Svedberg. 67

The virus researchers, well funded by both the Kaiser-Wilhelm-Gesellschaft and private industry, were able to afford the expensive new equipment. Both the KWI für Biologie and the KWI für Biochemie had large running budgets; each division leader received approximately RM 100,000 per year. 68 Butenandt’s close connections with the chemical industry were also important. His hormone research had medical and pharmaceutical implications that he was not reluctant to exploit; for example, his patent on the hormone Progynon earned him RM 160,000 between 1936 and 1940 from the Schering chemical company. 69 He could therefore make a legitimate claim for the practical utility of his research. Heinrich Hörlein, Director of the Elbersfeld Branch of the IG Farben Chemical Trust, was a close friend of both Butenandt and Schramm and supported the Dahlem researchers generously. IG Farben funded the general budgets of the institutes to a considerable extent and also provided money to Butenandt as an individual. In 1938, they gave him RM 40,000 specifically for equipping the virus lab; in


68 In 1938, the budget for the KWI for Biology, all three directors, was RM 286,700. Butenandt’s budget in the KWI for Biochemistry was RM 101,477. Rheinberger, “Virusforschung,” 671.

69 Proctor, “Adolf Butenandt,” 7. In fact, Butenandt’s association with the chemical industry was so close that it made certain officers of the Rockefeller Foundation reluctant to support him. W.E. Tisdale came out in support of Butenandt, saying “As long as Butenandt is, as I think he is, a perfectly honest and sincere man, who is not working scientifically with the idea of cashing in on his energies, I feel that we can do little better than to accept the customs of his country [regarding the collaboration between academic science and industry].” W.E. Tisdale to Warren Weaver, October 8, 1935. RAC, Record Group 1.1, Series 700 D, Box 21, Folder 150.
addition, beginning in January, 1938, Gerhard Schramm’s monthly salary of RM 500 was paid by the Elbersfeld Branch of IG Farben.70

While Butenandt, Kühn, and Wettstein were building the research infrastructure for a modern virology lab, other researchers significantly revised Stanley’s claim that viruses were autocatalytic proteins. In England, plant virologists Frederick Bawden and Norman Pirie showed that TMV was in fact made up of a mixture of protein and nucleic acid. This discovery did not blunt the enthusiasm for viruses as possible models for genes, however; recent research had seemed to indicate that autocatalysis was not a property of proteins alone.71 While earlier research had focused exclusively on proteins as the carriers of heredity, the work of the late 1930s suggested that nucleic acids were also necessary, perhaps serving in a structural or stabilizing role. This new research therefore entailed a shift toward seeing genes as a mixture of the two, called a nucleoprotein.72 This shift happened just as viruses were understood as mixtures of nucleic acid and protein, making the virus-gene analogy suggested in 1921 seem even more appropriate.73

While the overall goal of combining biochemistry with genetics in order to understand the physical nature of the gene was quite exciting, the exact way to do so was

70 Hoechst Archive PSW 22. Auswärtige Mitarbeiter 1933-1945. I would like to thank Stefan Lindner for sharing these documents with me. Also see Deichmann, Flüchten, Mitmachen, Vergessen, 239. For Schramm, see MPG-Archiv, I. Abt., Rep. 1A, Nr. 204 (5).

71 In 1938, Torbijn Caspersson and Jack Schultz published a very influential paper in which they illustrated that the synthesis of nucleic acid within the living cell was closely associated with chromosomal replication. This suggested to them that the ability to synthesize nucleic acid was somehow involved in protein replication. Torbijn Caspersson and Jack Schultz, “Nucleic Acid Metabolism of the Chromosomes in Relation to Gene Reproduction,” Nature 142 (1938): 294-295.


by no means clear. After World War II, the TMV group settled on a three-fold strategy—studying the *structure* of TMV and its components, understanding the *function* of TMV’s protein and nucleic acid components, and making use of *variation* in TMV strains to conduct genetic experiments. However, in the late 1930s biochemical (as opposed to medical) virus research was a very new field, and within this field the German researchers were young and inexperienced. These avenues for exploration became clear only over time. After all, one reason the three directors chose to begin the research in such an informal manner was because they had very little upon which to build and knew that the first couple of years would be spent trying to catch up with Stanley and with Bawden and Pirie. Therefore the first several years of viral research in Dahlem were devoted to becoming acquainted with viruses themselves, understanding how to use the new equipment reliably, and developing interdisciplinary research habits.

In 1938, the outbreak of a viral infection in tomato plants in Dahlem provided the group with its first opportunity to integrate the skills of its researchers toward a common goal—the characterization of an unknown virus. The result of the investigation was a lengthy article published in 1940 that drew on the various backgrounds of all of the above mentioned researchers. Melchers investigated the new virus along classical genetic lines; he reasoned that since the symptoms it caused in tomato plants were similar to those produced by TMV in tobacco, the two viruses were very similar. Drawing upon his practical skills in breeding, characterizing, and growing plants under sterile experimental conditions, Melchers infected various kinds of tobacco plants with TMV and the new

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virus and developed a systematic list of differences and similarities between the two. He suspected that the new virus might simply be a mutant form of the more familiar strain of TMV, which opened the question of what kind of physical/chemical changes were caused in the process of mutation.\textsuperscript{76}

Approaching the problem from the perspective of a biochemist, Schramm made use of the new equipment for protein purification. He determined that the sedimentation constants (the rate at which a substance settled out of solution in the ultracentrifuge) of the two viruses were roughly the same, and that they had similar ultraviolet light absorption spectra. They could not be distinguished through electrophoresis, but Schramm did notice that when a mixture of the two viruses was placed in the Tiselius electrophoresis machine, the new virus migrated more quickly toward the negative electrode. This behavior led him to suggest that the differential behavior was caused by either a lack of basic side chains or the presence of acidic side chains on the amino acids of protein of the new virus. The overall similarity between the two viruses lent credence to Melchers’ speculation that they were closely related.

Freksa, cooperating with Manfred von Ardenne and one of his students in nearby Lichterfelde, brought a powerful new tool to the Dahlem arsenal—the electron microscope.\textsuperscript{77} Prior to the development of the electron microscope, individual virus particles had been invisible to scientific instruments; its inclusion in the repertoire of the Dahlem researchers made their work that much more sophisticated. Nevertheless,

\textsuperscript{76} Ibid., 531-532.

\textsuperscript{77} The electron microscope had been very recently developed in Germany and researchers were still in the early stages of understanding how to use the tool most effectively. Electron microscopes offered much higher resolution than traditional light microscopes due to the much shorter wavelength of the radiation used for imaging, Early electron microscopes had a resolution of several hundred times better than that of the best light microscopes, a figure that has been considerably improved since. For an outstanding introduction to the instruments and science of electron microscopy, see Nicolas Rasmussen, Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940-1960 (Stanford: Stanford University Press, 1997). The origins of the EM are detailed on pages 25-27.
Freksa's work was inconclusive. The differences between the viral strains were so slight that they were not at all visible, even with the tremendous resolving power of the electron microscope. Taken as a whole, though, this paper demonstrated that the research community had fulfilled its preliminary objectives. The Arbeitsstätte für Virusforschung had not made any earth-shaking, long-term discoveries, but it had succeeded in creating a collegial, interdisciplinary research group familiar with the most advanced laboratory technologies in the world.

2.2.2 Institutionalizing the Research Program

The virus research group had been assembled on the initiative of Butenandt, Kühn, and Wettstein. They diverted funds from their own institutes and procured outside support from industry; they provided space in their institutes for the research to take place; they committed their best students to the work; and they facilitated publication of the preliminary research. Now that these goals had been accomplished, the three directors turned their attention toward the next step—the institutionalization of a working group with its own budget within the Kaiser-Wilhelm-Gesellschaft. In September, 1939, as Germany was plunging the entire world into war, Wettstein wrote a draft proposal for the creation of such a group.

He wrote that virus research was a field the greatest developments in which had taken place in America. He summarized the efforts of the Dahlem directors, financially supported by I.G. Farben, to develop virus research in Germany, and suggested that the

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78 Butenandt, Kühn, and von Wettstein served on the editorial boards of journals such as Die Naturwissenschaften and Biologisches Zentralblatt, a situation that greatly speeded up the publication of the younger researchers' work.

79 In a five page document with the date Sept. 1939 written across the top in pencil, Wettstein outlined a plan for making use of an estimated RM 300,000 that was to be made available to him. He saw three areas of research as being of importance; in addition to virus research he suggested the creation of an institute for the collection and research of wild and primitive forms of domesticated plants and increasing the budget of the KWI for Biology. MPG-Archiv, I. Abt., Rep. 1A, Nr. 1538 (3).
best way to ensure an environment in which biological and biochemical research could work together was to create an institute dedicated to this end. The proposed institute was to have three divisions, chemistry, botany, and zoology, and Schramm and Melchers were named as leaders of the first two, respectively. In order for construction to commence, Wettstein wrote that a sum of at least RM 100,000 was necessary. He added that the yearly budget of such an institute would need to be at least RM 150,000.

On July 19, 1940, the three directors sent a formal proposal for the creation of an “Arbeitsstätte für Virusforschung” (“Branch Laboratory for Virus Research”) to the general administration of the Kaiser-Wilhelm-Gesellschaft. This proposal repeated many of the arguments of Wettstein’s earlier draft. In order to reach the cutting edge of research, the Germans wrote that they needed to learn from the strengths of the best American research groups, in particular those supported by the Rockefeller Foundation. Invoking the Rockefeller Foundation justified the emphasis the three directors placed on interdisciplinarity, which was to be implemented by making the new virus center a joint effort between the institutes for biology and biochemistry until it became fully independent. Depicting German research as lagging behind that of the United States was also undoubtedly a rhetorical strategy intended to justify the costs of the new research center in the nationalistic context of a country at war. Modern research was not cheap, and the three directors argued that the most advanced research apparatus were absolutely necessary in order for German research to compete with that of America. By this time the estimated budget had grown tremendously, and stood at RM 800,000 for construction and an operating budget of RM 250,000. By early 1941, these numbers were revised downward to RM 500,000 for construction costs and RM 140,000 for the operating

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budget.\textsuperscript{81} These were extremely large sums of money, especially when one considers that in 1938 the yearly salary of a junior scientist such as Schramm was RM 6,000.

Raising this kind of money was beyond the ability of the directors and necessitated firm support from the general administration of the Kaiser-Wilhelm-Gesellschaft, which in practice meant the support of its secretary, Ernst Telschow. After his appointment in 1937, Telschow became one of the most influential individuals in the KWG. He had received his doctorate in chemistry in 1911, supervised by Otto Hahn. Telschow served in World War I, and afterward went to work in his father’s famous bakery, the Telschow Konditorei. Telschow joined the Nazi party in 1933, earned a reputation as a reliable National Socialist, and his appointment as secretary of the KWG was thus a step toward coordinating the society with Nazi political goals.\textsuperscript{82} At the time of the creation of the virus research center, Telschow’s influence was at its zenith. The KWG’s president, Carl Bosch, died in April, 1940, and because of struggles between the society and the regime over his successor, the new president, Alfred Vögler, did not take office until June, 1941. Therefore, during the creation of the Arbeitsstätte für Virusforschung Telschow was effectively acting president. His support was therefore crucial if the directors hoped to achieve their goals.

Telschow provided the requisite backing, probably because he was convinced that the project was worthwhile scientifically and might have important practical applications

\textsuperscript{81} “Bauplan und Vorschlag für die Zweigstelle für Virusforschung.” MPG-Archiv, I. Abt., Rep. 1A, Nr. 2906 (3). The later drafts of the proposal named two other researchers to important positions in the research center. Rolf Danneel was named director of the zoological division. Danneel completed his doctoral work in Göttingen in the late twenties and had briefly been an assistant at the zoological institute before moving on to Berlin and then to Königsberg. In 1939, he came to Dahlem to work with Kühn and became his representative in the new group. In addition, a satellite lab dedicated to insect virology under the direction of Gernot Bergold was added. Bergold had already been pursuing his own research in Oppau under the direct support of I.G. Farben and Heinrich Hörlein. Lebenslauf, Rolf Danneel, MPG-Archiv, I. Abt., Rep. 46, Nr. 8. Danneel had also been a close friend of Butenandt’s in their undergraduate years in Marburg. Karlson, Adolf Butenandt, 31. Bergold’s contributions to the virus research group are thoroughly discussed in Rheinberger, “Virusforschung.”

\textsuperscript{82} Macrakis, Surviving the Swastika, Chapter 5 “The Turning Point 1936-1939.”
in agriculture. He began to secure funding for the center on July 15, 1940, by writing to Herbert Backe, a member of the Kaiser-Wilhelm-Gesellschaft Senate as well as a secretary in the Reichsministerium für Ernährung und Landwirtschaft (Reich Ministry for Food and Agriculture—RMEL). Telschow requested funds for three programs—the virus research center, a center for breeding research in Müncheberg, and a center for animal breeding research in Dummerstorf. He requested a sum of RM 1-1.5 million to establish all three centers and justified the expense by arguing that all three were imperative for agricultural and livestock research. He did not receive all the funds he hoped for from this source, however—in 1941 the RMEL committed only RM 50,000.\footnote{Letter, Telschow to Backe, July 15, 1940. MPG-Archiv, I. Abt., Rep. 1A, Nr. 2906 (2). Letter, RMEL to Telschow, May 17, 1941. MPG-Archiv, I. Abt., Rep. 1A, Nr. 2907 (1).}

The bulk of the funding for the creation of the new institute came from the Deutsche Industriebank, which Telschow had first solicited in the fall of 1940. By the summer of 1941, the bank had confirmed its commitment of RM 500,000 for the virus center.\footnote{Telschow originally wrote to the director of the Deutsche Industriebank on November 15, 1940, requesting RM 1.5-2 million. His justification was a reiteration of that of the three directors—the quality of Stanley’s research in the United States and the importance of virus research for practical subjects such as agriculture. On January 13, 1941, Telschow wrote back wondering why he had not received a reply. On January 15, Bötzke replied by promising to support the center, but was unsure as to where the funds would come from. In a letter to Telschow dated Feb. 15, he said the money had been found. All letters MPG-Archiv, I. Abt., Rep. 1A, Nr. 2906 (3). In a letter dated June 18, 1941, the Deutsche Industrie Bank reiterated its firm commitment to provide RM 500,000. MPG-Archiv, I. Abt., Rep. 1A, Nr. 2907 (1). Since the war forced the evacuation of Dahlem in 1943, the proposed virus center was not built and most of the money presumably was never spent.}

The transformation of the virus research community into the Arbeitsstätte für Virusforschung took place on April 1, 1941. Wettstein testified to the importance of its work before the Senate of the Kaiser-Wilhelm-Gesellschaft later that summer. Thanks to the preliminary work and dedication of Butenandt, Kühn, and Wettstein, the early financial support of Heinrich Hörelin and I.G. Farben, and Telschow’s practical assistance, the Senate unanimously ratified the creation of the Arbeitsstätte für
Virusforschung at their meeting of July 31, 1941. Backe, Hörlein, Telschow, and Wettstein, all committed supporters of virus research, were among the senators present.85

2.2.3 The Scientific Results of the Research Program

At 3:15 a.m. on June 22, 1941, just over two and a half months after the formal creation of the Arbeitsstätte für Virusforschung, the German army opened fire on the Soviet Union following preliminary air attacks. By 5:30 a.m. the German government had delivered what amounted to a declaration of war, concluding with the statement “Thereby the Soviet Union has broken its treaties with Germany and is about to attack Germany from the rear, in its struggle for life. The Führer has therefore ordered the Wehrmacht to oppose this threat with all means at its disposal.”86 Thus began the greatest land war in history, and within days nearly 3 million German soldiers and their allies poured into Soviet Russia. The German attack took the Soviet Union by surprise and rolled forward encountering relatively little resistance. By late July, when the Senate of the KWG was unanimously approving of the creation of modern virus research in Nazi Germany, the military situation in the Soviet Union had deteriorated to the point that General Zhukov, at that time chief of the Soviet General Staff, recommended abandoning the city of Kiev in order to shore up the defenses of Moscow. Stalin reacted by dismissing Zhukov, and Kiev fell by September, yielding hundreds of thousands of Soviet prisoners of war.87 Central to the German plan of conquest was a desire to

85 “Niederschrift über die Sitzung des Senats der Kaiser Wilhelm-Gesellschaft zur Förderung der Wissenschaften am Donnerstag, dem 31.7.1941, 16 Uhr, im Harnack-Haus, Berlin Dahlem.”


87 The meeting between Stalin and Zhukov took place on July 29, 1941, two days before the Senate meeting of the KWG. Erickson, Road to Stalingrad, 177-178.
colonize the east agriculturally in order to provide the agricultural self-sufficiency that the German nation had lacked since the late nineteenth century.\textsuperscript{88}

The outbreak of war with Soviet Russia did not disrupt the TMV research until 1942-1943, when the capital city of Berlin came under sustained aerial bombardment from the Western Allies. At that point, the entire research group was evacuated to the city of Tübingen in southern Germany, where work continued intermittently until the end of the war. While most of their time was spent on TMV, they also worked with other plant and animal viruses as well, lending credence to their claim that their research had important agricultural applications. To maintain the clarity of the narrative, though, I shall deal with only the most significant discoveries relating to TMV.

In a series of papers published in 1942-1943, Schramm and collaborators outside the virus center used the incorporation of radioactive phosphorous to study the growth cycle of the virus. This work adds another dimension to the picture of the Berlin biological community. Early in the 1930s, the Rockefeller Foundation had donated a great deal of money to found a Kaiser-Wilhelm-Institute für Hirnforschung (Kaiser-Wilhelm Institute for Brain Research) in Berlin-Buch, a suburb north of the city center. The institute had a division for genetics, where Nicolai Timoféeff-Ressovskiy worked. In the 1930s, Timoféeff was very interested in the role of mutation in evolution and began experimenting with x-rays as a way of inducing genetic mutations.\textsuperscript{89} He became one of

\textsuperscript{88} As part of this plan of agricultural colonization, several new Kaiser-Wilhelm-Institutes relating to agriculture were opened in conquered territory. See Kristie Macrakis, “The Ideological Origins of Institutes at the Kaiser-Wilhelm-Gesellschaft in National Socialist Germany,” in Science, Technology, and National Socialism, eds. Monika Renneberg and Mark Walker (Cambridge: Cambridge University Press, 1994), 139-159.

\textsuperscript{89} Jochen Richter, “Das Kaiser-Wilhelm-Institut für Hirnforschung,” and Josef Reindl, “Believers in an Age of Heresy? Oskar Vogt, Nikolai Timoféeff-Ressovskiy, and Julius Hallervorden at the Kaiser-Wilhelm-Institute for Brain Research,” in Szöllösi-Janze, ed., Science in the Third Reich, 211-242. The researchers in Berlin-Buch worked on Nazi projects such as nerve gas. They also believed in the power of genetics to mold humanity along lines of their choosing. According to the historian Karl Heinz Roth, their thinking was influenced by an international “paradigm shift” in genetics that equated modern genetics more closely with eugenics. This agenda was articulated at the international congress of genetics in Edinburgh in 1939. For these investigators, it was a short step from scientifically sanctioned eugenics to the racial
Germany’s most important radiation biologists, and in collaboration with Karl Zimmer and Max Delbrück published an extremely important paper on the physical nature of gene mutations in 1935.  

The radioactive phosphorus work combined Schramm’s biochemical expertise with the radiation biology of Timoféeff’s division. Since phosphorus was an important component of the virus (it holds the backbone of the viral nucleic acid, RNA, together), the researchers added radioactive phosphorous to tobacco plants. They later infected these plants with the virus and traced the radioactive phosphorous as it was incorporated into the virus during the process of infection. They found that the rate of virus reproduction was much greater than the normal rate of protein manufacture of the host plant. They concluded that the reproduction of the virus did not depend on the plant’s normal physiological processes, but that instead the viruses were directly assembled within the tissue of the living plants.

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90 Known as the “three-man-work,” this article became extremely influential despite the fact that it was published in a poorly distributed German journal that limited its accessibility. N. W. Timoféeff-Ressovsky, K. G. Zimmer, and M. Delbrück, “Über die Natur der Genmutation und der Genstruktur,” Nachrichten der Gesellschaft der Wissenschaften in Göttingen 6 (1935): 189-245. See discussion in Olby, Path to the Double Helix, 231-233.


92 Schramm, Born, and Lang, “Markierung,” 473.
Melchers continued to study the relationship between different strains of the same viruses. In a paper submitted in December, 1941, he followed up on the work on the new virus encountered in 1938. He determined that the new virus was a close relative of the normal strain of TMV and argued that the two were in fact mutants of one another. He used the term "parallel mutants," meaning that in most aspects the two were the same, but that they each had a difference in the same gene giving rise to two different sub-species. 93

In 1943, Schramm published what was to become the group's most significant contribution to the long-term history of virology. 94 He exposed TMV, which is usually very stable, to a high pH level (above 9) and found that the virus particle broke down into sub-components, including smaller pieces of protein and nucleic acid and nucleic acid-free pieces of protein. The sub-components were incapable of replication. The most interesting element of the work was that the sub-components could spontaneously re-aggregate into a TMV shaped particle when placed in a weak alkaline solution. The reconstituted particle itself was not capable of replication or infection either. This behavior led Schramm to conclude that TMV was made of roughly 70 equal sub-components (this number has since been revised upwards). Furthermore, he concluded that the size and shape of TMV particles was not determined by nucleic acid, nor did the nucleic acid hold the molecule together. Instead, the shape of the particle resulted from the viral components settling into an energetically favored configuration.


2.3 Virus Research and Total War, 1941 to 1945

2.3.1 Research for Autarky

The war in the east also presented possibilities for scientific researchers. By conquering vast amounts of territory, the Nazi leadership hoped to make Germany economically self-sufficient. As a consequence of this drive for autarky, scientific research in agriculture became a necessary part of turning military success on the Eastern Front into independence in food and raw materials. Scientists now had the opportunity to identify themselves with the interests of a fatherland at war rather than with the National Socialists per se. In this environment, their own self-interest combined with an ingrained sense of loyalty to the state to encourage many researchers in the biological sciences to put their talents at the disposal of its country’s political leadership. In the case of aerospace research, Helmuth Trischler has called this phenomenon the “self-mobilization” of science, and his conclusions are equally valid for biology researchers. The commonality of goals, if not ideology, made the political control of the scientific community unnecessary, and researchers became ready collaborators in the colonization of the East.95

The desire to achieve agricultural self-sufficiency predated the Nazis.96 The blockade maintained by the Entente powers in the First World War had devastated the German population, resulting in mass starvation, malnutrition, and disease.97

95 For more on Trischler’s idea of self-mobilization, see section 1.2.1; also see Susanne Heim, “Research for Autarky: The Contribution of Scientists to Nazi Rule in Germany,” Forschungsgprogramm "Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus" Ergebnisse 4, 2001, 23. In his study of the Nazi ballistic missile program, Michael Neufeld notes “For both patriotic and selfish reasons, leader of academic institutions eagerly sought military projects that would provide funding and draft exemptions.” Michael J. Neufeld, The Rocket and the Reich: Peenemünde and the Coming of the Ballistic Missile Era (Washington DC: The Smithsonian Institution, 1995), 82.

96 For an excellent cultural study of the impact of the east on the thinking of the German occupiers, see Vejas Gabriel Liulevicius, War Land on the Eastern Front: Culture, National Identity and German Occupation in World War I (Cambridge: Cambridge University Press, 2000).

97 The average diet in WWI Germany declined from 3400 calories in 1914 to 1000 calories in 1918. Holger Herwig suggests that postwar estimates of 800,000 civilian deaths resulting from the blockade might be
Consequently, self-sufficiency in food and in fodder crops for livestock became a national security priority for Germany in the interwar years. For these purposes, the Reich Ministry for Food and Agriculture (RMEL) was formed in 1920 to assist in the resuscitation of German agriculture, and soon turned its research efforts toward, among other projects, combating plant illnesses and parasites. The RMEL also supported directed breeding projects targeted at producing new crops (such as a modified version of the lupine) that would be fit either for human or animal consumption. The ministry supported a number of research institutes and enlisted the aid of Germany’s best geneticists and academic plant researchers. The result was that well before the Third Reich, agricultural research had been turned toward political ends as a means of achieving the desired national goal of agricultural self-sufficiency.98

Nazism exacerbated this trend. Hitler’s overarching goal was victory in the struggle against Bolshevism, which required an attack on the Soviet Union. Such an attack in turn necessitated mobilizing the economy to support a sustained rearmament program, manifested in the Four-Year Plan, which Hitler announced in the fall of 1936.99 Just as the Four-Year-Plan was a mobilization of the economy for the purposes of war, Nazi agricultural policy was also a program of war preparation.100 Early Nazi agricultural policy had actually harmed productivity, resulting in a significant decline in the yields of cereal grains and fodder crops in the mid-1930s.101 The decline in the

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100 Wieland, “Wir beherrschen den pflanzlichen Organismus besser, 203.

availability of domestically produced agricultural raw materials had had a two-fold impact on the autonomy of Nazi Germany. Insufficient access to raw materials meant that in the case of war, Germany would again be unable to feed its population, but it was also a threat to military mobilization as well. In the 1930s and throughout the entire Second World War, more than half of the German artillery was horse drawn, necessitating a secure supply of fodder crops. 102 The pressing need to improve crop yields made geneticists, botanists, and any other researchers who could convince the relevant authorities that their work was directed toward the improvement of agricultural productivity, potential allies of the state.

As with almost everything else that Hitler did, he had no clear idea of how to implement this program. His purpose was to achieve high levels of self-sufficiency in agriculture and raw materials in order to promote rearmament, but his vagueness allowed his subordinates to choose how to best fulfill the Plan’s goals as according to their own interpretations. From the perspective of academic research, the best strategy was to make one’s research compatible with the program of autarky in order to receive financial support from government ministries like the RMEL. Within the Kaiser-Wilhelm-Gesellschaft, several institutes, such as that for breeding research in Müncheberg, sought

102 John Keegan, The Second World War (New York: Viking, 1990) 44. Although the image of Germany’s blitzkrieg campaigns of the early war call to mind a thoroughly modern army dominated by armor and aircraft working in close coordination with one another, in fact armor made up a very small part of the army, most of which looked like the armies of World War I—infantry, cavalry, and horse-drawn artillery. In his recent study of the Battle of Stalingrad, Anthony Beevor wrote that because of the German army’s dependence upon foot soldiers and horse drawn artillery, the army’s speed of advance “was unlikely to be much faster than that of the Grande Armée in 1912. Anthony Beevor, Stalingrad: The Fateful Siege: 1942-1943 (New York: Viking/Penguin, 1998), 13-14. In fact, in 1940, the armor divisions of the German army were numerically and perhaps even qualitatively inferior to those of the French, obviously, they were used to much better effect, though. Their weakness in this respect was eventually exposed when Germany attacked Soviet Russia in 1941. Omer Bartov, Hitler’s Army: Soldiers, Nazis, and War in the Third Reich (New York: Oxford University Press, 1991), 12-18; also see Bernard R. Kroener, “Squaring the Circle: Blitzkrieg Power and Manpower Shortage, 1939-1942,” in Wilhelm Deist, ed., The German Military in the Age of Total War (Dover, NH: Berg, 1985), 282-303.
support from the ministry.¹⁰³ In order to receive the desired funding, researchers had to convince Herbert Backe of the utility of their work.

Backe was a secretary in the RMEL in the 1930s, ostensibly subordinate to Walther Darré, who was agricultural minister at the time. By 1940, Darré’s indecisiveness and romanticism had weakened his influence, while that of Backe, appointed Food Commissioner for the Four-Year Plan in 1936, was on the rise. He officially replaced Darré in 1942.¹⁰⁴ He was also a senator in the Kaiser-Wilhelm-Gesellschaft and a vice-president from 1941-1945. He tended to promote practically oriented research along National Socialist lines, and his appointment in 1936, coinciding with the creation of a centralized German Research Council (Reichsforschungsrat—RFR) meant that 1936-1937 marked more of a turning point in the relationship between research and the state than did 1933.¹⁰⁵ From that point onward, the ability to depict one’s research as having practical implications for the realization of the program of autarky became an increasingly effective way to win financial support.

For the virus research group, the years 1940-1941 became a turning point in their relationship with the state, for it was then that the group had to seek patronage instead of relying on the support of Butenandt, Kühn, and Wettstein. These years were the time of Telschow’s greatest power as well as of the rise of Backe, making these two men indispensable for the support of the virus research program. As was mentioned earlier, Telschow’s first request for financial support went to the RMEL and was addressed to

¹⁰³ Heim, “Research for Autarky,” the most thorough coverage of the impact of Nazism on academic plant research is Wieland, *Wir beherrschen den pflanzlichen Organismus besser*, Chapter 6, “‘Forschung für Volk und Nahrungsfreiheit’—die NS Zeit.”


Backe, not Darré, although the latter was still theoretically in charge. In his request, Telschow argued that by supporting the virus center as well as the above mentioned institute for breeding research in Müncheberg and that for animal breeding research in Dummersdorf, the RMEL would be supporting the three most important fields for the sustenance of the German people.  

In this context, the Dahlem virus researchers justified their requests for funding. The connection between agricultural self-sufficiency and biochemical virus research is not so distant as might at first appear. The virus researchers did not limit themselves to TMV but studied a number of viruses, including those that caused livestock illnesses such as hoof-and-mouth disease. Although the use of viruses as model systems seemed to provide a way to answer some of the more general problems in biology, the understanding of viruses as pathogenic organisms promised to safeguard agriculture from hazards to valuable crops such as sugar beets and potatoes as well. In his original 1939 draft proposal, Wettstein argued that virus research had medical and agricultural applications, and Telschow stressed these utilitarian aspects of the work in his fundraising efforts. The leadership of the RMEL was convinced that virus research would contribute to Germany’s agricultural self-sufficiency and they provided much of the funding for the Arbeitsstätte für Virusforschung during the war years. The rest of the

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106 "...den drei wichtigsten Gebieten unserer Volksnährung..." Telschow to Backe, July 15, 1940. MPG-Archiv, I. Abt., Rep. 1A, Nr. 2906 (20). During the war, the policy of agricultural autarky justified not only the funding of the virus center, but also the creation of three new Kaiser-Wilhelm-Institutes for agriculture and biology in recently conquered lands of central Europe. Macrakis, "The Ideological Origins of Institutes at the Kaiser-Wilhelm Gesellschaft."

107 Schramm discussed this in a 1944 article, writing that a theoretical understanding of the structure of virus particles would provide starting points for applied research against viral illnesses in people, animals, and plants. Gerhard Schramm, "Über die Konstitution des Tabakmosaikvirus," Die Chemie 57 (1944): 113.
center’s funding came primarily from other government ministries. (See Table 2.1). In this way, academic virus research became enmeshed in the strategy of autarky.¹⁰⁸

### Table 2.1:  Income and Expenditures of the Arbeitsstätte für Virusforschung, April 1, 1941-March 31, 1944 (All amounts in Reichsmarks)

<table>
<thead>
<tr>
<th>Fiscal Year</th>
<th>Income</th>
<th>Expenditures</th>
<th>Balance</th>
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</thead>
<tbody>
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<td>1941</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Government</td>
<td>60,000.00</td>
<td>Personnel</td>
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<td>Interest</td>
<td>47.55</td>
<td>Equipment</td>
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</tr>
<tr>
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<td></td>
<td></td>
</tr>
<tr>
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<td>March 31, 1943 Carryover</td>
<td>7.11</td>
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<td></td>
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<tr>
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<td>76,971.22</td>
<td>+49,693.46</td>
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<tr>
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¹ Financial support for the Arbeitsstätte für Virusforschung came from ministries in the Federal government and the state government of Prussia; the greatest single contributor was the Reichsministerium für Ernährung und Landwirtschaft, which provided RM 60,000 in 1941, RM 55,000 in 1942, and RM 141,100 for the 1944 fiscal year, of which half was probably never dispersed. The researchers also received funds from I.G. Farben and the Deutsche Forschungsgemeinschaft in addition to those listed above.

#### 2.3.2 Pure or Applied Research?

For all their rhetoric of independence and purity, scientific researchers in Nazi Germany were completely dependent upon outside sources for funding. They also depended upon their fellow scientists in other countries for vital feedback and professional prestige. Satisfying the requirements of these two very different communities necessitated creative wordplay on the part of the TMV researchers, who

¹⁰⁸ Ute Deichmann has also noted that such was the importance of genetic studies to Nazi ideology that the Nazi government supported many researchers simply because they were geneticists—further justification was not necessarily required. *Biologists under Hitler*, 119-120.
separated the complex process of science into a false dichotomy of “pure” and “applied” branches of research. When speaking to the international community of scientists, the virus researchers stressed the merit of their work as pure, objective research. In this way they distanced themselves from the regime and used the idea of the purity and independence of scientific research to absolve themselves of responsibility for the actions of their nation. In the context of National Socialist Germany, however, the TMV researchers legitimated their work as applied agricultural research. As the war progressed and their isolation from the international scientific community grew, the need to appear important to the state became increasingly important. By the early 1940s, utility to the state acquired a designation particular to Nazi Germany at war—Kriegswichtig (War-Related). Representing oneself or one’s work as important for the war effort became more than means to secure funding. It became a survival strategy for these people, exempting them from military service and facilitating their evacuation to areas relatively untouched by the Allied bombing campaign.

Butenandt, Kühn, and Wettstein tended to view their activities as “pure” research, but they recognized that they would also have to stress the potential applications of TMV research at the appropriate times. In the 1939 draft proposal to create the virus center, Wettstein argued that virus research should be supported in Germany since it was already established in America as both pure and applied science; for him the “pure” aspects of the research were potentially the most significant. The formal proposal for the creation of the virus center, dated July 19, 1940 and signed by all three, reiterates the dual nature of

109 I use the term Kriegswichtig since it is the term used by Butenandt to describe the work of the Kaiser Wilhelm Institute for Biochemistry; although war-important is a more literal translation, war-related makes for a smoother translation. The designation often used in official correspondence of the time was uk status; uk = unabhägig = indispensable. See Mark Walker, German National Socialism and the Quest for Nuclear Power 1939-1949 (Cambridge: Cambridge University Press, 1989), 42.

110 “rein und angewandte Wissenschaft,” MPG-Archiv, I. Abt., Rep. 1A, Nr. 2906 (1)
the research and the desire of the involved parties to focus on the basic problems thereby laying the basis for future practical applications. However, when speaking before the German Chemical Society in 1942, Butenandt chose to speak about only the potential for virus research to answer fundamental biological problems. He articulated four similarities between viruses and genes and suggested to his audience that the structural analysis of virus particles would lead to a better understanding of the way that genes transmit biological information. In this talk, the work of the virus center appeared not as part of a program of autarky but as part of the early history of molecular biology. When the Senate of the Kaiser-Wilhelm-Gesellschaft approved the guidelines for the new virus center in June, 1941, its task was clearly defined as pure or fundamental research.

When presented to a non-scientific audience, however, the work of the virus researchers tended to take a much more utilitarian character. For example, a short paragraph announcing the creation of the Arbeitsstätte für Virusforschung that ran in a number of papers in January, 1942, mentioned that virus research was important both for basic scientific work as well as for “maintaining the health of the people.” That same year, both Schramm and Danneel published articles about their work in popular magazines. Both men stressed the practical applications of virus research; Danneel, for


\[113\] MPG-Archiv, I. Abt., Rep. 1A, Nr. 2906 (4). Paul Weindling has noted this “Jekyll and Hyde” behavior among German researchers applying to the Rockefeller Foundation for funding. When applying for state funds they stressed the social relevance of their work, but when they applied to Rockefeller in the US they became internationalists, stressing humanitarian aims and the importance of basic research. Weindling, “The Rockefeller Foundation,” 136.

\[114\] “. . . die Gesunderhaltung des Volkes...” See, for example *Völkischer Beobachter*, Berlin, 24 January 1942.
animal and human illnesses, and Schramm for crop diseases. Biological chemistry and virus research seem to have served the public relations of the Nazi government more than they did the public health of the German population.

Kriegswichtig status conferred advantages in securing material and financial support for the Kaiser-Wilhelm-Institutes that other research centers, especially the universities, did not have. As the tide of the fighting turned, however, it conferred another very important advantage upon scientists—that of survival. On August 26, 1943, Albert Speer ordered the evacuation of the institutes for biology and biochemistry to the south German cities of Hechingen and Tübingen, respectively, stating that their work was important for the war effort as well as the armaments industry. The virus center went with the Institut für Biochemie to Tübingen. Butenandt himself had chosen the site and recommended it to Speer; before Speer issued the actual evacuation order, Butenandt already had contacted Dr. Otto Stickl, rector of Tübingen University, asking for his cooperation in creating a temporary location for the “war related work of the Kaiser-Wilhelm-Institut für Biochemie.” Butenandt’s willingness to associate his work with

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115 Rolf Danneel, “Virus-Krankheiten. Einige Ergebnisse und Probleme der Virusforschung,” Westermann’s Monatshefte, March, 1942, 357-360; Gerhard Schramm, “Viruskrankte Pflanzen,” Das Reich 16 (April, 1942): 1-2. An unsigned newspaper article from the same time period discusses the virus center’s work almost exclusively in terms of its ability to counter viral illnesses in plants and animals; it even mentions that the virus responsible for hoof and mouth disease was being studied in Dahlem. The last paragraph is the only one that mentions the importance of viruses as models for studying heredity. “Was versteht man unter Virusforschung? Deutsche Forscher im Kampf gegen Seuchengefahr.” Universitätsarchiv Tübingen 117/13634.

116 Ute Deichmann and Benno Müller-Hill have pointed out that although funding for biological research increased throughout the war, in reality most of this research had very little to do with winning the war. Deichmann and Müller-Hill, “Biological Research at Universities and Kaiser-Wilhelm Institutes in Nazi Germany,” in Renneberg and Walker, eds., Science, Technology, and National Socialism, 160-183.

117 Letter, Albert Speer to Stuttgart Gauleiter Murr, August 26, 1943. Universitätsarchiv Tübingen 117/13634. The two institutes were part of a much longer list of scientific institutes that were evacuated from Berlin.

118 Letter, Adolf Butenandt to Dr. Stickl, Rector of Tübingen University, July 2, 1943. Universitätsarchiv Tübingen 117/13634.
the war effort in order to expedite the evacuation of his institute was an entirely sensible one given the conditions of the time. By mid-1943, the air war was taking a terrible toll on Berlin. In the aftermath of a raid on March 1, Butenandt and Werner Heisenberg spent several hours walking from Potsdamer Platz to Dahlem and saw the devastating results of the Allied attacks first hand.119

Furthermore, in the early years of the war, scientists and workers from the Kaiser-Wilhelm-Gesellschaft had not been exempt from military service—roughly 31 per cent of the male staff members had been drafted into the armed forces by 1941. One of them, Erich Becker, a member of Kühn's group who had published with Butenandt, was killed in his first action on the Russian front in August, 1941.120 Consequently, association with the war effort could literally be the key to personal survival.121 After the war, Georg Melchers wrote that Wettstein was able to keep scientists out of military service by saying that they were involved with important war related research on phosphorescent sea life, research that in fact was obsolete and of no military value whatsoever.122 According to one account, Butenandt kept his student Wolfhard Weidel out of the Wehrmacht by creating bogus projects for him that were of "potential" military significance.123


120 Letter from Fritz von Wettstein dated September 22, 1941. MPG-Archiv, I. Abt., Rep. 1A, NR. 1538 (5); in a letter to Karl Henke dated Sept. 21, 1944, Alfred Kühn also mentioned that many young scientists had been killed in Russia. MPG-Archiv, I. Abt., Rep. III, Nr. 58 (14).

121 Deichmann, Biologists under Hitler, 170-3; Proctor, The Nazi War on Cancer, 259. This was also true for physicists involved with the nuclear power program; see Walker, German National Socialism, 47.

122 Melchers, "Fritz von Wettstein," 396. Alfred Kühn indicated that Wettstein used this same strategy to have a scientist named Greis released from Wehrmacht service. Letter from Alfred Kühn to Ernst Telschow dated March 2, 1945. MPG-Archiv, I. Abt., Rep. 1A, Nr. 1538 (8).

123 Gunther Stent, an émigré and friend of Weidel's after the war, wrote "During the war, Wolf [Weidel] worked at the KWI for Biochemistry in Berlin. He was saved from being reported missing on the eastern front because Butenandt, the institute's director, managed to cook up some chemical research projects of potential military importance for him. One of them was the development of a magic pill, which, when dropped covertly by a German secret agent into a 1,000-gallon Allied gasoline tank, would ruin the fuel. After a year or so of research, Wolf came up with a substance of which the stealthy German saboteur would
Legitimating scientific research in such a context-dependent manner was not unique to National Socialist Germany. In the United States, George Beadle was pursuing a program in biochemical genetics that had many similarities to the one pursued by Butenandt and Kühn.¹²⁴ Beadle was interested in exploring the relationship between genes and enzymes from an intellectual standpoint, but he did not hesitate to stress the practical relevance of his research in order to maintain his funding and autonomy during the Second World War. In another parallel with the Germans, Beadle used his research to secure draft deferments for his students and assistants.¹²⁵ After the war, Wendell Stanley secured financial support from the state of California by promising agricultural benefits from his TMV research. According to Lily Kay, "Virus research was 'sold' to the people of California through its medical and agricultural benefits, yet control of the program remained in the hands of the biochemists and virologists, rather than being administered through agricultural and medical institutions."¹²⁶ The two research programs mentioned here, biochemical genetics and TMV, were closely related to those of the Dahlem group, demonstrating that the selective marketing of these lines of research was a normal part of scientific practice, whether or not the host country was attempting to conquer Europe.

¹²⁴ Both Beadle and the German researchers had begun their research by studying chains of biochemical reactions in insects. The Germans had a great success in 1940, as we shall see in Chapter 4. In response, Beadle used a much simpler organism, the mold Neurospora, as a model system and had great success, winning the Nobel Prize in 1958.


Was the work of the Arbeitsstätte für Virusforschung pure or applied research? The answer, of course, is both, since the virus researchers saw their work as having both basic and applied aspects that were inseparable. They consciously used this false dichotomy as what Alan Beyerchen has called “code words for the legitimation of research activities” as they interacted with various audiences in the Third Reich.\(^{127}\) They themselves preferred identifying their own activities as intellectually prestigious pure research, but they understood that in Nazi Germany practical results were expected of them. The researchers told their various audiences what they wanted to hear, and in doing so gave themselves a measure of flexibility. TMV investigators could claim that their work was applied enough to justify state support under Nazism, but fundamental enough that it could be continued in the postwar era.\(^{128}\) They themselves did not take this distinction very seriously, however. In reflection Gernot Bergold, who worked in the Arbeitsstätte für Virusforschung’s satellite lab in Oppau, said: “To differentiate between ‘basic’ and ‘applied’ research is bureaucratic/semantic nonsense.”\(^{129}\)

2.3.3 “Biological Chemistry in the Service of the Volksgesundheit”

In a remarkable speech given before the Prussian Academy of Sciences in January, 1941, Butenandt engaged in just such an exercise in semantic nonsense in order to sell his research to the regime. Entitled “Biological Chemistry in the Service of the Nation’s Health” (Die biologische Chemie im Dienste der Volksgesundheit), the talk

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\(^{127}\) When discussing the development of science in Imperial Germany, especially in institutes that received a mix of public and private funding, as did the Kaiser-Wilhelm-Gesellschaft, Beyerchen has argued that our confusion regarding the overall excellence of Wilhelmian science is perhaps attributable to such a confusion in terminology. As we have seen, this idea can be generalized to scientific research as a whole. Beyerchen. “On the Stimulation of Excellence in Wilhelmian Science,” 142-143.


documented, at length, the importance of chemistry research for a nation at war.\textsuperscript{130} Early in the speech, Butenandt stressed that many of the innovations he was discussing had \textit{not} been produced by programs of directed, applied research, but followed from the desire for pure knowledge. He urged that such research be allowed to continue in Germany, and promised that the uses to which it could be put were many and diverse. He spoke at length of chemical research on vitamin isolation and synthesis and gave specific examples of how vitamin supplements could counteract specific illnesses, such as rickets, that resulted from dietary deficiencies. Next, he discussed the merits of hormone therapy. The most significant potential application of hormone therapy was to restore fertility to German women with hormonal imbalances and thus to increase the overall reproduction rate of healthy Germans. He also discussed the importance of biochemical research for combating bacterial infections as well as understanding cancer. Not surprisingly, he concluded his speech with comments on the importance of virus research, both as a means of solving fundamental problems in biology as well as overcoming sicknesses caused by viruses.\textsuperscript{131}

This talk was striking because of Butenandt’s militaristic tone, which was absent in his other wartime publications. Butenandt’s desire to present scientists as indispensable contributors to a nation at war permeates the speech. He portrayed chemistry as the key to autarky, calling the attainment of security in raw materials for the entire economy the chief task of chemistry. In addition to economic self-sufficiency, he assured his that advances in chemistry would enable the manufacture of gasoline via the liquefaction of coal, the production of artificial rubber, and the synthesis of raw materials

\textsuperscript{130} Adolf Butenandt, “Die biologische Chemie im Dienste der Volksgesundheit,” \textit{Preussische Akademie der Wissenschaften Vorträge und Schriften, Heft 8} (Berlin: Verlag Walter de Gruyter, 1941).

\textsuperscript{131} Ibid., 5-6; vitamins, 8-11; hormone therapy, 12-14; antibodies and bacterial illnesses, 15-16; cancer, 18, virus research, 19-20.
for explosives and munitions. He said that scientists and soldiers stood together in close 
solidarity in the struggle for life, and even quoted Napoleon to strengthen his case for the 
importance of science to a nation at war. Finally, he credited the National Socialist 
government with providing the support for these scientific advances. 132

This talk has understandably drawn attention to Butenandt’s wartime activities 
and some have suggested that it provides evidence that Butenandt supported Nazi racial 
hygiene programs. For example, in an article in the German weekly newspaper *Die Zeit*, 
published in January, 2000, the German journalist and author Ernst Klee used this speech 
to link Butenandt to the racial mentality of the Nazi party. Klee noted that Butenandt 
hoped to use hormone therapy to allow infertile women to have children, then printed the 
partial quote, “Of course, the appropriate demand to be made of these measures is that 
only healthy genotypes are supposed to be reproduced…”133

While certainly disturbing, when read in context the quote is less repellent. 
Butenandt continued:

Of course, the appropriate demand to be made of these measures is that only 
healthy genotypes are supposed to be reproduced; the abolition of every form of 
sterility (which one can conceive of as a deliberate form of protection by nature) 
is every bit as improper as allowing a hereditarily ill man to reproduce. It is an 
important task of the doctor to decide in which cases the new techniques are to be 
used; among other things, experience has taught us that in general, only healthy 
tissue responds to these natural healing methods in the desired fashion, so that this 
therapy cannot abolish an illness that is constitutionally based, but only those 
cases where accidental, harmful, environmental influences have led to 
physiological defects.

There are obviously eugenic implications in this passage. However, even in a speech 
where he touched on eugenic ideas and praised the Nazi party for its support of science,

132 Ibid., 4-5.

Butenandt did not associate his work with the racial hygiene of the Nazis.\(^{134}\) It is fair to say that Butenandt put a tantalizing spin on his work, knowing full well that many in his audience would interpret his work as supporting racial hygiene. He was indeed telling his audience what they wanted to hear.

Butenandt’s relationship with the Nazi Party, convenient in many respects, did come with a price. In 1939, he was awarded the Nobel Prize in Chemistry for his hormone research. In 1937, however, Hitler had forbidden German nationals to accept Nobel Prizes. Hitler was outraged that Carl von Ossietzky, a pacifist and opponent of the Nazi government, had been awarded the 1936 Nobel Peace Prize. On direct orders from Hitler, Butenandt and two other German nationals, Richard Kuhn and Gerhard Domagk, were ordered to reject the prize. All three men complied.\(^{135}\)

Recently evidence has come to light that Butenandt may have been involved with two programs of medical research that were conceptualized as part of Nazi racial hygiene and made use of concentration camp inmates as subjects.\(^{136}\) His level of participation is

\(^{134}\) Unfortunately, the more general eugenic ideal of preventing the reproduction of hereditarily ill people via such dramatic methods as forced sterilization was common among many intellectuals and scientists, even in 1941. For connections between German eugenics and the more general international eugenic movement see Karl Heinz Roth, “Schöner neuer Mensch,” Stefan Kühl, The Nazi Connection: Eugenics, American Racism, and German National Socialism (Oxford: Oxford University Press, 1994). For a broader introduction to the international eugenics movement, see Daniel J. Kevles, In the Name of Eugenics: Genetics and the Uses of Human Heredity (Berkeley: University of California Press, 1985).

\(^{135}\) Elizabeth Crawford, “German scientists and Hitler’s vendetta against the Nobel prizes,” Historical Studies in the Physical and Biological Sciences 21 (1999): 42-7. According to Butenandt’s postwar account (which has not been corroborated by contemporary evidence), he was given a letter of rejection prepared by the government to sign. When he objected to the insulting tone of the letter, he was told that refusal to sign would result in dire circumstances for himself, his institute, and his family. In his article in Die Zeit, Ernst Klee mentions the prize as well, he inaccurately wrote “In 1939 Butenandt received (bekommt) the Nobel Prize.”

unclear, especially since he spent most of the last two years of the war in Tübingen, but he probably had knowledge of some of the worst abuses of the Nazi regime while they were occurring. It is certain that he worked with some of those who participated in experiments on concentration camp victims (such as Otmar Freiherr von Verschuer and Gunther Hillmann), and that he did his best to suppress knowledge of these relationships after the war. The lack of archival material (some of which can be attributed to Butenandt’s efforts to clean up the archives in the postwar years) means that his exact level of knowledge and complicity will probably never be known.  

So to what extent was biological chemistry put to the service of the National Socialist government? The best explanation for the militaristic tone of the above speech is the obvious one: at the time of the talk, January, 1941, Butenandt and his colleagues were asking a government at war for RM 500,000 for research on a relatively harmless cylindrical virus that destroyed tobacco, certainly not Hitler’s favorite agricultural crop. Prudence dictated that Butenandt at least attempt to convince the political leadership that unfettered scientific research would be useful to the state in return.

The example of the Dahlem TMV investigators, as exemplified by Butenandt, supports Beyerchen’s conclusion of three decades ago—scientists during the Nazi time tended to be neither pro-Nazi nor anti-Nazi, but instead were committed to independence in their own affairs. To phrase this behavior in terms of Ian Kershaw’s biography of Hitler, they were not working toward the Führer, but they were not working away from him either.  

Academics in early twentieth century Germany enjoyed an unusually high level of social prestige, and it was common for them to think that they were above

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politics and daily life. They tended not to acknowledge responsibility toward anything other than themselves and their disciplines. As the elite carriers of German high culture, they expected the government to support their research and to give them the freedom to carry it out as they saw fit. If the government kept to its end of the bargain, the scientists kept up their end by maintaining their indifference. Within this general framework, levels of complicity varied according to individual circumstances. Understanding how the pursuit of individual interests, even those seemingly unrelated to the goals of National Socialism, contributed to the success of the Nazi government, is one of the most important lessons to be learned from the example of virus research in the Third Reich.

The face that National Socialism showed the world changed from 1933 to 1945. In the early 1930s, the Nazis appealed to many Germans for many different reasons; by the mid-1930s, the political and economic successes of the government convinced many, both in Germany and abroad, to ignore the dark menace that lay behind the façade of respectability. Historian Mark Walker has described this phenomenon with a very apt metaphor:

The Third Reich can be thought of as a slowly but steadily accelerating train. It is easy to get on or off at the beginning, as the train slowly picks up speed, but, just as one begins to be concerned about the speed or direction, trying to leave the train or alter its course no longer seems such a good idea. Once the train has

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139 This was true of German-Jewish intellectuals as well as German nationalist intellectuals, see the accounts of German scientists such as Fritz Haber and Paul Ehrlich in Fritz Stern, Einstein's German World (Princeton: Princeton University Press, 1999).

140 Daniel Greenberg argues that the scientific community is unified by the idea that society should support, but not govern, it, permitting science to govern itself in a loose, meritocratic fashion. He was writing in the context of the US scientific community after the Second World War, but he intended his ideas to be generalizable, and they certainly coincide with how Adolf Butenandt viewed the ideal relationship between science and society. Daniel S. Greenberg, The Politics of Pure Science, 1999 edition with introductory essays by John Maddox and Steven Shapin and a new afterward by the author (Chicago: University of Chicago Press, 1999), 5.
reached a certain speed, the danger is clear to all, but any attempt to get off now appears suicidal. All one can do is to hang on and wait for the end. \footnote{Walker, German National Socialism, 203-204.}

My reservation about this metaphor is that it tends to remove agency from the passengers and grant it to the crew of the locomotive who are somehow leading all to disaster. I believe that the residents of Nazi Germany, party members or not, had a great deal more control over the course of their own history than this metaphor seems to imply.

Instead of passengers, the TMV scientists in the Third Reich collectively should be seen as catalysts or enablers of National Socialism. None had joined party groups before 1936, and one of the most surprising aspects of their research is the extent to which it did not reflect the racial biology of the Nazi leadership. They were willing to work with Nazism, however, so long as they received financial support and scientific freedom. Even if they did not agree with its ideology, the TMV group, and scientists in general, needed the National Socialist government in order to continue their professional lives. The opposite, however, was not true. In his review of John Heilbron’s biography of Max Planck, Mark Walker noted that scientists such as Planck were “very useful if not necessary... to the National Socialist leadership.” \footnote{Mark Walker, “Science, National Socialism, and the longue durée,” 397.}

Scientists were not essential to the success of Nazism. They did not support the party in its early years, and toward the war’s end, they did not produce the technologically advanced wonder weapons of Hitler’s fantasies. However, as David Cassidy noted, throughout the Third Reich scientists “lent the regime an unwarranted and false credibility.” \footnote{David Cassidy, Uncertainty: The Life and Science of Werner Heisenberg, (New York: W. H. Freeman and Company, 1992), 507. Like Walker, Cassidy wrote of the German physics community, but his point is valid for all complicit groups of German scholars.}

Whatever their true intentions or beliefs, scientists made National Socialism more successful than it would have been in their absence.
In Nazi Germany, most people were located between the extremes of the true believer and the dedicated opposition. The enabling effect of uncommitted, self-interested people such as these helps explain how a strong minority of convinced National Socialist believers could turn the entire machinery of a modern state to their will to the extent that they did. Through their commitment to normalcy, the virus researchers catalyzed political disaster. They showed that useful, accurate knowledge about the natural world could be generated in an environment of racist hate. They showed that the best minds in Germany could be counted upon for the support of the project of autarky. By example, they showed that rational, intelligent human beings, whether or not they were members of the party, could work with it under conditions of relative normalcy.  

Conclusion

The example of virus research in National Socialist Germany shows that innovative, original scientific research can take place in a dictatorship. Like other scientists during and after the war, the German virus researchers secured support through negotiation in which they promised useful knowledge in exchange for professional autonomy and money. They were concerned with their own self-interests more than they were those of the country as a whole. Their research was inspired by Wendell Stanley, not by Nazi ideology. The most distinct characteristic of the Arbeitsstätte für Virusforschung was its interdisciplinarity, a characteristic modeled after the most

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144 Here my findings are in complete agreement with Konrad Jarausch’s more general conclusions about the academic professions, including lawyers, teachers, and engineers. He writes: “More disturbing than positive or negative Third Reich stereotypes is the realization that precisely the accommodation of most practitioners to abnormal NS normality made the frightful dialectic between perpetrators and victims possible. For responsible experts, the ethical dilemma was irresolvable. Maintaining professional standards did create an apolitical refuge and serve persecuted clients, curious pupils, and trusting customers. But such competent performance also helped to support dictatorship at home and aggression abroad.” Konrad H. Jarausch, The Unfree Professions: German Lawyers, Teachers, and Engineers, 1900-1950 (New York: Oxford University Press, 1990), 169.
advanced laboratories in the United States and one that stood in stark contrast to the ways that biological research was carried out in most German universities.

To this point, much of the literature on science in Nazi Germany has concerned itself with the impact, usually negative, of Nazism on pre-existing research programs, or with the extent to which scientific research supported the crimes of the regime.145 The research of the Arbeitsstätte für Virusforschung provides the opportunity to go beyond these questions since it was a new program, conceived of only after the devastating wave of dismissals, and it was not a product of racial ideology. Certainly the unique experiences of Nazi Germany had an impact—the expulsion of talented scientists eroded the talent base, the isolation from the international community hampered vital communication with other labs, and the devastation of the war made certain lines of research all but impossible after 1943. These issues will be explored at length in later chapters, but the extent that normal scientific research lent support to an increasingly radical dictatorship suggests that science is not apolitical or independent of its social context. As a specific subculture it is somewhat isolated, but this distance means that its impact, positive or negative, is more indirect. Instead of being the driving force behind Nazism, the people who researched TMV during the Nazi time catalyzed the political radicalism around them. Through their pursuit of normalcy, they became co-responsible for the catastrophe of National Socialism.

145 Beyerchen, “What We Now Know About Nazism and Science,” 629
Chapter 3:

Science in a Shattered State, 1945-1950

Introduction

The people of the TMV community survived the war relatively unscathed, but the country around them was destroyed. Throughout occupied Germany, the physical infrastructure for modern life lay in ruins. Most people were preoccupied with clearing the rubble and procuring the necessities for daily life. It was an environment in which survival was a challenge, making the systematic pursuit of scientific knowledge, and the world of the intellect in general, subordinate to more practical activities. Within this bleak desert, the city of Tübingen was a veritable oasis. It had not been heavily bombed, it did have politicians enthusiastic to pursue scholarship even in the midst of hardship, and thanks to the evacuation of Berlin, it possessed an intact, functioning scientific group. The TMV researchers immediately flourished in their new home.

By using the argument that their scientific research had been “pure” and not pursued as a means toward Nazi political or economic ends, researchers, even those with very definite links to National Socialist organizations, effectively were able to bypass the process of de-Nazification and continue their work. This was true on both an individual and an institutional level, as the same arguments about the purity of research allowed for the continuation of the Kaiser-Wilhelm-Gesellschaft after the war as the Max-Planck-Gesellschaft. Begun in the British zone thanks largely to the efforts of Ernst Telschow
and Otto Hahn, the re-named society soon spread to the American zone but encountered strong opposition from the Tübingen researchers, who resisted membership until 1949. Funding for the renamed organization was provided by the state rather than by private means, as had been the case with the Kaiser-Wilhelm-Gesellschaft. The universities, meanwhile, were rebuilt in a form that scarcely differed from that of their late nineteenth century predecessors. The conservative style and disciplinary rigidity found in university biology departments made it very difficult to do innovative, interdisciplinary research there, and consequently the most innovative virus research took place in the Max-Planck-Gesellschaft.

3.1 Assessing the Situation

3.1.1 Research Conditions in the Western Zones of Occupation

In the years immediately following the Second World War, Germany’s physical infrastructure was severely damaged. Years of Allied bombing had shattered the German cities; hundreds of thousands of people were killed, more were wounded. All of the largest cities had at least twenty cubic meters of rubble per inhabitant that had to be cleared before reconstruction could actually begin, and some cities, such as Köln and Dortmund, had over thirty cubic meters of rubble per resident. By the end of the war, up to twenty per cent of Germany’s housing had been totally destroyed and another twenty per cent was heavily damaged and required extensive repair before it could again be inhabitable. Furthermore, there was a constant flood of displaced persons from the East who had lost their homes as the Soviets re-drew the map of Eastern Europe. The number of refugees reached twelve million by 1950, and, coupled with the destruction of civilian residences, led to a severe housing shortage that was not solved until the early sixties.

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The German currency was all but useless, and the only way essential goods could be acquired was through barter or purchase on the black market.

The postwar conditions were especially daunting for the pursuit of scientific research, as unrelated as it seemed to be to the hardships of daily life. The challenges faced by everyone, as well as problems specific to research itself, combined to minimize the output of researchers during the occupation. Hunger and illness limited their activities. Very little work could be done in the winter months since there was not enough coal to heat the buildings. Laboritories, classrooms, and libraries had been damaged or destroyed. The four occupying powers controlled and exploited German research to varying extents. The lack of a viable currency presented a number of challenges to the research community, and their international isolation prevented them from sharing in the advances of their colleagues abroad. While these challenges were severe, they paled in comparison to the conditions of the millions of Europeans whose lives had been destroyed by the war. The TMV researchers continued to be a privileged elite, even amidst the rubble, and the hardships they faced were actually relatively mild given the conditions of postwar Germany.

Institutions

The emerging Cold War and the division between East and West Germany resulted in a considerable reduction of contact with university and non-university research centers in the Soviet Zone. These included the former Kaiser-Wilhelm-Institute for Brain Research in Berlin-Buch (the institute had been evacuated but the building and

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2 Robert J. Havighurst, "Report on Germany." Prepared on November 20, 1947. RAC, Record Group 1.1, Series 717, Box 3, Folder 17, p. 51. These conditions had a dramatic impact on the health of the scholarly community; entrance examinations given to students at Bonn University in 1946 showed that nearly a third of them (30 per cent) had some form of tuberculosis, of whom eight per cent had progressive cases. "Report on Educational Conditions in Postwar Germany. Based on the notes made by A. R. Mann during trip to Germany January and February 1947." RAC, Record Group 1.1, Series 717, Box 5, Folder 24, pp. 16-17.
labs remained), the Kaiser-Wilhelm-Institute for Physical Chemistry and Electrochemistry, and most of the academic buildings of Berlin’s Humboldt University.\(^3\) Owing to the dearth of space in downtown Berlin, many of the University’s research institutes had been built in southwest Berlin, near Dahlem, which became part of the American sector. By 1948, Soviet efforts to control the school convinced the Americans to form a new university using these conveniently located research institutes as a nucleus; this new institution became the Free University of Berlin.\(^4\) Newly created Kaiser-Wilhelm-Institutes in the conquered territories of the East had, of course, been abandoned in the wake of the retreating Wehrmacht.

In the universities in the western Zones, the damage was severe. Only four—Erlangen, Heidelberg, Göttingen, and Tübingen—were relatively untouched. The others were between fifty and eighty-two per cent destroyed.\(^5\) A later report by the Deutsche Forschungsgemeinschaft (German Research Society—DFG) estimated that among biological research institutes, including those at the universities, roughly one third were totally destroyed and another third severely damaged.\(^6\)

Further hampering research was the fact that the universities had too many students and not enough faculty members. For many reasons, student enrollment was much higher than it had been in the prewar years. One was simply pent-up demand; owing to the war, many young men had not had the opportunity to attend university for

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\(^3\) Some of these institutes had worked closely with the TMV group. Schramm’s radioactive tracing work had been done in collaboration with colleagues at the institute in Berlin-Buch, and Freksa’s electron microscope work had been done at Manfred Ardenne’s laboratory in Lichterfelde. Ardenne himself was basically kidnapped by the Soviets and forced to work for ten years in the Soviet Union. See Norman M. Naimark, *The Russians in Germany: A History of the Soviet Zone of Occupation, 1945-1949* (Cambridge: Belknap/Harvard, 1995), 210-211.

\(^4\) RAC Record Group 1.2, Series 717, Box 1, Folder 6.

\(^5\) Havighurst, “Report on Germany,” 43.

the previous six years, creating an unusually large group of prospective students. Employment prospects were also very grim, leaving few alternatives other than university education for young people. Finally, more and more young women began to attend German universities, encouraged by the Allies in order to diversify and democratize the student population. While women were still greatly outnumbered by men, their numbers were significantly higher than in the prewar years, adding to the total postwar influx. Altogether, the number of students enrolled in 1949-1950 was approximately three times the number of 1937-1938.\(^7\)

The under-staffed faculties of the universities could not meet the demands of so many students. As one consequence of the Allied policy of de-Nazification, faculty members linked to National Socialist organizations were not permitted to teach after the war. The number of overall teaching faculty, including full professors, assistant professors, and unsalaried Privatdozents, was reduced to roughly two-thirds of its prewar level. In certain universities, such as Erlangen and Munich, nearly a third of all senior level teaching chairs were vacant after the war, with the medical faculties being hardest hit.\(^8\) The combination of more students and fewer teachers meant that the student/faculty ratio doubled or even tripled, depending on the university. In the natural sciences faculties, the ratio tripled, quadrupled, and in a few rare cases, quintupled.\(^9\)

**The Occupation**

From the perspective of the occupying powers, biological research potentially fell within the scope of two general policies—those on education and on scientific research.

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The Allied powers believed that education could be a powerful tool for inculcating democratic values in Germany. The Potsdam Agreement of August, 1945, stated "German education shall be so controlled as completely to eliminate Nazi and militarist doctrines and to make possible the successful development of democratic ideas."  

Many, however, saw scientific research as being independent of educational policy. Instead, they viewed research in the light of its military and economic potential. The Americans in particular tended not make the connection between research and educational influence. Therefore their policy on scientific research focused first on halting, then controlling, and finally exploiting German scientific research for the purposes of the occupying powers. 

There was no general agreement among the allies as to how this policy should be implemented. On the general topics of science policy and dismantling of war related research institutes and industry, the four occupation powers proceeded according to their own pace and according to their own agendas. For the reasons discussed in Chapter 1, Section 1.2.3, the Soviet zone does not enter directly into our discussion of the TMV group; nevertheless, a few brief comments on Soviet behavior will help illustrate the diversity of Allied policies. The Soviets quickly established control of the universities in their zone, but they also intended to extract economic and technical reparations to assist in the reconstruction of their own devastated country. In August, 1945, the American

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12 David Cassidy, "Controlling German Science I: US and Allied Forces in Germany, 1945-1947," Historical Studies in the Physical and Biological Sciences 24 (1994): 202, 234. There is a very substantial literature on the occupation as it related to scientific research; Cassidy provides a nice introduction to this material beginning on 200.
detonation of atomic bombs over Hiroshima and Nagasaki provided all the justification the Soviets needed to exploit German science, particularly physics and chemistry, ruthlessly. Institutes were confiscated and scientists kidnapped and shipped (along with their families) to research centers in the Soviet Union. Norman Naimark concludes his study of Soviet science policy in occupied Germany by stating “In short, the Soviet desire to acquire German science, technology, and material, especially uranium, brought the Stalinist terror very close to home for the Germans.”  

America’s own war effort had demonstrated the value of scientific research for military purposes, and researchers there had the greatest respect for the German scientific community, not recognizing how far behind America the Germans had actually fallen. Therefore the US occupation began by closing down what little research existed in its zone, then cautiously allowed the resumption of work that was determined to be of no immediate military or economic significance. The Americans also had their own program of scientific exploitation, the Field Information Agency, Technical (FIAT). Before its termination by General Clay in 1947, FIAT collected millions of pages of technical intelligence for both military and commercial use. Historian John Gimbel assessed the value of these “intellectual reparations” at roughly $5 billion.  

Although the British did partake of intellectual reparations on a scale nearly equal to that of the United States, they were milder both in regards to de-Nazification and the

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treatment of scientific research. They differentiated between pure and applied research, and from the very beginning British officials argued that pure research should be allowed to continue, while efforts at control and exploitation should focus on applied research. Their views were expressed in Law 25 of the Allied Control Council, passed on April 29, 1946. This law stated that pure research would be allowed to continue as long as it was not military in nature and as long as its physical apparatus could not be used for military purposes. A number of fields, such as nuclear physics and aerodynamics, were completely banned for the time being. 15 In their treatment of scholarship, the British were more consistent than were the Americans—they saw that both research and teaching could be constructive elements in the long term process of re-educating Germany.

At first glance the policy of the French in their zone of occupation (where the city of Tübingen was located) might seem to have been singularly unfavorable for the re-establishment of innovative scientific research. The French approached their zone with the intention that it should be thoroughly de-militarized, de-Nazified, economically exploited, re-educated and democratized. 16 They were understandably zealous in their efforts to stamp out militaristic values. According to one postwar report sponsored by the Rockefeller Foundation:

The French in their zone were endeavoring to win the intellectual groups by every possible means, but according to some reports they were thoroughly disliked. They compelled the study of French from the first grades and were said to require


16 According to F. Roy Willis, the “French Thesis” on Germany entailed the following: A) The Rhineland should be de-militarized; B) The Ruhr should be separated, placed under international control; C) The Saar should be united economically with France; D) Germany should be economically exploited for the benefit of its neighbors; E) De-militarization and de-Nazification, F) Re-education and re-organization as a democratic state. F. Roy Willis, The French in Germany 1945-1949 (Stanford: Stanford University Press, 1962), 45-46.
proficiency in French for admission to the universities and to professions. It was also stated that the French actively endeavored to keep the students in their zone from having any contact with students in the other zones.\textsuperscript{17}

It is evident that a certain heavy-handedness on the part of the French existed, but this report also demonstrates their commitment to education and cultural exchange as fundamental goals of the occupation. The Americans tended to view their own task in Germany as that of a short-term military occupation. The French thought in terms of a much longer time frame and hoped to achieve lasting peace.\textsuperscript{18} This attitude opened up the possibility of a much more enlightened approach toward scientific research; in the French zone, as in the British, science was understood in both cultural and military/economic terms. It could therefore be used in the cultural process of re-education.

\textit{Currency}

At the time, the German currency, the Reichsmark, was useless for alleviating the material shortages that hampered scientific research. In addition to basic necessities such as books, journals, and paper, university laboratories also needed specialized equipment and supplies, many of which were very expensive. The biological sciences, which often required live animals for experimentation, were particularly difficult to provision. The currency helped very little because it was worthless. What mattered in postwar Germany were physical goods, which were available only through barter or the black market. Regarding the German students and supplies, one American observer noted “The primary need was not money. There seemed to be plenty, but the things the students needed were

\textsuperscript{17} Mann, “Report on the Educational Conditions in Postwar Germany,” 16.

\textsuperscript{18} According to historian Richard Gilmore, “Cultural questions, particularly matters concerning education whose regulation depended on extensive allied controls, would not have been stressed had Paris not thought that this program would bring a high payoff over time.” Richard Gilmore, \textit{France’s Postwar Cultural Policies and Activities in Germany: 1945-1956} (Washington DC: Balmar Reprographics, Inc., 1973), 32.
simply not to be bought.”\textsuperscript{19} This lack of material goods plagued established researchers in the Kaiser-Wilhelm-Gesellschaft as well. When they attempted to build new institutes for their work in the postwar years, their greatest problem was procuring building materials such as bricks and nails, not money itself.\textsuperscript{20} For scientists the Reichsmark was doubly useless, since it could not be used to purchase goods abroad; the only way that German researchers could purchase supplies, books, or journals from foreign countries was with foreign currency.\textsuperscript{21} They also could not pay for memberships in foreign scientific societies, so the worthlessness of their currency contributed to their isolation from the rest of the international scientific community.

\textit{Isolation}

According to one of the Rockefeller Foundation’s postwar surveys of German science, “The one thing which all German scientists agreed upon as their greatest need is communication with the outside world.”\textsuperscript{22} Communication, while not as tangible as the material resources discussed above, is one of the most indispensable elements of modern science.\textsuperscript{23} Beginning with the purge of the scholarly community in 1933, German contact with the rest of the world had steadily declined. By the postwar years, many German scientists had been without foreign literature for years and had no idea of what was happening in their fields. They did not know what subjects to pursue, what questions to ask, or how their own work would be received by their colleagues. Science, understood as a social process of knowledge production, could not operate in such an environment.

\textsuperscript{19} Mann, “Report on the Educational Conditions in Postwar Germany,” 16.

\textsuperscript{20} Interview with Alfred Gierer, Tübingen, May 23, 2000.

\textsuperscript{21} Havighurst, “Report on Germany,” 60.

\textsuperscript{22} Ibid.

\textsuperscript{23} See Chapter 4, Sections 4.1 and 4.2 for more on this topic.
The improvement of communication via foreign literature and personal contact with foreign scholars appeared to be the most practical way to improve German scholarship relatively quickly. Since the Americans were wary of encouraging research in the physical sciences, their efforts focused on the social sciences and humanities, but the difficulties facing the re-establishment of scholarly communication in these fields held true for the physical and biological sciences as well. To facilitate the democratization of German education, the US War Department planned to bring books, journals, and other cultural materials to Germany beginning in 1946. Despite these efforts, access to foreign scholarly literature on an individual level, especially in the physical and biological sciences, depended upon personal connections. Scientists throughout Germany benefited from the generosity of their colleagues who sent reprints of articles and copies of journals at their own expense.

The exchange of personnel was just as important, but was fraught with greater complications. Initially, the Americans hoped to send over selected American faculty members to teach German students and professors and in doing so bring the German faculties up to date on the most recent developments. Of course, carrying out such a program required both volunteers (preferably those capable of lecturing in German) and funding, neither of which was in plentiful supply. Accordingly, the US government was eager to have other groups step in and share the burden. The accommodations in many parts of Germany, including Tübingen, were also less than ideal, further discouraging extended stays by established scholars from abroad.

24 Albert Mann, Memo dated November 21, 1946, subject: Conferences at State and War Departments concerning activities in Germany and Austria. RAC Record Group 1.1, Series 717, Box 4, Folder 21.

25 Ibid.

26 Commenting on the unpleasant food and primitive conditions that greeted him during a visit to Tübingen in 1949, Gerald Pomerat of the Rockefeller Foundation noted, "How the French or Germans ever expect to attract distinguished lecturers or professors to this place for more than a day is quite beyond GRP's [Gerald R. Pomerat's] comprehension... No one expecting even a very moderate amount of personal comfort should
The other possibility was to send German scholars abroad as teaching and research fellows, an even more difficult proposition. First of all, because of the weak currency, funding for the exchange had to come entirely from the host country.\textsuperscript{27} A second difficulty was establishing clearance for international travel for fellowship candidates. The final difficulty was in identifying potential candidates. For reasons of political reliability, German scholars and their foreign sponsors deliberately sought candidates who were young enough to be innocent of high level complicity in the Nazi regime. They also sought people with great intellectual potential as well as foreign language skills. The problem was that young men (the researchers involved spoke deliberately and exclusively in terms of men) with such talents were likely to remain in their host countries rather than return to the poverty and misery of Germany. Karl Thomas and Karl-Friedrich Bonhoeffer, both of Göttingen, advised one of the Rockefeller Foundation’s representatives to exercise caution in evaluating prospective fellowship candidates. They suggested that the potential for defection among young Germans was so high that German faculty members might seize upon foreign fellowships as a way of reducing high enrollments or ridding themselves of particularly disruptive students.\textsuperscript{28}

3.1.2 Tübingen

The situation for research in postwar Germany was indeed grim and the prospects for immediate improvement were hardly promising. In Tübingen, the situation was ever be sent to Tübingen without adequate warning.” Pomerat did temper this by stating that conditions in other parts of Germany he had visited were much better. “Diary of Gerald R. Pomerat, 1949.” RAC, Record Group 12.2, Diaries, Box 36, p. 233.

\textsuperscript{27} The Office of Military Government, United States (OMGUS) urged the US government to make funds for academic exchange available, and requested assistance from private groups as well. Mann, “Report on the Educational Conditions in Postwar Germany,” 80-84.

\textsuperscript{28} “Diary of Gerald R. Pomerat, 1949,” 152.
somewhat better, while not ideal, conditions there were as good as possible in postwar Germany. The same shortages of food, clothing, and construction materials plagued researchers there, as of course did the useless currency. In addition, the researchers in Tübingen were also isolated from the international community, although as we shall see in the next chapter, their own personal connections helped to alleviate this situation somewhat. Tübingen, however, possessed several advantages that helped the researchers there cope with these problems effectively. The infrastructure of the city and university had hardly been touched by the air war. The city’s leaders, as well as those of the French occupation, were interested in promoting research and supported the TMV workers from early on. Furthermore, the core of the Dahlem group had been evacuated to Tübingen intact. Both the institutes for biology and biochemistry were eventually housed there, and they had brought with them their technical assistants and other support personnel as well as their own equipment and libraries. They could therefore continue the interdisciplinary work they had begun in Dahlem without interruption.

In contrast to most other German cities, Tübingen was never subject to massive aerial bombardment. The city’s small size, isolation, and lack of heavy industry all conspired to keep it low on the Allies’ list of potential targets. Consequently, air raids killed only 44 people in Tübingen, all within the last three months of the war. Only 1.6 per cent of the city’s buildings (82 total) was destroyed; 2.3 per cent was heavily damaged, and 11.6 per cent lightly damaged. The city’s good fortune was certainly not due to its location—Allied bombers could have reached it easily, and the residents often saw waves of aircraft flying overhead on their way to Stuttgart, north of Tübingen, which was heavily damaged. Tübingen’s relatively pristine condition encouraged many

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businesses and industries to relocate there during the war years, and by 1945 the city’s population had increased to 40,000, up from roughly 30,000 at the beginning of the war.\textsuperscript{30}

It was no accident that the leaders of the biological research institutes from Dahlem chose Tübingen to be their new home, for Hans Friedrich-Freksa knew the area and the university well. He had attended school there and had later worked for the university’s zoological institute. In the summer of 1943, Freksa helped Butenandt negotiate the plan to house the evacuated Kaiser-Wilhelm Institut für Biochemie in several different buildings of Tübingen University.\textsuperscript{31} The researchers of the Arbeitsstätte für Virusforschung took up residence in the various institutes of the university as follows: Freksa in the zoological institute; Melchers in the botanical institute; Schramm in the hygiene institute; and Gernot Bergold, who had previously worked at a satellite lab in Oppau, in the pharmacology institute. The majority of the researchers from the Kaiser-Wilhelm-Institut für Biologie were evacuated to the nearby town of Hechingen.\textsuperscript{32}

Although dispersal in so many different buildings was hardly ideal (the Institut für Biochemie, including the virus center, was located in a total of eight different buildings), at least the virus researchers had the chance to continue working with a minimum of interruption. In fact, one could hardly have asked for a better host than Tübingen University. Not only was it functionally intact, but on October 15, 1945, it became one of the first universities in Germany to re-open after the war.\textsuperscript{33} Tübingen University had

\textsuperscript{30} Ibid., 367. The population increase in Tübingen contrasts with the French zone as a whole, which lost population over the course of the war. Willis, \textit{The French in Germany}, 107.

\textsuperscript{31} Butenandt to Herr Knapp, Oberregierungsrat, Tübingen. August 20, 1943. Universitätsarchiv, Tübingen (henceforth UAT) Sig. 117/13634.

\textsuperscript{32} Memo from Schramm, February 22, 1944; letter from Albert Speer to Gauleiter Murr, August 26, 1943. Both UAT Sig. 117/13634.

\textsuperscript{33} \textit{Schwabisches Tageblatt}, 12 October 1945. For a general history of Tübingen University that covers the late nineteenth and early twentieth centuries and includes an excellent review of recent literature on the history of German universities, see Sylvia Palatshek, \textit{Die permanente Erfindung einer Tradition: Die Universität Tübingen im Kaiserreich und in der Weimarer Republik} (Stuttgart: Franz Steiner Verlag, 2001).
one of the few functioning libraries in Germany, and although the total number of students had increased, the burden was not nearly as overwhelming as it was at other German universities. Of course, shortages of all kinds existed, but the students were well dressed and the buildings still stood. In fact, such was the contrast between Tübingen and the other German universities that one postwar observer wrote "I got the feeling that Tübingen is a bit smug and willing to ignore the difficulties of the other universities in Germany."  

In practice, the policy of the French occupation was less harsh than it seemed in principle, although this was not readily apparent in the first days after the war. When French troops entered Tübingen in May, 1945, they immediately began to loot laboratories and research institutes. The greatest loss was the electron microscope belonging to the university’s hygiene institute. It was probably the only working electron microscope in Germany at the time, was very expensive, and therefore was a great prize. On May 11, 1945, six French offices seized three crates full of parts for the microscope and sent them to a laboratory in Paris; the director of the Hygiene Institute, Professor Otto Stickl, was unable to stop them. However, over the next several days the officers met with Butenandt as well as some American officers. Impressed by Butenandt’s

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34 Robert J. Havighurst, "Germany—Diary and Interviews," October 5-6, 1947. RAC, Record Group 1.1, Series 717, Box 3, Folder 19. Havighurst noted that the number of students at Tübingen had increased from about 2,500-3,000 in the prewar years to 3,000-3,200 after the war.

35 From a historical/cultural perspective, southwest Germany was the easiest part of the country to reconcile with France. The Länder of Baden and Württemberg had a tradition of constitutionism. In addition, the French zone did not include either Berlin (center of Prussian militarism) or Munich (capital of the Nazi movement). The French recognized that they had a rare opportunity to improve the Franco-German relationship. According to Gilmore, "France’s program in 1945 was based on the premise than an intensification of cultural relations at all levels and in every imaginable form would be the answer to what had been missing in earlier Franco-German relations. World War II was the watershed and, consequently, the main impetus for the highly innovative and comprehensive approach France followed in her occupation zone." France’s Postwar Cultural Policies, 280-281.

36 The main body of the microscope was mounted on three cubic meters of concrete to provide stability and was therefore not easily confiscated.
scientific credentials, especially the support given him by the Rockefeller Foundation, the French ceased their appropriation of German lab equipment. 37 Unfortunately for Stickl (and the university), they kept the parts they had already taken, rendering the remainder of the electron microscope useless. From that point on, the French were cooperative, and Butenandt recalled that overall they were very helpful, and even gave his institute priority for building materials. Although the French had begun their occupation intending to halt all research, they did not enforce this policy, and according to Butenandt, he did not lose a single day's work because of the occupation. 38

More importantly, the French worked productively with the city government to promote scientific research in their zone of occupation. 39 An extraordinary politician, Carlo Schmid, helped coordinate this smooth relationship between the Germans and their conquerors. Schmid had been born in France of a French mother and German father. He became a professor of history and law at Tübingen University, and during the war served as part of the civilian administration of occupied France, where he was noted for his sympathy toward the French people. In 1944, he returned to Tübingen. He spoke French as well as any of the occupiers and was quickly appointed as president of the State Secretariat of Württemberg. He was an outstanding choice for the position and was able to cooperate with the French while simultaneously defending the interests of the Germans. 40

37 Diary of Otto Stickl, Director of Hygiene Institute, Tübingen University, May 11-17, 1945. UAT, Sig. 663/5.

38 Havighurst, “Germany—Diary and Interviews,” Interview with Adolf Butenandt, October 6, 1947, pp. 175-176.


Upon his return to Tübingen, Schmid had met Alfred Kühn and Max Hartmann, both directors of the evacuated Kaiser-Wilhelm-Institut für Biologie. Schmid was immediately impressed with both men and struck by the twist of fate that had brought such a collection of scientific talent to the small university city of Tübingen. He immediately took steps to retain these researchers and establish Tübingen as a center of culture and scholarship.\footnote{Schmid, Erinnerungen, 211-226.} For example, he helped Butenandt secure a professorship in physiological chemistry at the university in December, 1945. This arrangement was rather unusual—the directors of the Kaiser-Wilhelm-Gesellschaft had previously been independent of the universities. Nevertheless it had advantages, not the least of which was that the university would pay for part of Butenandt’s salary, easing the burden on the budget of his own institute.\footnote{Alfred Kühn had already accepted such a position and was therefore the first KWI director in Tübingen to lead such a “double life.” It gave both men the opportunity to teach at the university as well, something that Butenandt valued, and when he later moved his institute to Munich, he insisted on such a dual appointment there. For Butenandt, see “Carlo Schmid,” 44, and Havighurst, “Germany—Diary and Interviews,” 175-176; for Kühn, see Havighurst, “Germany—Diary and Interviews,” Interview with Alfred Kühn, October 6, 1947, p. 177.} Schmid also worked with the city government to provide the kind of facilities that would keep the evacuated institutes in Tübingen for the foreseeable future. At the time, the institute for biology was still located in the nearby town of Hechingen. The French government expressed a wish that it be moved to more permanent quarters in Tübingen, and the city government immediately complied, drawing up tentative plans for the construction of three new institute buildings, a new library, and housing for the institute’s workers.\footnote{Suggested plan from Herr Karrer, Mayor of Tübingen, May 13, 1946. Stadtarchiv Tübingen (henceforth SAT), Sig. A200/1815.} In August, 1946, under the encouragement of Schmid, officials in the city government expressed their desire to begin construction immediately in order to retain the biology institute. (By that time, the French wanted to
move it to the city of Mainz to become part of the new university they were establishing there.) An estimated RM 220,000 was allocated for the construction of new institutes and the improvement of existing ones.\footnote{Unsigned letter to Landesdirektion für Kultus, Erziehung, und Kunst, Tübingen, August 20, 1946. SAT Sig. A200/1815.}

While Tübingen had not been heavily damaged, the influx of persons and industry throughout the war had created an acute housing shortage. The overall scarcity of building materials in Germany severely constrained the rate at which this crisis could be relieved. With this in mind, the decision by the local leadership to commit themselves to the construction of five new buildings for scientific research (in addition to the four buildings for the Institut für Biologie they promised to build a completely new building to house the division for virus research) as well as housing for their personnel represented an extraordinary commitment to research.

Even more extraordinary than the initial commitment itself was the extent to which the promises became reality. The total cost reached millions of marks. Before the currency reform, the cost was not as prohibitive as was the availability of building materials, which the occupation government was very helpful in providing for the institutes. The contraction of the monetary supply resulting from the currency reform brought prices and materials into line with one another, so that after June, 1948, money became the determining factor. The city and state governments persevered; in October, 1948 Melchers wrote that given the circumstances, the efforts of the state government to support the KWI für Biologie were extraordinary.\footnote{"Die Leistungen, die das kleine Land für unserem laufendem Etat und die beiden Neubauten auffbringt, sind gemessen an den Möglichkeiten ganz enorm." Melchers to Otto Hahn, October 12, 1948. Melchers Papers, Ordner 1. In the documentation, it is often difficult to tell when the researchers are referring to the city (\textit{Stadt}) government or the state (\textit{Land}); Tübingen is sometimes described as a \textit{Land} government, as is the entire area of Württemberg-Hohenzollern. Complicating matters further is the fact that the boundaries of the current \textit{Land} of Baden-Württemberg, where Tübingen is located, were not fixed until after the creation of the Federal Republic of Germany in 1949.} Nevertheless, a month later Melchers
had to ask the mayor of the nearby city of Reutlingen for an infusion of cash in order to keep construction proceeding as planned. In spite of these difficulties and many others, three new buildings, two for the KWI für Biologie and one for the virus center, were finished on schedule by 1950.46

Such a heroic effort would not have been possible had Tübingen not been in the unique position of housing an outstanding group of displaced scholars. Whereas scientists from many other institutes had been dispersed across western Germany, the Dahlem biological community made the transition to Tübingen intact. The most significant change was the death of Wettstein in February, 1945. Shortly afterward, Melchers left the virus group to succeed Wettstein as a division leader within the Kaiser-Wilhelm-Institut für Biologie. The virus group became an autonomous division within the Institut für Biochemie, rather than a bridge organization between the two institutes. Freksa and Schramm continued to lead their respective groups, while Bergold left for Canada in 1948. Werner Schäfer, whose training was in animal virology, replaced him.47

The senior researchers brought with them co-workers, assistants, and equipment, having lost very little because of the war.48

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46 I have not found any documentation of the total costs of the institutes, but from the fragmentary evidence I have gathered, it was considerable. For the construction of Melchers’ institute alone the local government budgeted DM 150,000 for 1948-1949 and DM 200,000 for 1949-1950. Building the infrastructure necessary to service the new institutes (including new streets, water, sewer, and electrical lines) cost an estimated DM 193,800, of which the city government paid more than half. The specialized equipment for the greenhouses and labs also cost in the hundreds of thousands of marks; added to the cost of construction and one cannot escape the conclusion that the construction of the new institutes was an enormous expense. Budget for Melchers’ institute, memo from Melchers, August 5, 1948; Melchers Papers, Ordner 1. Infrastructure costs from draft of contract between the city administration of Tübingen and the Finance Ministry, August 12, 1949, SAT Sig. A 200/1815; for equipment costs see Melchers Papers, Kasten 1; also Melchers to Herr Kalbfell, Mayor of Reutlingen, asking for DM 75,000, November 17, 1948. Melchers Papers, Ordner 1. Some relief was provided by ERP (Marshall Plan) funds, which became available in 1950; at that time Melchers was promised DM 8,000 to help equip his lab. Kurt Zierold to Melchers, October 14, 1950. Melchers Papers, Order 72—Deutsche Forschungsgemeinschaft.

47 Rolf Danneel’s group was separated and re-located in Göttingen. MPG Archiv I. Abt., Rep. 46, Nr. 8; for background on Schäfer and the reasons for his selection, see Klaus Munk, *Virologie in Deutschland: Entwicklung eines Fachgebiets* (Basel: Karger, 1995), 39-46.

48 Havighurst, “Germany—Diary and Interviews,” 175-177.
Keeping this extraordinary group of scholars intact and in Tübingen appealed to French and German officials for different reasons. The French welcomed the decentralization of German scientific research and had no desire to see Berlin or any other city become a national center for scientific research. Supporting research in their own zone prevented the large scale organization of science in Germany and also gave them considerable influence over one of the more powerful remaining pockets of German scientific research. As late as 1948, they hoped to create their own zonal confederation of scientific institutes. The relatively innocuous nature of TMV research (the Germans were once again stressing its pure, rather than applied, aspects) meant that such support could not be seen as compromising the military or economic security of France. German officials, such as Schmid, recognized that they had a unique opportunity to transform Tübingen from a sleepy, isolated university town to one of the most dynamic centers of biological research in Germany. They, too, had no desire to see the Dahlem group return to Berlin, or to take up residence in any of the other prestigious cities of German learning such as Heidelberg or Göttingen.

The loss of any of these researchers was therefore a cause for concern. In 1946, Melchers received an offer from Cologne University. He ultimately rejected the offer, but the threat of his departure was used to justify proceeding as quickly as possible with the construction of a new building for his division of the Institut für Biologie. In 1948, Butenandt received an offer to take a position in physiological chemistry at the University of Basel. His case was more complicated than that of Melchers for two


50 Unsigned letter to Landesdirektion für Kultus, Erziehung, und Kunst, Tübingen, August 20, 1946. SAT Sig. A200/1815.
reasons. The first was his relationship with the Nazi regime, because there was significant resistance in Basel against recruiting a former party member for such a prestigious position. The university administration was sufficiently concerned that they solicited references on Butenandt’s character from several of his colleagues, who acknowledged his opportunism but in general defended him against accusations of enthusiastic support for National Socialism.\textsuperscript{51} The second complication was the international nature of the move. The loss of Butenandt was perceived as a loss to Germany as a whole, not just Tübingen. A coalition of German industrialists persuaded Butenandt to stay by providing him with roughly DM 100,000, with which he was able to purchase a new electron microscope to replace the one lost in the early days of the occupation.\textsuperscript{52}

Both of these cases were resolved in favor of Tübingen’s newly founded biological research community. The decisions of both Butenandt and Melchers to stay in Tübingen, combined with the other advantages the city had to offer, guaranteed that it would become an advantageous home for German virus research in the postwar era. The success of Tübingen therefore did not result just from the coincidence of a number of positive trends, but from the mutual interaction of factors that fed into and reinforced one another, creating the most positive possible environment for virus research in postwar Germany. The Dahlem community chose Tübingen because the city was undamaged. Because the city was undamaged, after the war it could afford to allocate resources to the construction of new research institutes. City leaders could justify the expense by

\textsuperscript{51} Two of these letters are in the Melchers papers of the Archive of the Max-Planck-Gesellschaft. See letters from the Education Department of Basel to Melchers, dated May 21, 1948, and to Hans Weber, dated June 12, 1948, and the two men’s replies, dated June 3 and July 8, respectively. Melchers Papers, Ordner 1.

\textsuperscript{52} Peter Karlson, \textit{Adolf Butenandt: Biochemiker, Hormonforscher, Wissenschaftspolitiker} (Stuttgart: Wissenschaftliche Verlagsgesellschaft mbH, 1990), 181-183.
referring to the prestige of the displaced scientists. The French contributed because they wanted to support science in their own zone at the expense of Germany as a whole. The researchers themselves, unique in talent and group coherence, found themselves very much in demand, and were therefore able, to an extent, to dictate the terms of scientific reconstruction within their zone.

3.2 Suppressing the Past

3.2.1 De-Nazification

One of the most important policies of the occupation powers was de-Nazification, which quite naturally had an impact on education and research in postwar Germany. The Allies generally agreed that there were two aspects to de-Nazification: punishing individuals for specific crimes committed during the Third Reich, and purging the German people of fascist attitudes via re-education and democratization. The two were of course linked, with the second part, education, falling to those who passed the first part. De-Nazification had its symbolic beginning with the international military tribunal held in the city of Nuremberg in 1945-6, where many of the top surviving Nazi officials were tried for their behavior during Hitler's reign.53 Outside of the high ranks of leadership, though, the identification of individuals deserving of punishment was very challenging. Since the Nazi party had been the only official political party for twelve years, obviously many people had joined the party out of opportunism or necessity rather than ideological conviction. The climate of virulent racist hate condoned by the Nazis also meant that others who had never joined the party were guilty of the most horrific of crimes. The process of sorting out who had been a real "Nazi" was very difficult.54


54 This was especially difficult in the medical profession. While a disproportionately high number of physicians in Nazi Germany belonged to the party and its organizations, many others contributed to its
Like most other occupation policies, de-Nazification was pursued by each occupation power according to its own inclinations—there was no four-power cooperation outside of the Nuremberg trials. De-Nazification in the Soviet zone was strict and thorough, but also subject to a host of abuses.\textsuperscript{55} The Americans tended to err on the side of caution, and their thoroughness in this regard was one of the reasons so many positions were vacant in universities in their zone, such as Munich and Erlangen. Still, the sheer number of cases to be investigated meant that many criminals escaped prosecution while many whose conduct was acceptable suffered as a result of easily documented superficial connections.\textsuperscript{56}

Such hypocrisy, whether accidental or intentional, was probably the most frustrating element of the process for the Germans. In specific cases, Germans who were unable to work in Germany as a result of their Nazi affiliations were invited to the US by the War Department and promised that they would be de-Nazified so long as they worked racial crimes although they were not formally affiliated. Many of these people re-entered the medical profession, unpunished, after the war. See Götz Aly, Peter Chroust, and Christian Pross, Cleansing the Fatherland: Nazi Medicine and Racial Hygiene, trans. Belinda Cooper (Baltimore: Johns Hopkins University Press, 1994), esp. “Introduction” by Pross; Michael Burleigh, Death and Deliverance: ‘Euthanasia’ in Germany 1900-1945 (Cambridge: Cambridge University Press, 1994), esp. Part Four “Aftermaths,” 269-298; and Peter Weingart, Jürgen Kroll, and Kurt Bayertz, Rasse, Blut, und Gene: Geschichte der Eugenik und Rassenhygiene in Deutschland (Frankfurt: Suhrkamp, 1988).

\textsuperscript{55} For example, leaders in the Soviet Union were intent upon creating a reliable secret police force to help administer their zone and were therefore quite willing to accept Germans with the appropriate qualifications (i.e. former members of the Gestapo) into the postwar police. See Naimark, The Russians in Germany, Chapter 7 “Building the East German Police State.” For a specific case study of the failure of de-Nazification in the Soviet zone see Timothy R. Vogt, Denazification in Soviet-Occupied Germany: Brandenburg, 1945-1948 (Cambridge: Harvard University Press, 2000).

\textsuperscript{56} One of the most unsettling documented cases of unintentional de-Nazification was that of Hans Joachim Sewering. He voluntarily joined the SS in 1933 and there is evidence of his participation in at least one case of euthanasia. He was cleared by an overworked court in the American zone and went on to an enormously successful postwar career. In the autumn of 1992, he was elected president of the World Medical Assembly. It took a dedicated effort to publicize his crimes to persuade him to resign his position, a position he by rights should never have been considered for. See Michael H. Kater, “The Sewering Scandal of 1993,” in Medicine and Modernity: Public Health and Medical Care in Nineteenth- and Twentieth-Century Germany ed. Manfred Berg and Geoffrey Cocks (Cambridge: Cambridge University Press, 1997), 213-234.
on defense related research.\textsuperscript{57} Noting this double standard, the German chemist Otto Hahn declared that the only difference between the United States and Russia was that the Russians took two hours to remove a man while the United States took two weeks.\textsuperscript{58} The British tempered the need for thoroughness with pragmatism. They recognized at an early stage that many higher officials and educators had joined the party out of necessity and allowed certain of these individuals to resume their careers relatively early in the postwar era.\textsuperscript{59}

In theory, the French took a very strict line on de-Nazification. Many French citizens saw Nazism as the logical consequence of German history and believed that all Germans were responsible to some extent. In an effort to root out this tendency, the French paid careful attention to the legal and educational professions during their de-Nazification process, and according to one estimate had dismissed roughly 35 per cent of the university teaching personnel in their zone in 1945-46.\textsuperscript{60} Once this phase of de-

\textsuperscript{57} This was especially true of Werner von Braun and other scientists affiliated with the German ballistic missile program. They were not space enthusiasts who tolerated the regime in order to reach the stars, but collaborators of the first order. In fact the scientific leadership of the program initiated the use of slave labor in order to counter a general labor shortage. After the war, security reports on people like von Braun had to be doctored in order to allow the researchers to remain in America. See Michael Neufeld, \textit{The Rocket and the Reich: Peenemünde and the Coming of the Ballistic Missile Era} (Cambridge: Harvard University Press, 1995), 82, 184, 219, and 270; and “Wernher von Braun, the SS, and Concentration Camp Labor: Questions of Moral, Political, and Criminal Responsibility,” \textit{German Studies Review} 25 (2002): 57-78. The United States did in fact go to great lengths to get security clearances to bring large numbers of German scientists and technicians to the US as part of Project Paperclip, which picked up largely where FIAT had left off. Tom Bower, \textit{The Paperclip Conspiracy: The Hunt for Nazi Scientists} (New York: Little, Brown, & Company, 1987).

\textsuperscript{58} Hahn cited in Mann, “Report on Educational Conditions in Postwar Germany,” 27. Given that one of Hahn’s closest associates after the war, Ernst Telschow, had by all accounts been a dedicated National Socialist, his righteous indignation rings hollow.

\textsuperscript{59} For example, upon return from a fact-finding mission to the British zone in early 1947, a delegation from the Association of University teachers advised that rather than purging university faculty wholesale, careful attention should be given to cases of younger faculty members who might have joined the Nazi Party under duress. “The Universities in the British Zone of Germany.” RAC Record Group 1.1, Series 717, Box 2, Folder 15, pp. 21-28.

Nazification was finished, however, the French did everything they could to re-establish education and learning in their zone as the next stage of de-Nazification.

Of the Tübingen researchers whose work was most relevant to TMV, the only ones who had affiliated themselves officially with the party were Butenandt and Schramm. Butenandt's reasons for joining have already been discussed. After the war, his decision cost him very little as far as prestige was concerned, and he was treated with a high level of respect by French and American forces during the early days of the occupation. When the question of his political reliability arose, he was able to convince most of those around him that he had never really been a member of the party. The impression he gave was that of someone who had worked with the Nazis in order to profit from the association; selfish, yes; arrogant, yes again, but not criminal. The recommendations solicited for Butenandt as part of the call to Basel University confirmed this picture.

According to Butenandt's biographer, Peter Karlson (a former student who accompanied Butenandt from Danzig to Dahlem to Tübingen), in early 1936 Butenandt recommended that his students join the SA as a way of keeping all their career options open. When Schramm arrived at the SA recruiting Bureau, the officer in charge took one look at him and determined that someone of his height and appearance belonged in the SS and required him to join. Whether or not one accepts Karlson's story, Schramm's

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61 Diary of Otto Stickl, May 13, 1945. UAT, Sig. 663/5. According to Mark Walker, Butenandt was even able to use his influence to stop the French Naval Commission from expropriating valuable laboratory equipment from the Hechingen Branch of the KW1 für Physik. *German National Socialism and the Quest for Nuclear Power 1945-1949* (Cambridge: Cambridge University Press, 1989), 186.

62 Gerald R. Pomerat, of the Rockefeller Foundation, wrote “B[utenandt], while not officially a member of the Nazi Party, without question profited enormously by cooperating with its officers. RF [Rockefeller Foundation] officers have no direct evidence that B engaged in reprehensible practices although they have heard of incidents in which he displayed typical Prussian arrogance.” Diary of Gerald R. Pomerat, 1949, 322. Also see pp. 245-246, where Professor Adolf Portmann of Basel University confirmed “that Butenandt was not a bad Nazi, but that he profited from them and was useful to them.”

63 Karlson, *Adolf Butenandt*, 172-173. Refer to Chapter 2, Section 2.1.4, for the potential shortcomings of Karlson’s biography and references to more critical work on Butenandt’s life.
membership in such a murderous organization is disturbing, to say the least. The only evidence regarding Schramm's wartime relationship with the SS is indirect and therefore cannot be taken as final, but it is consistent with what we do know with certainty about him. During a 1949 visit to Tübingen, Rockefeller Foundation representative Gerald Pomerat had the opportunity to speak with Paul Fouché, a member of the French occupation government, about Schramm. Pomerat's account of the meeting reads as follows:

After dinner, when we were not with the Germans, Fouché [sic] said that he succeeded in preventing the French from sending Schramm to the denazification courts. S. had joined the S.S. in their early days but voluntarily resigned in 1938 because he could no longer tolerate their activities. F. felt this early resignation meant that S. was OK and the French military government eventually agreed with him. Melchers, who has an excellent political record during the war, agreed with F. on this at the time. Nevertheless, S.'s promotion is not being pushed just now because F. believes it might antagonize the French.\(^6^4\)

Whether or not Schramm was able to document his withdrawal from the SS is unclear, but one assumes that as a former member he must have presented a very convincing case. The French were certainly not tolerant of those whom they linked to German atrocities.

There is no evidence linking Schramm to the criminal activities of the SS, although the lack of evidence is not exculpatory in itself. Schramm's published research clearly shows that he did not subscribe to the Volkish ideology associated with Himmler and the upper ranks of the SS. The safest assumption was that his association with the SS, whatever its true duration, was a prudent, opportunistic move rather than an affirmation of the SS agenda.\(^6^5\) Schramm provides another example of how the most intelligent and talented people in Germany supported Nazism via their own self-interest.

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\(^6^5\) Robert Lewis Koehl writes “The mental furniture of Himmler and his intellectual cohorts was the warmed-over folkish world view that well-educated Germans regarded as slightly ludicrous.” Koehl, \textit{The Black Corps: The Structure and Power Struggles of the Nazi SS} (Madison: University of Wisconsin Press, 134
In a sense there was no real need for the de-Nazification of the Tübingen group and their colleagues since they had not been truly “Nazified.” They had cooperated with the Nazis out of self-interest, and they cooperated with the occupation powers for the same reason. Their lack of ideological commitment to either government justified (to them) their outrage in those instances when the occupation authorities did prosecute former Nazis. Pursuit of individual self-interest contributed to a feeling of corporate self interest, and many scientists vouched for one another in order to receive clearance. The most egregious example of this behavior occurred in September, 1949, when Butenandt, Max Hartmann of the Kaiser-Wilhelm-Institut für Biologie, and two others met to help clear Otmar Freiherr von Verschuer of wrongdoing during the Nazi period. Verschuer had been Josef Mengele’s mentor and was undeniably involved in human experimentation on victims from Auschwitz. Thanks to the report produced by Butenandt and the others, Verschuer was able to take a position in human genetics at Münster University, after which he had a long and successful postwar career.

1983), 227. The best discussion of Himmler’s thoughts is Richard Breitman’s The Architect of Genocide: Himmler and the Final Solution (New York: Knopf, 1991), esp. Chapter 1 “Hitler, Himmler, and the SS.” Breitman quotes Hitler as saying “Those who throng to the SS are men inclined to the authoritarian state, who wish to serve and obey, who respond less to an idea than to a man.” 34.

66 Otto Hahn used the subject of de-Nazification to denounce the American occupation. According to one postwar report, “Dr. Hahn himself denounced the American Military Government scathingly, both for dissolving the Gesellschaft and for its whole denazification policy. (See Denazification) The denazification law was a quadripartite law, but was being enforced only in the American zone, and this subjected the United States to the charge of being out to destroy German science. According to Dr. Hahn, we were on ‘as low a status as the Russians’.” Mann, “Report on Educational Conditions in Postwar Germany,” 65.

3.2.2 The Utility of Purity

The sheer scope of the task made de-Nazification imperfect, partial, and even hypocritical. German scientists learned that they could count on deception, mutual support, and external political factors to negotiate a favorable outcome to the de-Nazification process. The pure/applied science dichotomy that had been instrumental in securing both scientific prestige and financial support in the Third Reich became another tool in their arsenal. The promise of research for application became a liability in the postwar environment, but at the same time the idea of purity assumed a greater significance. Mehrten's argues that the idea of pure research offered scientists a symbol with which they could identify that was independent of the social context in which they were operating. It gave them the opportunity to declare that they were apolitical even when working for the interests of the state. When confronted with the occupation, the German TMV community simply had to stress these values and ignore the more pragmatic ones they had articulated during the war. Since many of them truly identified with scientific research as a higher calling that did elevate its practitioners above daily life, they welcomed the chance to embrace purity and the independence it conferred.

Scientific purity became an important tool for negotiating with the occupation powers. German scientists and their supporters could claim that the conduct of quality scientific research was a way of defying the anti-intellectual Nazis. Many in the occupation governments were sympathetic since they believed that the Nazis had done everything they could to destroy science in Germany. This belief allowed researchers to use the rhetoric of purity to depict passive complicity as heroic resistance. In the case of the physical sciences, Historian Mark Walker concluded "By misrepresenting their

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research during the National Socialist period and portraying it as academic 'pure' science, the Germans misled their conquerors deliberately, repeatedly, and successfully." The British, as noted above, were amenable to this conceptualization thanks to their association of academic science with education. Their position was perhaps a more enlightened approach to the nature of science, but it also susceptible to abuse, and it is hardly surprising that the distinction between pure and applied science was used to its best effect to reestablish science in the British zone.

The occupation powers, and the British in particular, were willing to accept the German explanation because it was part of a common scientific culture that they all shared. It was perfectly normal to invoke the idea of purity to create moral and ethical insulation for the researcher; it was the other end of the dichotomy, the application of scientific research, which was rather new and not universally accepted. Scientific research can of course be "pure," in the sense that there are questions that can be pursued whose answers have no foreseeable application other than the satisfaction of intellectual curiosity. Cosmology falls into this category. Most of the research done in the Kaiser-Wilhelm-Gesellschaft, however, fell somewhere between this one extreme and that of empirical tinkering. It was a complex process in which pure knowledge and practical application were linked together in diverse and not always predictable ways. However, since the appeal to purity became such an effective way for individuals and institutes to continue their work, there was a noticeable trend in postwar Germany for scientific rhetoric to shift toward the abstract rather than the practical. The irresponsible purity of

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69 Two of the scientists whose work Walker discussed were Ernst Telschow and Otto Hahn, both of whom were essential in refounding the Kaiser-Wilhelm-Gesellschaft as the Max-Planck-Gesellschaft in the postwar era. Walker, German National Socialism and the Quest for Nuclear Power, 189. This was true of biomedical researchers involved in eugenic crimes as well. See Weingart, Kroll, and Bayertz, Rasse, Blut, und Gene, 581-585.
Nazi era science became the useful purity of the postwar era as scientists used this idea to lobby for autonomy and self-administration in occupied Germany.

3.3 Rebuilding the Research Infrastructure

3.3.1 The Max-Planck-Gesellschaft

The same decentralizing tendencies that led to the promotion of science at the local level in Tübingen operated throughout occupied Germany, and at first, there were no plans to allow for the coordination of scientific research at the national level. After the war, the institutes of the Kaiser-Wilhelm-Gesellschaft, like Berlin and Germany itself, were divided among four occupying powers. Therefore, the successful re-establishment of the Kaiser-Wilhelm-Gesellschaft as the Max-Planck-Gesellschaft just four years after the war was an extraordinary accomplishment that depended upon both the dedication of the German researchers as well as the emerging Cold War. It is an interesting and complex story, one that has been told in detail elsewhere.\textsuperscript{70} However, given the importance of the Max-Planck-Gesellschaft in supporting the TMV work done in Tübingen, some background facts are necessary to establish the context of postwar virus research.

Shortly after his capture by the Americans, Albert Vögler, President of the KWG, committed suicide in April, 1945. With its official leader dead, Ernst Telschow once again became the source of organization and authority in the society, much as he had during the interregnum between Presidents Bosch and Vögler in 1940. Despite his long tenure in the Nazi party, which had earned him the enmity of many of his colleagues,

Telschow was cleared by British de-Nazification courts and was able to act relatively freely in 1945-46. In addition, shortly after Vögler’s death Max Planck came to Göttingen to serve as a figurehead president, lending the society some much-needed integrity. Planck was one of the few German scientists whose opposition to the regime was well-known—one of his sons was even executed in association with the July 20 plot to assassinate Hitler. One of Planck’s first acts was to choose a successor who would be a true acting president rather than an interim figure. He decided upon Otto Hahn, a chemist who had been active in the society since its founding. Hahn had won the 1944 Nobel Prize for physics for his participation in the discovery of nuclear fission. Hahn, though, was unavailable to the society in the early postwar months because he and several other high ranking German physicists and chemists had been interned in Britain after the war to determine the extent of the German atomic weapons program. Nevertheless, Planck quickly arranged for the directors of the Kaiser-Wilhelm-Institutes in the British zone to select Hahn, who began his term as President in April, 1946, shortly after his release by the British.

Hahn had been Telschow’s doctoral adviser. The two had a very close relationship and from 1946-1948 they formed the nucleus of the general administration of

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72 Physicist Lise Meitner, who worked with Hahn and Fritz Strassmann before being forced to leave Germany in 1938 (she was an Austrian Jew) made decisive contributions to this research as well, which neither the Nobel committee nor Otto Hahn ever recognized. See Ruth Lewin Sime, *Lise Meitner: A Life in Science* (Berkeley: University of California Press, 1996).

73 There was a further complication. Robert Havemann, a communist and dedicated anti-fascist who had spent the last two years of the war in a Nazi jail with a pending death sentence, was named as leader of the KWG in Berlin by the city’s administration. Many of the other scientists in the city protested. Havemann was largely excluded from helping to found the MPG in the West; he lost all authority in 1950 when he criticized American nuclear policy. Due to his communist beliefs, he chose to live in the east, but he eventually criticized the DDR’s inflexibility and lack of freedom. He lost his job there as well and spent the last two decades of his life under house arrest. See Heinemann, “Wiederaufbau,” 425-427 and Dieter Hoffmann, “Robert Havemann: Antifascist, Communist, Dissident,” in *Science under Socialism: East Germany in Comparative Perspective* ed. Kristie Macrakis and Dieter Hoffmann (Cambridge: Harvard University Press, 1999), 269-285.
the Kaiser-Wilhelm-Gesellschaft. Their immediate task was to convince the occupation that the KWG had not been a Nazi organization and had not carried out military research. By presenting evidence and events in a fashion that in retrospect looks decidedly selective, they were able to convince the British, but the Americans were less sympathetic. To the Americans, the Kaiser-Wilhelm-Gesellschaft looked like a national cartel for scientific research, and they demanded that it be dissolved. Eventually a compromise solution was reached; the KWG was officially dissolved, but was immediately replaced by a successor that had essentially the same membership, institutes, and a similar constitution. The greatest difference was the name; to most ears, Planck’s name was much more agreeable than that of the Kaiser. On September 11, 1946, the Max-Planck-Gesellschaft was officially founded in the British zone.

The Americans were still skeptical of the society. If in fact its institutes were devoted to pure research, as Hahn and Telschow argued, the Americans thought the institutes would be more effective if they were included in the universities. The scientists of the Kaiser-Wilhelm-Gesellschaft were unanimous in their opposition to this suggestion, arguing that the only way that they could be truly free in their research was to belong to an independent, self-administered scientific society. They claimed that such autonomy had allowed the KWG to resist thoroughgoing Nazification, and that it had a much better record than the German universities in this respect. The presence of individuals such as Planck in the new organization made such claims more credible. German scientists in the American zone requested inclusion in the society, and in fall of 1947, Otto Hahn met with General Lucius Clay to discuss expanding the Max-Planck-Gesellschaft into a bi-zonal association of research institutes. By that point, the inefficiencies of administering Germany along tripartite lines had become very clear.

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74 Hahn was not one of them, he was outraged by the change and threatened to resign his presidency. Mann, "Report on the Educational Conditions in Postwar Germany," 65.
making the Americans more sympathetic to bi-zonal cooperation. They soon gave their approval. On February 26, 1948, the Max-Planck-Gesellschaft was founded for a second time in the city of Göttingen, now including institutes and researchers from the American and British zones. Several constitutional changes that guaranteed the freedom of individual researchers as well as the independence of the society from government and industry were made to alleviate the last American misgivings.

3.3.2 The Tübinger Herren

The unification of the former Kaiser-Wilhelm institutes in the American and British zones was a significant accomplishment, but there was still much to be done in order to create a true successor organization to the Kaiser-Wilhelm-Gesellschaft in the postwar era. The most significant obstacle, as it would turn out, was the community of researchers in Tübingen, who steadfastly resisted incorporation into the new organization. Throughout the late 1940s, Kühn, Butenandt, Melchers, and several others refused to join the Max-Planck-Gesellschaft until its leadership promised to address several pressing issues. These included differences regarding the nature of the organization, concerns over its finance, concerns regarding the scientific freedom of its members, and, perhaps surprisingly, differences over how to handle the Nazi past of specific members.

The directors were not the only source of resistance in the French zone. The French occupation government opposed the incorporation of "its" Kaiser-Wilhelm-Institutes into the Max-Planck-Gesellschaft as well. The French had supported the Tübingen researchers as a means of promoting occupation policies, not out of altruism. After two world wars (and one Franco-Prussian war) in living memory, the French leaders were determined to use the occupation to ensure France's long-term military and economic security. They supported research in their zone because doing so promised to

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75 Heinemann, "Wiederaufbau," 438-447.
give them a voice in German research policy and because research was part of the long-
term process of re-education. In fact, they initially desired to incorporate the research
institutes in their zone into the universities rather than allowing them to maintain their
independent status, but the researchers, like those in the American and British zones,
unanimously refused.76 At the very least, the French hoped to prevent the kind of
centralized control of research that the Max-Planck-Gesellschaft represented.

While they initially disagreed on the appropriate level of independence for the
institutes of the Kaiser-Wilhelm-Gesellschaft, the French occupiers and the scientists
themselves were of like mind when it came to the promotion of research in the French
sector at the expense of science at the national level. Butenandt and Kühn were in regular
contact with the General Administration of the MPG, but in 1945-47, they stood to lose
much more than they stood to gain by joining. In Tübingen they were receiving more
material and financial support than one could reasonably expect in postwar Germany.77
They also had an extraordinary amount of independence; since Schmid and others hoped
that the scientists of the Kaiser-Wilhelm-Gesellschaft would contribute to the spiritual
and intellectual life of Tübingen, they received unlimited intellectual freedom.78 After

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76 In 1946, the French opened a new university in Mainz; they had hoped to move the KWIs from Tübingen
to Mainz and hoped to use them to strengthen the university's natural sciences faculty, but both Kühn and
Butenandt refused to move with the institutes. See Willis, *French in Germany*, 172-177, and Osietzki,
*Wissenschaftsorganisation und Restauration*, 178. This contradictory strengthening of German institutions
despite the French desire for de-concentration mirrors the experience of the BASF chemical company. The
French needed the output of BASF, so they centralized operations and tried to maximize output while
maintaining as much control as possible by only slowly allowing Germans back into positions of
responsibility. Raymond Stokes, *Divide and Prosper: The Heirs of I. G. Farben under Allied Authority,

77 In a letter to Melchers, Joseph Mattauch, then at the KWl for Chemistry in Tailfingen in the French zone
(the new location of Hahn's former institute), wrote that Otto Hahn was concerned about financing new
Kaiser-Wilhelm-Institutes that the French planned on creating in their zone. Mattauch closed the letter
suggesting that Hahn believed that this multiplication of institutes was not in the interest of the MPG and
could pose an obstacle to the future unification of the MPG with the French zone institutes. Mattauch to

78 Schmid was very concerned that the dire material circumstances of the Germans would cause them to
disengage from cultural and spiritual development. He held this to be dangerous, and therefore believed
years of selling their research in the context of agricultural autarky, they could pursue now their investigations unencumbered by the need to justify their work to the state. They did not want to sacrifice their recently won freedom, autonomy, or financial security to a centralized administration in Göttingen. To put it bluntly, the Max-Planck-Gesellschaft had little to offer them.

The self-assured stance of these directors and their unwillingness to cooperate with the institutes in the other two zones led to their designation as the “Lords of Tübingen” (die Tübingen Herren). Efforts at reconciliation only exacerbated tensions between Göttingen and Tübingen. On May 25, 1948 Telschow met with Butenandt, Kühn, Melchers, and four others in Butenandt’s institute, to try to persuade them to change their minds. From Telschow’s perspective, the meeting went extremely poorly. The assembled directors told Telschow that they thought that the Max-Planck-Gesellschaft had been founded too hastily and was too accommodating toward the Americans and British—they also resented the fact that they had not been consulted. They objected to the applied nature of the research done in several institutes and declared that the international reputation of the society as whole was certain to suffer as a consequence. They even gave specific examples of institutes that they believed should be excluded from the MPG. They saw no possibility that they would join the society as of that time. Telschow closed an exasperated report of the meeting, written three days later, with the following: “To my regret I could not establish any kind of understanding with these gentlemen that with the founding of the Max-Planck-Gesellschaft and its initial recognition in the Bi-zone an essential step forward for German research institutes had been reached.”

that the crisis made the support of all aspects of high culture even more important than it would otherwise have been. Schmid, Erinnerungen, 262.

External events soon altered the terms of the debate. The most important of these events was the establishment of a system of funding for German scientific research in all three western zones in early 1949. Representatives of the Länder, the independent states that make up Germany’s federal structure, were anxious to use the Allied desire for decentralization to acquire more competencies for themselves. They demanded that they be sovereign in matters of culture and education, and for them research fell within this rubric. The trick was for the Länder to have an influence on scientific research without infringing on the scientists’ desire for political independence and self-administration. At that point, the Länder were supporting research in their respective territories through agreements with individual institutes. This arrangement linked research to the individual states, but was insufficient to satisfy the financial needs of the German scientific community. Controlling the purse strings was one way that the Länder representatives could continue to assure themselves of a role in research in Germany, but they had to develop a funding system that was reliable without being overly centralized.

Representatives of the Länder met in early 1949 to establish an agreement through which research would be funded by a federation of the state governments. The resulting agreement, the Staatsabkommen der Länder des amerikanischen, britischen und des französischen Besatzungsgebiete über die Finanzierung wissenschaftlicher Forschungseinrichtungen (usually called the “Königsteiner Abkommen,” or “Königstein Agreement) went into effect on April 1, 1949, for an initial duration of five years. According to the terms of this agreement, the individual Länder would contribute to a general fund that would be used to support research in all three zones. In addition, the Land where a particular institute was located was required to pay a supplement, a kind of “self-interest tax” based on a percentage of the specific institute’s budget.\(^{80}\) As a result of

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\(^{80}\) The rules of the agreement are actually rather complex; for a clear discussion see Kurt Pfuhl, “Das Königsteiner Abkommen,” *Mitteilungen aus der MPG* 1 (1959): 285-294. Also see the discussions in
the Königstein agreement, the Länder paid the Max-Planck-Gesellschaft almost DM 12 million in 1950, its first full year of effect.\textsuperscript{81} What had originally been an organization funded largely by private sources would now be almost exclusively supported by public money.\textsuperscript{82}

The Königstein Agreement was an example of the policy of cultural federalism, a reaction to the centralizing tendencies of the Nazi state. One solution for checking the power of the German central state was to encourage a strong federal structure in which regions were given a great deal of power relative to the central government. With the financial situation of the society finally clarified, one of the objections of the Tübinger Herren to membership in the MPG was removed. Since the Länder of the French zone, including Württemberg-Hohenzollern, had signed the agreement, funding for scientific institutes throughout all three zones would now come from the same source.

A second complication that vanished was the objection of the French, which was the result of both politics and finances. The French demanded that German research should be independent of industry (hence their desire to move the Kaiser-Wilhelm-Institutes into the universities) and decentralized. The Königstein Agreement, with its principle of “cultural federalism” solved both of these issues. Furthermore, the changes to the constitution of the MPG, made to accommodate the Americans, also addressed French concerns by guaranteeing that research would not be directed toward political or

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\textsuperscript{81} The exact amount was DM 11,859,600, more than half the DM 21,917,900 generated in total. Pfuhl, “Das Königsteiner Staatsabkommen,” 291.

\textsuperscript{82} This transition from public to private funding had actually begun many years earlier, in the Weimar Republic, and by 1933 public money provided a substantial portion of the budget of the Kaiser-Wilhelm-Gesellschaft, which is why so many of its institutes were subject to the Law for the Restoration of the Professional Civil Service. As the example of the Arbeitsstätte für Virusforschung illustrates, government money became an even greater part of the society’s budget under National Socialism. In this sense, the Königsteiner Abkommen was not a break in tradition, but instead the next step in a decades-long process of increasing state support for the KWG/MPG.
economic goals. Finally, by 1949 the overall political situation had convinced the French that further coordination of their zone with that of the other two western Allies was necessary. The Soviet blockade of Berlin, beginning in 1948, demonstrated that Germany was already effectively divided between east and west. That same year Marshall Plan aid from America began to arrive, alleviating French concerns about their economic security and allowing them to take a policy less at odds with Britain and the United States.\(^\text{83}\) The North Atlantic Treaty, signed on April 4, 1949, committed the United States to the security of western Europe and encouraged greater cooperation among the western Allies in Germany. Shortly afterward, the French merged their zone of occupation with the bi-zone of Britain and the United States, completing the economic unification of western Germany. Economic unification led, in very short order, to the political unification of the zones as the Federal Republic of Germany in May, 1949. From that point, the French were more tolerant of the idea of a national German science organization, and in July, they recognized that the constitution of the Max-Planck-Gesellschaft was valid for the entire tri-zonal area.\(^\text{84}\)

3.3.3 "Der Zuckerbacker"

The French approval and the securing of the society's finances removed two of the obstacles standing between the unification of all former Kaiser-Wilhelm-Institutes in the western zones. Nevertheless, in the late summer and early fall of 1949, the intransigence of the Tübingen Herren did not decrease, as one might expect, but if anything grew stronger, indicating that their refusal to join was based not only on political and financial grounds, but on philosophical and personal differences as well.

\(^{83}\) Willis, *The French in Germany*, 46-47.

Philosophically, they continued to insist that the institutes of the society should be dedicated to pure research; despite the assurances of the society's constitution, a number of its institutes, such as those for brain research and fiber research, pursued topics that had immediate medical or industrial applications. More important, however, was the personality conflict between the Tübingen directors and the secretary of the Max-Planck-Gesellschaft, Ernst Telschow, which focused on his behavior during the Third Reich.\(^85\) Telschow became the one irreconcilable point that prevented the unification of the society until November, 1949.

Telschow had joined the Nazi party in 1933. As noted above, in 1937 he succeeded Friedrich Glum as general secretary of the Kaiser-Wilhelm-Gesellschaft, and by the late 1930s, he had become one of its most powerful members, coordinating its activities with the Nazi government. After the war, he claimed that he had used his position to keep the society free of political influence, which apparently was sufficient to convince the British to allow him to continue in his position as general secretary.\(^86\) Several scientists of the KWG, however, including Melchers and Kühn, were not convinced. They were well aware of Telschow's activities during the war years, having worked with him to secure funding for the virus research center. They were appalled that Telschow had been permitted to retain such a position of influence after his early and consistent association with the Party. Melchers in particular had nothing but contempt for Telschow; in his correspondence, he referred to Telschow as "der Zuckerbacker," ("confectioner," or "pastry chef") a condescending reference to the famous bakery owned by his family. Telschow's continuing presence as general secretary was the one point on

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\(^{85}\) The Tübingen directors were not the only scientists unhappy with Telschow's previous conduct. Apparently the biochemist Otto Warburg told an American visitor that Telschow was "The worst Nazi he knew and never a scientist." Mann, "Report on the Educational Conditions in Postwar Germany," 70. Also, see Heinemann, "Wiederaufbau," 410-411.

\(^{86}\) Heinemann, "Wiederaufbau," 420.
which the Tübingen directors were unwilling to compromise. In September, 1949, they sent a letter to the general administration of the Max-Planck-Gesellschaft demanding that Telschow step down as general secretary as one of the preconditions of their joining. Less than a week later, Melchers wrote to a friend:

To this point it was not possible for the KW-Institutes in the French zone to join with those in the other zones, that for a while have been re-named Max-Planck-Institutes. Recently the official barriers have been completely removed, but since a few of us find fault with the general administration in Göttingen and its postwar activities, and above all with its leader, Mr. Telschow, I cannot at this time say if and when the final reunification of all former KW-Institutes will take place.\(^{87}\)

In early October, Melchers wrote to the physicist Karl Wirtz reaffirming the willingness of the Tübingen directors to abandon the MPG if Telschow were retained.\(^{88}\)

Several days later, though, the Tübingen directors conceded, opening the way for the official re-unification of all institutes in the three western zones on November 18. What had changed? According to Melchers’ account of the events, the opposition collapsed when Butenandt broke ranks and sided with the Göttingen administration. In return, Butenandt and Hans Dölle, director of the Kaiser-Wilhelm-Institute für Privatrecht, also in Tübingen, both received seats in the Senate of the Max-Planck-Gesellschaft. These positions were created to provide a counterbalance to Telschow’s influence, and were given to Butenandt and Dölle since they were perceived as the most moderate of the Tübinger Herren. Melchers was outraged and referred to Butenandt’s defection as treason. He believed that Butenandt had reduced the opposition of the Tübinger Herren from a morally justified critique of the society to the complaints of a

\(^{87}\) Melchers to K. Mothes, September 30, 1949. Melchers Papers, Ordner 1.

\(^{88}\) Melchers to Wirtz, October 10, 1949. Karl Wirtz Nachlass, Generalandesarchiv Karlsruhe, Nr. 3. I would like to thank Cathryn Carson for informing me of this document.
couple of cranky old Jacobins (Melchers and Kühn). Their legitimacy gone, they saw no alternative but to acquiesce, and their institutes were incorporated into the Max-Planck-Gesellschaft that November.

Without access to Butenandt’s papers, we currently have only Melchers’ account of the above episode, which of course should be read with caution. Melchers was very straightforward and honest, but he was also stubborn, opinionated, and harshly critical of nearly everyone who disagreed with him. His frankness and strength of character won him many admirers abroad, but were often not appreciated by his colleagues in Germany. In his own lab he could be authoritarian, was often short with people, and could blow up for little reason, but he was also fiercely loyal to his friends and students and had an excellent, if sarcastic, sense of humor, which is on prominent display in his extensive correspondence in the Max-Planck-Gesellschaft’s archives. Since he was often in the right on moral questions, he associated his own opinions, even in questions of personal choice regarding administration of the Max-Planck-Gesellschaft, with the morally correct position. Melchers’ inflexibility, coupled with his sometimes abrasive attitude toward his colleagues, could form a recipe for conflict when the actual reasons for antagonism were slight.

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89 Melchers to Anton Lang, begun November 1, 1949, continued November 5. Melchers Papers, Ordner 1. In addition one must note that the Tübingen directors did not represent all the KWIs in the French zone. The KWI for chemistry, which had been evacuated to Tailfingen, was Otto Hahn’s former institute, and his co-worker, Fritz Strassmann, continued to work there. Strassmann and Joseph Mattauch, the overall director of the institute, may have shared some of the Tübingen Herren’s general reservations, but I have found no evidence that they agreed with the opposition to Telschow.

90 Gerald Pomerat, a representative of the Rockefeller Foundation, later described Melchers as “obstinate as a mule.” “Diary of Gerald R. Pomerat, 1960.” RAC, Record Group 1.2, Series 717, Box 6, Folder 68.

91 In his memoirs, Gunther Stent wrote “Melchers was the only senior German biologist of whom Max [Delbrück] was dead certain that he had never been a Nazi—a judgement for which, having known and admired Melchers for forty-five years, I would thrust my own hand into the fire.” Stent, Nazis, Women, and Molecular Biology, 352.

Butenandt’s biographer does not mention the episode at all, and in an interview later in his life, Butenandt himself discussed the phenomenon of the Tübinger Herren, but he mentioned only the desire of himself, Melchers, and Kühn to eliminate applied research institutes from the Max-Planck-Gesellschaft. Still, from the evidence available, some tentative conclusions are possible. Butenandt was never as devoted in his opposition to Göttingen as were Melchers and Kühn. He certainly believed that scientific research should be independent of direction by industry or government, and he believed that several of the postwar Max-Planck-Institutes, such as the Institute for Metal Research, were not really scientific institutes but instead were glorified workshops. His willingness, in 1948-1949 to accept DM 100,000 from the chemical industry to equip his own lab suggests that his opposition was not based on antipathy toward the general idea of research for practical applications, but instead on the quality of the research and the specific ends to which it would be put. Butenandt could not take a principled stand against Telschow for his complicity with the Nazi party, since Butenandt had also joined. Making a public event of the situation, as Melchers hoped to do, would in all likelihood have damaged Butenandt’s reputation as well — after all, September, 1949, was when Butenandt helped to clear Verschuer.

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95 One of Butenandt’s students, Hans Georg Zachau, later wrote that in contrast to many of his colleagues, Butenandt had no reservations about working closely with industry. “Adolf Butenandt als Wissenschaftler und Lehrer,” in MPG Berichte und Mitteilungen 4 (1995): 22.
Finally, Butenandt had not benefited from the generosity of the city of Tübingen to the same extent that Melchers and Kühn had. The division for virus research had received its own building, but Butenandt’s Institut für Biochemie remained dispersed in several different university buildings. With the Königstein agreement in place, there was little reason for him to think that joining the Max-Planck-Gesellschaft would compromise the financial security of his institute in any way. In fact, the Tübingen institutes had faced a serious financial crisis in the wake of the currency reform, and in response reduced the salaries of their workers to 70 per cent of the contracted amount. This significant reduction in wages came at a time when the cost of living was steadily rising.  

Therefore, by continuing his opposition, Butenandt risked both financial insecurity and further damage to his own reputation. By switching sides, he assured himself of a position of influence within the general administration of the Max-Planck-Gesellschaft, secured the financial future of his institute, and buried his past.

Kühn’s and Melchers’ dedication to all of the above issues was much stronger than that of Butenandt. Even after their inclusion in the Max-Planck-Gesellschaft, Melchers worried that membership would hamper their financial support, partially because they would no longer be favored, as they had been by the Tübingen government, and also because Telschow presumably was in no mood to be generous to them.

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96 For example, see Melchers to Otto Hahn, September 6, 1948. In a letter to his cousin Georg Koch, dated September 16, 1949, Melchers wrote “Obviously much has become easier since the currency reform, but the financial burdens on people with fixed incomes has become terrible due to the tremendous increase in the cost of living.” Both letters Melchers Papers, Ordner 1. The currency reform also dramatically reduced the accounts of the city government making it difficult for them to fulfill their obligations. [No first name given] Sauer, “Memorandum über die nach Württemberg Hohenzollern verlagerten Forschungsinstitute der Max-Planck-Gesellschaft, besonders Kaiser-Wilhelm-Institut für Biologie.” October 25, 1950. Melchers Papers, Ordner 88.

97 Melchers wrote to Kühn, “What do you hold to be more essential for biology in Germany—that we are members of the MPG, or that the entire institute is built in the foreseeable future? If you hold the construction to be more important—I do, even though my own division is practically finished—is the resignation from the MPG not even to be considered?” Melchers to Kühn, December 8, 1949. Melchers Papers, Order 1.
two, Kühn certainly believed in the need to keep scientific research independent of economic or political direction. He had never been associated with industry or directed research, and he had not been closely involved in selling the virus research group to the Nazi regime based on its utility.98

Both Melchers and Kühn were firmly in agreement in their opposition to individuals whom they believed had worked too closely with the Nazi regime. Neither Kühn nor Melchers had joined any significant party organizations, and in the immediate postwar years both were harshly critical of their colleagues who had.99 Telschow was not the only person whose behavior concerned them. Kühn demonstrated this in no uncertain terms in 1949 in a letter regarding the publication of research by Karin Magnussen in the Zeitschrift für inductive abstammungs- und Vererbungslehre, which Kühn helped to edit. The letter reads as follows:

Very esteemed Fräulein Dr. Magnussen!

I am returning the galley proofs of your manuscript “The Function of Color Genes on the Development of Pigment in Rabbit Eyes” to you. The typeset was destroyed at the same time as was the publishing house. The publisher of this journal rejects any further reproduction of your publication. I will explain the reason why honestly.

Your National Socialist and anti-Semitic convictions were known to me. At that time many young people succumbed to these deceptions and have since

98 Jonathan Harwood characterized Kühn’s scientific style as being “comprehensive”—that is, Kühn pursued science in order to answer fundamental questions such as the nature of man, not for utilitarian purposes. Harwood contrasted the approach of Kühn (and Wettstein) with others whom he called “pragmatists,” researchers whose work was narrower in scope and targeted toward application. Jonathan Harwood, Styles of Scientific Thought: The German Genetics Community 1900-1933. (Chicago: University of Chicago Press, 1990), 230-231, 243, 299. Melchers was perhaps a bit less extreme in this regard—even after the war he did publicly discuss the potential relevance of virus research for agriculture. In a postwar report on the research of the KWI for Biology, Melchers is cited as pointing out how freely directed, fundamental research can have important applications in agriculture. Hans Kretzer, “Wissenschaft hilft dem Bauern: Aus der Arbeit im KWI f. Biologie.” November, 1948. Melchers Papers, Kasten 1.

99 Melchers’ lack of commitment to the Nazi party has been substantiated by many of his colleagues; Kühn’s file in the Max-Planck-Gesellschaft Archive shows that he was never a member of the party either. MPG Archiv, Alfred Kühn Papers, III. Abt., Rep. 5, Nr. 4.
recognized their mistakes. Whether or not this is the case with you I do not know. But we have learned that while in the KWI for Anthropology you also worked on human material, on gypsy eyes from the camp Auschwitz. It is incomprehensible to me how a person could support a connection to such a horrible institution. In order to ensure that this is not a rumor, we have inquired with the leader of the KWI for Anthropology in Dahlem, and Prof. Nachtsheim confirmed the facts.

From this point we refuse to accept publications from you. Your formal clearance through a de-Nazification court (Spruchkammer) does not change this.

A. Kühn

Melchers also believed that the Max-Planck-Gesellschaft was too lenient regarding membership for Nazi collaborators. In addition to Telschow, he criticized Otto Hahn for not opposing the de-Nazification of Wilhelm Rudorf, director of the KWI für Züchtungsforschung during the war. One of Melchers’ greatest concerns was that allowing such people to take positions of authority amounted to “re-Nazifying” German science and would result in condemnation from the international scientific community. In 1948, Melchers and several of his colleagues had been invited to attend the 8th International Congress of Genetics in Stockholm. In his Presidential address, American geneticist Hermann J. Muller announced that he did not hold all German scientists responsible for the crimes of the Nazis, but cautioned that the threat to German science was not yet over. He said, “Nevertheless this danger is by no means completely past, as is shown, for example, by the increasing influence of ex- or not-so-ex-Nazis among the staffs of the German universities in the Western zones.”

Melchers immediately reported Muller’s comments to Otto Hahn. Melchers wrote that his experiences at the Congress had convinced him that the Germans

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100 Alfred Kühn to Karin Magnusson, January 1, 1949. Melchers Papers, Ordner 1.


themselves would have to make efforts to ensure that the scientific profession was not re-
Nazified. He continued:

It is hardly tolerable that long term Party comrades are directors of our greatest
research institutes; however, it seems to me to be fully intolerable that such
people have remained or become deacons, section heads in the Scientific
Advisory Board (Wissenschaftlichen Rat) of the MPG, and so on. More and more
conversations with foreign colleagues about this topic have indicated that the re-
establishment of normal international relations will depend to a very great extent
on the conduct of the Germans on this point.\footnote{Melchers to Otto Hahn, July 26, 1948. Melcher’s repugnance for active National Socialists lasted for
many years. For example, in 1958 he wrote to several colleagues in an effort to mobilize support against
inviting a Professor Lauprech to join the Deutsche Gesellschaft für Vererbungswissenschaft due to
Lauprecht’s anti-Semitism and active support for the Nazi party. Melchers to Süers, Nachtsheim,
Schiemann, and Stubbe, December 13, 1958. Melchers Papers, Ordner 14.}

The letter clearly contains a veiled barb against Telschow, but it also illustrates a more
important general point. The international community expected the German researchers
themselves to take responsibility for their own past as one of the preconditions for
normalizing relations. Melchers and Kühn were two of the few German scientists who
recognized the importance of this task and hoped to fulfill it.\footnote{For more on the failure of most German researchers, including Butenandt, to engage in critical self-
examination, see Ute Deichmann, “The Expulsion of German-Jewish chemists and biochemists and their
correspondence with colleagues in Germany after 1945—the impossibility of normalisation?” in Science

They failed in this task because for them, just as for the Allies, precisely defining
a Nazi was impossible. For example, neither Melchers nor Kühn agreed with
Butenandt’s wartime behavior, but they were willing to ally themselves with him against
Telschow. Based on Melchers’ 1948 evaluation of Butenandt’s personality and political
leanings, he did not regard Butenandt as a true “Nazi.” For Melchers and Kühn, there
was a border between passive complicity and active support that Telschow had crossed,
while Butenandt had not. The problem was that this border was not easy to define—it
was simultaneously personal, moral, and professional. Thus the repugnance of Melchers
and Kühn toward Telschow's close collaboration with a criminal government was inseparable from their own personal dislike for his authoritarian style of leadership. In the postwar years, their opposition to Telschow and Hahn had a moral component, but it was also a defense of their own privilege as well as part of a strategic plan to re-integrate German science into the international community.

Promoting a critical re-examination of their own past would have required those scientists who had not collaborated with the regime to mobilize their colleagues against those whose behavior was inexcusable. Such a call for the moral examination of the society could not be extricated from efforts by individuals to secure professional and financial security, and in fact opened up the possibility of the accusation of "Nazi" becoming a tool to settle personal scores.\textsuperscript{105} It was therefore very difficult for people like Melchers and Kühn to persuade other scientists to engage in such a process. Out of selfishness and insecurity the entire society turned away from its past. More generally, individual researchers, both in the Max-Planck-Gesellschaft and outside of it, either completely ignored the past, or when they did speak of it, they fell back on the idea of pure research to portray their complicity as resistance to the anti-intellectual Nazis.\textsuperscript{106} They had alternatives, however. The campaign of the Tübinger Herren against Telschow demonstrates that the kind of critical self-examination that the Max-Planck-Gesellschaft is currently undergoing was not impossible in the postwar era.

\textsuperscript{105} Melchers was undoubtedly aware of the power of the word Nazi, and in a letter to his close friend Anton Lang indicated that the Tübinger Herrn had considered using public opinion as a weapon in their campaign against Telschow. He wrote "It would have been completely unthinkable to have to explain to the public that the Institute for Biochemistry (Butenandt, Schramm, Freksa), Biology (Hartmann, Kühn, Melchers), Private Law (Dölle, Erbe, Zweiert, Rupp, Makarov), and Physics in Hechingen would not join the MPG because it stood by the old Nazi Telschow." Melchers to Lang, November 1/5, 1949. Melchers Papers, Ordner 1.

\textsuperscript{106} For an analysis of several examples, see Benoît Massin, "Anthropologie und Humangenetik im Nationalsozialismus oder: Wie schreiben deutsche Wissenschaftler ihre eigene Wissenschaftsgeschichte?" in Wissenschaftlicher Rassismus: Analysen einer Kontinuität in den Human- und Naturwissenschaften, ed. Heidrun Kaupen-Haas and Christian Saller (Frankfurt: Campus Verlag, 1999), 12-64.
3.3.4 The Max-Planck-Institut für Virusforschung

Although a few significant personnel changes had accompanied the transition from Dahlem to Tübingen, the core of the TMV group remained intact. The Arbeitsstätte für Virusforschung, originally split between the institutes for biology and biochemistry, became a division of the Institut für Biochemie. Within this division, Schramm and Freksa continued to serve as the leaders of two of the three sub-divisions. Melchers continued to pursue TMV work as a division leader in the Institut für Biologie, and remained in close contact with the others. Their continuing presence, more than any other single factor, accounts for the consistently high quality of the research on TMV and other viruses during the occupation.

Gerhard Schramm's intelligence, enthusiasm, and charisma made him a valuable asset to the virus group after the war. As a student, he had impressed Butenandt with the breadth of his knowledge and his desire to tackle questions of great significance in biological chemistry. To impose a sense of order on his protégé, Butenandt assigned Schramm a dissertation topic in synthetic organic chemistry that taught him the importance of discipline and rigor in the chemistry lab. After this foundation was set, Butenandt had no reservations at all about Schramm's ability to tackle the most difficult of problems.\(^\text{107}\) Tall, handsome, and outgoing, Schramm made a good impression on people and often represented the entire Tübingen TMV research group abroad after the war. He was very open and did not behave like a big boss toward his students. Throughout his career, he developed a reputation for presenting material so enthusiastically that he would finish demonstrations or lectures with his hair and clothing completely rumpled. His outgoing nature, his broad intellectual and artistic interests (he

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was a talented painter as well), and his ability to recognize exciting new areas of research led his successor in Tübingen to describe Schramm as the “heart” of the institute.  

Hans Friedrich-Freksa's training in zoology and biophysics provided him with an intellect so far-ranging that even in a group of researchers characterized by their openness and interdisciplinarity he clearly stood out. If Melchers' frankness and honesty made him the group’s postwar conscience, and Schramm’s energy and charisma made him the heart, Freksa was the brain. His colleagues remember him as being open-minded, original, and having broad intellectual interests as well as a liberal attitude toward his co-workers. He was perhaps less outgoing than the others; one of his students recalls that he seemed to spend much of his time in his office with a mountain of books and the lights off, thinking and reading. However, his colleagues also recall that he was present at nearly all post seminar discussions, even those that ran until early in the morning, and was rarely without a glass of wine in his hand.

As I have noted, Schramm, Freksa, and their co-workers were able to continue their work on TMV thanks largely to the generosity of Tübingen University. Prof. Otto Stickl, director of the university’s hygiene institute was especially helpful and allowed Schramm’s entire group to be housed in his building. Nevertheless, conditions were extremely crowded, and it was far from an ideal situation. The city of Tübingen came to the rescue, though, just as it had with the institute for biology. In 1947, the city began construction of a new building to house the virus research group. The local government

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108 Details on Schramm’s personality come primarily from interviews with his colleagues; in particular I have relied on interviews with Alfred Gierer, Tübingen, May 23 and 25, 2000, Karl-Wolfgang Mundy, Stuttgart, May 25, 2000, and Heinz Schaller, Heidelberg, July 21, 2000.

persevered despite shortages of materials and money, and Butenandt helped to equip the new building with the funds he received from the chemical industry. In early 1950, the new building, located at 36 Melanchthonstrasse, was completed.\textsuperscript{110} All three subdivisions were housed in the new building, which itself quickly became crowded. In addition to the scientific labs, the families of Werner Schäfer and Gerhard Schramm lived on the top floor of the building, and there was a communal kitchen that was used by all of the scientific workers.\textsuperscript{111} This rather intimate situation helped foster communication between the various research groups but it was also indicative of the ongoing shortage in housing and laboratory space in Tübingen.

None of the researchers felt this shortage more acutely than did Butenandt. In 1952, seven years after the end of the war and nine years after the evacuation from Dahlem had begun, he was still occupying space in several university buildings rather than his own institute, despite the fact that plans for a new building for him had existed since 1945. The proliferation of research institutes in the Tübingen area temporarily ended in the early 1950s, and Butenandt saw little opportunity for improvement in his current location.\textsuperscript{112} The unexpected death of Amandus Hahn, holder of the chair of physiological chemistry at Munich's Ludwig-Maximilians-Universität, opened an exciting new possibility for Butenandt. In the autumn of 1952, Butenandt received an offer to succeed Hahn as professor of physiological chemistry in Munich. Butenandt was intrigued but was unwilling to step down as director of the Institut für Biochemie. As I have already noted, he had grown fond of his joint position as university professor and


\textsuperscript{111} Interview with F. Alfred Anderer, Tübingen, July 28, 2000.

\textsuperscript{112} By the early 1950s the construction costs of the scientific institutes in Tübingen had dramatically exceeded estimates, leading many to question whether Württemberg-Hohenzollern could continue to pay construction costs. "Tübingen Planck-Institute in Schwierigkeiten." \textit{Neue Zeitung} 19 March, 1951.
institute director, and requested that he be able to continue this arrangement in Munich. Of necessity, that meant moving the biochemistry institute to Munich as well.\footnote{The episode is very thoroughly analyzed in Stephan Deutinger’s “Vom Agrarland zum high-Tech-Staat: Zur Geschichte des Forschungsstandorts Bayern.” (Ph. D. diss., Ludwig Maximilians-Universität, München, 1998), esp. 125-143. The dissertation has recently been published as Stephan Deutinger, \textit{Vom Agrarland zum high-Tech-Staat: Zur Geschichte des Forschungsstandorts Bayern 1945-1985} (München: Oldenbourg, 2001).}

Most of the following year was spent in negotiations with various groups in Munich in an effort to secure the best possible deal for Butenandt. The city of Munich badly desired to “win” Butenandt for themselves. They believed that he and his institute would serve as a magnet, attracting students, industry, and research dollars to a city where the postwar economy was largely agrarian. Butenandt’s status as a Nobel Prize winner added to his appeal, and he was able to achieve his desired goals by negotiating from a position of strength.\footnote{In 1949 Butenandt received the Nobel Prize he had been forced to reject in 1939, but did not receive the cash award that normally accompanies the prize.} In addition to his chair at the university, he was promised a new research institute and support buildings. The total cost was estimated at roughly DM 5.835 million, out of a total budget of DM 17 million for reconstruction of the university as a whole.\footnote{Deutinger, “Vom Agrarland…” 141.} The local leadership in Tübingen was unable to match the offer, and in December Butenandt declared his intention to move to Munich as soon as his new buildings were completed in 1956.\footnote{“Landesregierung bedauert Butenandts Weggang,” \textit{Schwäbisches Tageblatt} 23 December, 1953.}

Since the Division for Virus Research was already housed in its own institute on Melancthonstrasse, it made little sense to move it to Munich as well. By 1953, the virus group was producing a steady stream of publications, and Butenandt could write with justifiable pride that it was the most successful virus research group in West Germany. Ongoing collaboration with the Institut für Biologie was an essential feature of the virus
group's work, and it seemed likely that separating the two would cause work in both to suffer. Furthermore, Butenandt did not want the people of Tübingen to feel that they had been slighted. Butenandt and the other directors believed that the best choice would be to allow the division to remain in Tübingen as a fully independent Max-Planck-Institut. Separating the division for virus research and granting it independence would mean that Tübingen would have the same total number of Max-Planck-Institutes and would still be the center of West German virology after Butenandt's move. Butenandt articulated all of these justifications for the creation of a new Max-Planck-Institut in a long letter to Otto Hahn written in December, 1953.117

While Butenandt was not planning to leave Tübingen until 1956, the process of transforming the Division for Virus Research into an independent Max-Planck-Institut became more urgent in early 1954, when Hans Friedrich-Freksa received an offer to head a research institute to be created in the city of Essen. Freksa hoped to remain in Tübingen, but was willing to do so only at what he deemed to be the appropriate level of authority—that of division leader of a Max-Planck-Institut. For this reason, Butenandt advocated haste, advising Otto Hahn to conclude the appropriate negotiations by April 1 so that Freksa would have a counter offer with which to balance the one from Essen.118 Freksa applied pressure to the administration of the Max-Planck-Gesellschaft as well. In late February, he repeated his own desire for a clear indication of his future prospects in the MPG, and he also mentioned that Werner Schäfer had received an offer from a Federal Research Institute in Bonn. Since Schäfer was unsure of his own chances of


securing a long-term position in the MPG, Freksa warned that the society was running the risk of losing one of the most talented young virologists in West Germany.\textsuperscript{119}

Butenandt's call for haste was difficult to achieve, though, because of the very nature of the institute he and his colleagues proposed to create. The constitution for the new virus institute, as proposed, contradicted the statutes of the MPG. Article 13 of the society's statutes outlined the powers of the Senate; one of these was the right to appoint institute and division leaders based upon recommendations of the society's scientific advisory board.\textsuperscript{120} The system proposed by the Tübingen directors technically would have violated this rule by allowing the overall director of the institute to have the power to appoint his subordinates independent of consultation with the Senate and Advisory Board. The leadership of the Society believed that limiting the power of the its governing bodies in such a way would set a bad precedent that might in the future compromise its central administration.\textsuperscript{121} Clarifying the status of the new institute relative to section 13 of the society's statutes became the top priority of the meeting of the Biological/Medical commission of the Max-Planck-Gesellschaft's scientific advisory board on March 7-8. As a result of this meeting, the Tübingen directors failed to secure the new organizational changes they desired, but the new Max-Planck-Institut für Virusforschung was approved by an overwhelming majority (ultimately forty members of the commission voted in


\textsuperscript{120} Satzung der MPG, Article 13, Paragraph 1, Section c. Joachim Hämerling explained how the organizational structure proposed by Butenandt differed from other institutes in the MPG in a letter to Freksa from early February. Hämmerling to Freksa, February 5, 1954. MPG Archive, II. Abt., Rep. 1A, Virusforschung 5 (1954-1974).

\textsuperscript{121} In March, 1954, Prof. Hans Dölle of the MPI for Foreign and International Private Law, also in Tübingen (and himself a senator) provided legal advice on reconciling the proposed structure of the MPI für Virusforschung with the statutes of the Max-Planck-Gesellschaft. He concluded that it would be a bad precedent to limit the powers of the Senate in such a way, even though the desires of the Tübingen directors were not unreasonable in and of themselves. Hans Dölle to Joachim Haiemerling, March 6, 1954. MPG Archive, II. Abt., Rep. 1A, Virusforschung 5 (1954-1974).
favor of the institute, one against, and two abstained). Hans Friedrich-Freksa was named
director of the overall institute as well as leader of one of its three divisions, while
Schramm and Schäfer were named as the other two division heads. The institute
received its independent status on April 1, 1954, exactly thirteen years to the day after the
creation of the Arbeitsstätte für Virusforschung.

3.3.5 The Max-Planck-Gesellschaft and the Universities

Butenandt’s joint appointment as both professor of physiological chemistry and
leader of a Max-Planck-Institut was a rare instance of close cooperation between the two
most important organizations for research in postwar Germany. Collectively, the
memberships of each had equated their own identities with a higher ideal, the MPG with
pure research, and the universities with their role as scholarly communities independent
of the concerns of daily life. Both therefore believed that pursuing their own self-
interests would be in the best interest of German scholarship as a whole, and both tended
to defend their own corporate privileges against one another in lieu of working
cooperatively. They divided, instead of sharing, the limited material, financial, and
human resources that were available. Of these resources, outstanding people were both
the most indispensable and in the shortest supply. The impossibility of transcending
organizational interests in pursuit of a common national strategy for scientific research
under the occupation powers meant that this precious resource was inefficiently utilized,
handicapping German biological research for decades.

The complicity of the universities with National Socialist rule was beyond doubt.
University officials could not look to their wartime activities, even selectively presented,
as a means to justify their re-establishment after the war, so there was considerable room

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122 Joachim Hammerling to Otto Hahn, March 18, 1954. The final count of the votes was submitted by

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for idealism in suggesting reforms for the universities. One of the first to articulate a coherent set of suggestions was the philosopher Karl Jaspers, an opponent of Nazism who had been suspended from his teaching position in Heidelberg in 1937. Jaspers published a pamphlet called “The Idea of the University” (Die Idee der Universität) in 1946, in which he wrote that the new university should provide professional training, cultivation and education of the individual, and research, all integrated as part of an unending quest for knowledge. The university should have full independence from politics and daily affairs in order to pursue this goal, but Jaspers gave it an important social role—instilling intellectual confidence and responsibility in the individual. A number of conferences and commissions met in the postwar years and suggested practical ways of implementing the kind of reforms suggested by Jaspers. Individuals in the occupation governments were also interested in reforming and democratizing German higher education.

The ideals of the reformers were not easy to implement in the circumstances of the German educational system after the war. The Nazi purge of the academic community and the damage of the war had removed many valuable scholars. Denazification removed many more, and most young academics were viewed with suspicion. One American report referred to the entire 25-45 age cohort as a “lost generation.” The majority of university professors left to begin the process of rebuilding were therefore those who were old enough to have established successful

123 For an introduction, see Max Weindorf, Hitler’s Professors: The Part of Scholarship in Germany’s Crimes Against the Jewish People. With a new introduction by Martin Gilbert (New Haven: Yale University Press, 1999).


125 The best known of these are the “Gutachten zur Hochschulreform vom Studienauschuss für Hochschulreform,” (also called the “Blaues Gutachten”) produced in 1948. See Rolf Neuhaus, ed., Dokumente zur Hochschulreform 1945-1959 (Wiesbaden: Franz Steiner Verlag GmbH, 1961), 289-368, as well as the background in Stamm, Zwischen Staat und Selbstverwaltung, 65-68.

careers prior to the rise of National Socialism. After the lost war, they desired to return to some kind of normalcy, which for them was the academic world of late Imperial/Weimar Germany.\textsuperscript{127}

They expressed their desire to return to the world of their academic apprenticeship in the language of the Prussian reform period, consistently invoking the ideals of Wilhelm von Humboldt, foremost of which were the freedom of the individual researcher and the unity of research and teaching. However, the world of the German university had changed between the early nineteenth and early twentieth centuries, and the German academic profession after World War II sought to restore an institution that had deviated from the ideals of Humboldt.\textsuperscript{128} By 1900, the German academic elite was growing estranged from a society that was becoming modern and industrial in character. Enrollment in German universities had increased, and the population had diversified to include increasing numbers of women, Catholics, and foreigners. The number of full professorships did not increase proportionately, which made them more difficult to attain. A lengthy training period became necessary, after which one usually had to wait several years before receiving a university position. Only those from propertied backgrounds could afford this process and thus only the privileged elite had access to an academic world that was itself becoming more prestigious and exclusive. Faced with a student body that was increasingly interested in university education for career and professional purposes rather than for scholarship, the academic “mandarins” of the German


\textsuperscript{128} See Rüdiger vom Bruch, “A Slow Farewell to Humboldt? Stages in the History of German Universities, 1810-1945,” in German Universities Past and Future, 3-27.
universities felt themselves under siege. They responded not by engaging the world around them but instead by withdrawing from it.¹²⁹

The lost war confirmed the German professors' aloofness and inchoate sense of dissatisfaction with the modern world, which was readily noticed by foreign observers during the occupation. A delegation of eight British university professors, upon visiting the universities in the British zone of occupation in early 1947, noted:

The German universities are now dominated, so far as internal affairs are concerned, by a compact group of elderly conservative and nationalist professors who constitute the 'hard core' of the teaching body, who are stubbornly unresponsive to new ideas, and whose main aim is to restore as completely and as rapidly as possible the academic system and the academic style which prevailed before 1914.¹³⁰

Authoritarianism remained strong, and the British delegation reported that “we feel that we should place in the forefront of our Report our strong and unanimous impression that no radical and lasting reform of the universities which we have visited is likely to be initiated from within the universities.”¹³¹ An American group concluded “Whatever contribution the German universities made toward the maintenance of a democratic society before the war, they are making no more and probably less of a contribution


¹³⁰ "The Universities in the British Zone of Germany.” RAC, Record Group 1.1, Series 717, Box 2, Folder 15, p. 9.

¹³¹ Albert Mann, Report, Conferences at State and War Departments concerning activities in Germany and Austria. September 4, 1946. RAC Record Group 1.1, Series 717, Box 4, Folder 21, “Universities in the British Zone of Germany,” Section II, Point 8.
now.”\textsuperscript{132} In the end, the universities were not reformed or democratized, but rather restored along lines that had been outdated dozens of years earlier.\textsuperscript{133}

The researchers of the Max-Planck-Gesellschaft had no desire to be part of this environment. Both Butenandt and Kühn had welcomed the chance to leave the university setting for the research institutes of Dahlem, and despite their joint university appointments, neither was willing to have his institute incorporated into the universities in the postwar years. Researchers in the MPG as a whole believed that the society should continue to carry out the most advanced research in the physical and biological sciences in postwar Germany. The separation of research from the university meant fracturing the Humboldtian ideal of the unity of teaching and research. According to one of the Rockefeller Foundation’s postwar observers, the physicist Max von Laue believed “that the MPG will have the principal share in the redevelopment of research in Germany, but he admits that it will not teach, nor give lectures, nor receive students in the lower academic categories.”\textsuperscript{134} To enforce this separation, Max-Planck-Gesellschaft President

\textsuperscript{132} Havighurst, “Report on Germany,” 50.

\textsuperscript{133} Several other scholars have interpreted the reconstruction of the universities after World War II as a kind of restoration, though one must be very careful with this term, for it carries with it connotations of the Marxist critique of the restoration of Bourgeois society in general after the war. See Jarausch, “Humboldt Syndrome,” 35-38, and Osiertzk, \textit{Wissenschaftsorganisation und Restauration}, 368-377. Perhaps the greatest missed opportunity to reform German higher education in the postwar era was the failure of the “Berlin Research University” project. In 1946, Fritz Karsen, a former émigré who had returned to Berlin to work for the US occupation government, hoped to democratize German education by creating a new kind of research university in Berlin. He hoped to link university and non-university research institutes and install a progressive pedagogical approach modeled after the most advanced American universities. He met with opposition from both the universities and the former KWIs. Both resented the intrusion on their own professional competencies that the new university would represent. The researchers of the KWIs, including Otto Hahn, believed that it would restrict the freedom of research by coupling it too closely to society, and the universities believed that it would lower the social status of education. Both also feared losing money or resources to the new organization, and it is also fair to say that they resented the challenge to their elite social status implicit in its existence. The project failed. Maria Osiertzk, “Reform oder Modernisierung: Impulse zu neuartigen Organisationssstrukturen der Wissenschaft nach 1945,” in \textit{Exodus von Wissenschaften aus Berlin: Fragestellungen—Ergebnisse—Desiderate. Entwicklungen vor und nach 1933}, eds. Wolfram Fischer, Klaus Hierholzer, Michael Hubenstorf, Peter Th. Walther, and Rolf Winau (Berlin: Walter de Gruyter, 1994), 284-295, esp. 286-290. For the financial concerns of the KWIs in Berlin, see Heinemann, “Wiederaufbau,” 434-436.

\textsuperscript{134} Diary of Gerald R. Pomerat, 1949. RAC, Record Group 12.2, Diaries, Box 36, p. 156.
Otto Hahn repeatedly encouraged Butenandt to give up his joint appointment as director of a Max-Planck-Institut and Tübingen University professor.\footnote{Gerald R. Pomerat, Interview with Adolf Butenandt, 1 December 1953. “Diary of Gerald R. Pomerat, 1953.” RAC, Record Group 12.2, Diaries, Box 37, pp. 332-333.}

Since public money financed the Max-Planck-Gesellschaft, there was competition for resources between the MPG and the universities. The city and Land governments funded the universities directly, while money for the MPG was collected and distributed according to the Königstein Agreement. Both organizations had access to funds from the Deutsche Forschungsgemeinschaft, which also received the bulk of its money from the Königstein Agreement.\footnote{The Deutsche Forschungsgemeinschaft is the most important funding agency for scholarly research in the Federal Republic of Germany. It began as an emergency funding organization for German science in the twenties (Notgemeinschaft der deutschen Wissenschaft), assumed the name DFG during the Third Reich, and re-emerged as the Notgemeinschaft after the war. It was fused with another body, the German Research Council (Deutscher Forschungsrat) in 1951 to form the current DFG. It distributes its funds in the form of grants to individual researchers in all areas of research. It will be discussed in more detail in Chapter 6.} As noted above, local governments also pledged construction money to the MPG beyond what they were required in order to attract or retain institutes and researchers in their areas. The disdain of the universities for the practical arts meant that there was relatively little industry support for university research, while individuals such as Butenandt were able to receive additional private funding for their research in the Max-Planck-Gesellschaft.

Thus, the financial situation for modern biology research (as opposed to teaching) was much more promising in the Max-Planck-Gesellschaft than in the university setting after the war. Such was definitely the case in Tübingen. For example, Butenandt was able to use funds from private industry to purchase an electron microscope for his institute to make up for the microscope (owned by the university) that the French had dismantled. When Otto Stickl wrote to request funds for a new machine for the
university, he was turned down and told Butenandt would make his available to university researchers. While a generous offer, in reality the university could make little use of the machine as it was used regularly by the virus researchers. Because they sometimes worked on potentially dangerous viruses, the microscope often had to be quarantined after use, making it even less accessible.\footnote{Akademisches Rektoramt to Stickl, April 12, 1949; Stickl to Rektoramt, June 2, 1949. Both letters Stickl Records, UAT, 376/61.}

In 1953, Butenandt admitted that research funding for the two organizations was not equitable when he conceded that the lack of money and of new positions in the universities would encourage the best researchers to flock to the Max-Planck-Gesellschaft. He therefore thought that it was imperative for higher MPG staff members to maintain close relationships with the universities.\footnote{Gerald R. Pomerat, Interview with Adolf Butenandt, 1 December 1953, 332.} His concern was echoed more forcefully by Gerald Pomerat, the Rockefeller Foundation office most familiar with the German situation. In September, 1953 Pomerat wrote:

> On the other hand, the criticism is frequently made, and I think justifiably made, that the Max Planck Institutes are robbing the universities of their best men, are taking out of the universities first-class research men who might also have played a significant role in the inspiration and guidance of graduate students, and are capturing a disproportionately large fraction of funds that might be available for the support of research within the university system.\footnote{G.R. Pomerat to T.M. Sonneborn, September 22, 1953 (excerpt). RAC, Record Group 2, Series 717, Box 44, Folder 289.}

### Conclusion

As we shall see, the example of virology/molecular biology research in the Federal Republic of Germany both supports and contradicts Pomerat’s assessment. On the one hand, young researchers in the Max-Planck-Gesellschaft did receive generous financial and material assistance from the state, enabling them to carry out first rate
research. They did much less teaching than did staff members of universities, and therefore one can make a plausible argument that their funds could have been more productively spent on the universities. However, the conservative nature of the German universities made them incompatible with the kind of research the Tübingen researchers hoped to accomplish. Biological fields were drawn along nineteenth century lines, and disciplinary boundaries were strictly enforced. Resources aside, this structure made it difficult for young researchers to carry out sophisticated, interdisciplinary, biology research in the average German university institute. In the first years after the war, German biological researchers had a choice of innovative research or old fashioned teaching. The Tübingen group chose the former. To combine both would have required a thoroughgoing institutional reform that was not financially possible, shaped by a comprehensive vision of science in the service of the state that was not politically possible. Therefore, it was not a case of the Max-Planck-Gesellschaft profiting at the expense of the universities, but instead a compromise solution in which members of both organizations pursued their own self-interests.
Chapter 4:
From Virus Research to Molecular Biology, 1945-1956

Introduction

The dedication to professional values and "pure" science was characteristic of the twentieth century scientific community as a whole, not just the researchers in Nazi and postwar Germany. The elitism and isolation entailed in such a professional identity insulated the scientific community to an extent, and in doing so has perhaps contributed to the tendency to grant scientists the transcendent, objective status we give to the knowledge that they produce. It is highly likely that for many people the natural world itself is more accessible and understandable than is the professional community that studies it. The specialized language, knowledge, culture, and customs of scientists undoubtedly appear quite foreign to many. The combination of partial isolation from general society and shared values among researchers has made modern science uniquely cosmopolitan. Scientists in a lab in one country often have an easier time understanding their colleagues in a foreign lab than they do their non-scientific colleagues in their own country. Thus the virulent nationalism of Nazi Germany threatened the values that were at the very core of the international scientific community. For German investigators working in the postwar years, rebuilding links to this community was as important as rebuilding labs and institutes in Germany itself.

Perhaps one of the greatest ironies of science as a social activity is the way that human characteristics such as bias, self-interest, and competitiveness have, over the
centuries, produced meaningful knowledge about the natural world. These characteristics have served to balance the opposing tensions of cooperation and competition in the scientific community. Rewarding competition gives individuals an incentive to succeed, if need be at the expense of their colleagues, while this entire process is governed by the need to cooperate in order to build a case persuasive enough to be accepted as scientific knowledge by the majority of the community. The experience of the German TMV researchers during the war and immediately afterward demonstrates importance of such a community for successful research. Isolation from foreign colleagues excluded the TMV researchers from many exciting new developments, but more importantly, isolation weakened the self-correcting mechanism of science by preventing the double-checking of the German research by colleagues abroad.

To become completely accepted by their peers abroad, the German researchers had to prove that their work was of high quality and of general significance. In the immediate postwar era, their research seemed to be of interest only to those in the specialized field of plant virology. They were aware of developments outside of Germany, however, and by the mid-1950s began to suspect that nucleic acid and not protein was the genetic material. In 1956 they were able to realize their long-term goal of making contributions to general biological research by using TMV to provide decisive proof that helped solidify this new scientific paradigm. When TMV became an effective model system for studying genes, the German researchers became a more important part of the international scientific community.¹

¹ The most extensive discussion of TMV as a model system is Angela N. H. Creager's *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930-1965* (Chicago: University of Chicago Press, 2002). Creager defines the TMV experimental model as more than just the virus itself—it also consisted of the techniques, ideas, and strategies associated with TMV research. She also argues that systems like TMV are models in two senses (4-8). In the first sense, they are models because the results generated from them can be generalized to other systems. In the second sense, they are models because they show by example how other experimental objects can be studied. Due to my focus I shall use the term model system/model organism in only the first sense.
4.1 The Nature of the Scientific Community

4.1.1 Competition and Cooperation

In the last decades of the twentieth century, many analysts tried to explain the system that produced the spectacular scientific achievements they witnessed. They noted that the scientific community was a particular subculture with its own sets of norms, values, and practices. Scientists often emphasized the division between the scientific community and society as a whole to confer authority on their work, but as noted above, the scientific process is essentially human and therefore social.\(^2\) Owing to their universality, scientific ideas transcend cultural, linguistic, and national boundaries, making scientific communities uniquely international.\(^3\) Yet the people who produce these ideas are located in specific national contexts. Since modern science emerged contemporaneously with the nation-state, the national context of individual researchers has often conflicted with the internationality of their collective identity. This conflict affected the scientific community in Germany during the First World War and happened yet again in the Nazi period.\(^4\) Then, the racial policies of Nazi Germany seriously tested the internationality of the biological research community by placing the German researchers in a position such that membership in their national community violated the

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\(^3\) Alfred Gierer, *Im Spiegel der Natur erkennen wir uns Selbst* (Hamburg: Rohwalt, 1998). Gierer differentiates between the cultural process of science and the knowledge it produces regarding the natural world. On 237 he writes, “Culturally specific is the creation of science, as well as its metatheoretical meaning, but not the knowledge itself. That is accessible to whoever makes the effort to learn it—it is in this sense the property of humankind.”

standards, not only of the international community of science, but of human decency. The expulsion of German-Jewish scientists on irrational grounds and the aggressive war waged by Nazi Germany temporarily removed practicing German scientists from the stimulation and oversight of the broader community. Unable to share in the productive tension of cooperation and competition with their colleagues, their research at home as well as their reputations abroad suffered.

Competition can serve as a motivating factor in scientific research, providing an element of urgency and intensity that might not exist otherwise. This is because one of the most important criteria for recognition in science is priority—publishing first is infinitely better than publishing second, and solo credit is better than shared. Individuals thus have a strong motivation to succeed at the expense of their peers. Some scientists argue that the competitive element in science has been over-emphasized, but most will admit that it exists to a considerable extent. For example, Richard Lewontin, a molecular biologist, wrote in 1968:

What every scientist knows, but few will admit, is that the requirement for great success is great ambition. Moreover, the ambition is for personal triumph over other men, not merely over nature. Science is a form of competitive and aggressive activity, a contest of man against man that provides knowledge only as a side product. That side product is its only advantage over football.\footnote{Lewontin wrote this as part of a review of James Watson’s \textit{The Double Helix: A Personal Account of the Discovery of DNA}. Watson’s book covered his own biases and competitive urges every bit as much as it did his Nobel-winning work, and Lewontin argued that this was a very honest presentation of how scientists view their work, although certainly many practicing scientists would disagree. Richard C. Lewontin, “‘Honest Jim’ Watson’s Big Thriller About DNA.” Review of James Watson’s \textit{The Double Helix}. \textit{Chicago Sunday Times} 25 February 1968, pp. 1-2. Molecular biologist Robert Sinsheimer is reluctant to accord competition so much credit; in the case of the discovery of the structure of DNA, he points out that Linus Pauling was not even aware that he was “competing” with Watson and Crick. Robert L. Sinsheimer, \textit{The Strands of a Life: The Science of DNA and the Art of Education}, (Berkeley and Los Angeles: University of California Press, 1994), 90.}

According to Lewontin, excessive competition threatens to make science wasteful and destructive, and indeed this would be the case if it were not constantly checked by the need for cooperation.
Cooperation is essential because of the social nature of science. Scientists must balance their own original work with the need to cite others for reasons of prestige and support. Citation is a professional necessity because citing the work of a colleague in a scientific paper demonstrates respect (provided, of course, that the citation is favorable) and is one way of strengthening professional allegiances. Citation also lends intellectual credibility to a piece of work by contextualizing it within a broader framework of previously existing research. As understood by Robert K. Merton, citation is an essential part of a system that safeguards the intellectual property of individual scientists:

This system of open publication that makes for the advancement of scientific knowledge requires normatively guided reciprocities. It can operate effectively only if the practice of making one's work communally accessible is supported by the correlative practice in which scientists who make use of that work acknowledge having done so. In effect, they thus reaffirm the property rights of the scientists to whom they are then and there indebted. This amounts to a pattern of legitimate appropriation as opposed to the pattern of illegitimate expropriation (plagiarism).
We thus begin to see that the institutionalized practice of citations and references in the sphere of learning is not a trivial matter.

David Hull has argued that this tension between competition and cooperation, despite their apparent contradiction, is the fundamental relationship that allows for scientific change. Scientists' desire to receive credit for their individual contributions and the necessity of checking those contributions against those of other researchers shape their quest to understand the natural world. Competition provides at least partial

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motivation for the individual contributor, and also serves as a community safeguard, since any scientific claim may be challenged by one or more competitors. Cooperation is necessary to provide the groundwork for new research and to safeguard this research from criticism. Individual knowledge becomes the property of the community through the process of publication in which credit to others is exchanged for their support for the author’s individual contributions.

The need to cite (and be cited by) others brings individual ideas into the broader community, but also has another vital effect. It provides a check on what constitutes scientific knowledge. Individual scientists cannot make a claim to objectivity, but they do not need to; the community as a whole has the final say. Theories are accepted or rejected according to the collective assessment of scientists with similar training applying similar criteria.9 From such an interpretation it does not follow that the content of scientific knowledge is socially determined, but rather that the community of scholars determines when speculation about the natural world has sufficient evidence to be accepted as scientific fact.10 Scientific knowledge, therefore, does not equal the natural world but is a human effort to make sense of that world.11

The social nature of the production of knowledge increases the likelihood of transcending individual biases. “One of the strengths of science is that it does not require

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9 Thomas Kuhn discussed this as part of the process of “normal science;” he argued that the articulation of a new paradigmatic interpretation of natural phenomena leads to the conversion of like-minded individuals and eventually the creation of an entire disciplinary apparatus. Whether or not one accepts his interpretation of scientific change, the public nature of science, particularly the importance of citation in publication, clearly show that what is accepted as science at a given time is the consensus view of certified experts in the field. Thomas S. Kuhn, *The Structure of Scientific Revolutions*. Second, Enlarged Edition. (Chicago: University of Chicago Press, 1970). See Chapter II, “The Route to Normal Science,” esp. 18-19.


that scientists be unbiased,” says Hull, “only that different scientists have different 
bases.” 12 There is no guarantee that what scientists as a whole accept at a given time is 
in fact the “truth;” communities can be mistaken, and the same biases can be shared by a 
majority of members of a given community. For this process to function efficiently it is 
important that the open exchange of ideas between researchers be allowed, and the larger 
the community, the larger the number of mutually canceling biases and the greater the 
likelihood that someone will recognize and correct mistaken ideas. The more closed the 
environment and the smaller the community, the greater the likelihood for the 
perpetuation of error.

The international community of virus researchers in the late 1930s exemplified 
the way a scientific community could function along these lines. Stanley’s work, though 
initially incorrect in some respects, was studied, repeated, and corrected by his colleagues 
abroad. He provided the inspiration for the German researchers and helped provide the 
materials necessary for them to begin their work. Once established, the groups 
corresponded, read and cited one another’s papers, and corrected one another when 
necessary, demonstrating the uniquely productive tension of cooperation and competition 
noted by Hull. This community was severely disrupted by the isolation of wartime 
Germany and the TMV research suffered significantly as a result.

4.1.2 Racist Science?

The expulsion of Jewish scientists limited but did not prevent the remaining 
German researchers from working productively with their colleagues abroad. It caused 
harm to the German scientific community as a whole since fully one fourth of the 
country’s talented biochemists left. Since TMV research was so new, however, there 
were no established virologists working on this topic who were forced to flee Germany,

12 Hull, Science as a Process, 305, 310-311, 322.
and so the Dahlem sub-community was not hindered in this respect. Relations with the United States remained surprisingly strong; although the Rockefeller Foundation had curtailed funding to many groups, it continued to support Butenandt and Kühn, and it was in 1935 that Butenandt received the offer to join the Harvard faculty. The racism of German policies had not yet discredited these individuals in the eyes of their peers.

Furthermore, there is no reason to think that the acceptance of the racial laws of Nazi Germany compromised the TMV research on a theoretical level. Of course believing in Nazi “scientific” racism in the absence of proof would be evidence of the corruption of the scientific mindset by demonstrating the importance of ideology over empiricism. However, as we have seen, the TMV researchers did not preach the Nazi dogma and were not direct participants in the crimes of the regime, but rather quietly tolerated a climate of extreme racism as they fulfilled their own personal and professional goals. Nazi Germany was not the first place where scientific research was performed in a climate of extreme racism. In fact discrimination has been, and still is, endemic in the scientific process, an obvious example being the systematic exclusion of women from equal participation in scientific circles until the postwar era.\(^{13}\)

In the 1930s, racism was legal and institutionalized in the United States, and yet many researchers there successfully advanced scientific knowledge. For example, black physicians were excluded from membership in the American Medical Association and

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black students from most American institutions of higher learning. Germans publicly noted the hypocrisy of American criticism of German racism, as demonstrated by an article from the *Berliner Börsenzeitung*, dated February 22, 1939, which stated, "The Negro would well be surprised that the white American becomes outraged at the elimination of Jews from German universities, while they do not even consider the exclusion of Negroes from many American universities." In addition, the presence of a strong eugenics movement in America meant that a few members of the scientific and medical communities publicly endorsed German racial laws.

Acknowledging that institutionalized racism was prevalent in the international scientific community in the 1930s is not meant to serve as a condemnation of science as a whole. Nor is it meant as an attempt to rationalize Nazi barbarism or to equate racism with genocide. Instead, it is meant to demonstrate that irrational beliefs such as chauvinism, racism, and anti-Semitism can be quite easily reconciled with excellent science as it has been practiced over the last several centuries. People are, and were, complicated and often inconsistent. One need not be a good, unprejudiced person to do good science, and in fact the opposite is very often the case. Hull concludes the introduction to his work with the following: "Although objective knowledge through bias and commitment sounds as paradoxical as bombs for peace, I argue that the existence and ultimate rationality of science can be explained in terms of bias, jealousy, and irrationality." Therefore acquiescence and tacit support of Nazi racial policies did not

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necessarily imply an ideological commitment on the part of the German TMV investigators that would prevent the practice of first rate science.

The expulsion of Jewish scientists hurt the German community not because of its immorality, but for the practical reason that it removed talented researchers and put them in the service of Germany’s foes. Owing to the circumstances of their expulsion, it was difficult for many Jewish émigrés to accept their colleagues who had stayed behind and remained silent, or had even profited from the loss of the Jews. This kind of behavior was not part of normal practice because it was directed against accepted and accredited members of the scientific community. Typically racism or chauvinism served the purpose of safeguarding the frontiers of the community from outside groups. Once a group had made it past these social boundaries, as the Jews had, normal practice was to treat them as equals.

Nazi Germany, however, was far from normal, and what happened there was that the phenomenon of common biases, which typically existed to define and safeguard the scientific community, were turned inward and used to divide the community. This blatant violation of accepted standards of professional conduct did give many émigrés reason to question whether German researchers would allow other standards, such as those of research itself, to slide. Consequently, in the postwar era there was a significant cadre in the scientific community who would find it very difficult to normalize relations with the German researchers, either personally or professionally.

4.2 War and the Scientific Community

4.2.1 Isolation

The radicalization of Nazi Germany caused an increasing isolation of the German research community that hampered its stimulating and self-correcting relationship with foreign scientists. Already in September, 1939, the German assault on Poland began to strain the good relationship between the Dahlem researchers and their colleagues in
America. In 1939 one of Butenandt’s students, Ulrich Westphal, was in the United States on a Rockefeller fellowship. During the summer he was at Columbia University learning physiology, but both he and Butenandt hoped that he would be able to spend some time in Stanley’s lab.\(^\text{17}\) Stanley agreed, and in August he and Westphal began to arrange for Westphal to make an extended stay at Stanley’s lab prior to his scheduled return to Germany in December.\(^\text{18}\) The war ended Westphal’s American visit prematurely, however, and in October Westphal wrote to Stanley, thanking him for his kindness, but expressing regret that his continued stay was now impossible owing to the situation in Europe.\(^\text{19}\)

Despite limitations such as these, the German TMV researchers were still able to receive the American scientific literature and to correspond with people like Stanley until the commencement of hostilities between the United States and Germany. Schramm and Stanley corresponded regarding their TMV research until October, 1941. At that time Stanley wrote to Schramm to tell him that he was having difficulty duplicating some of Schramm’s published results and requested that Schramm send him some experimental materials. The German declaration of war on America in the aftermath of the Japanese bombing of Pearl Harbor effectively ended this direct scientific exchange before Schramm could reply.\(^\text{20}\) It also limited the Germans’ access to English language scientific literature. They were not completely cut off, as analysis of the citations in their

\(^{17}\) Ulrich Westphal to Wendell Stanley, August 1, 1939, Stanley Papers, Carton 13, Folder 41; Adolf Butenandt to Stanley, August 22, 1939, Stanley Papers, Carton 6, Folder 199.

\(^{18}\) Westphal to Stanley, August 29, 1939. Stanley Papers, Carton 13, Folder 41.

\(^{19}\) Westphal to Stanley, October 10, 1939. Stanley Papers, Carton 13, Folder 41.

\(^{20}\) Schramm to Stanley, October 13, 1941. Stanley Papers, Carton 12, Folder 24. Although this is the only letter in Stanley’s prewar correspondence with Schramm, it is unlikely that this was the only time that the two corresponded. In any case, the letter shows clearly that detailed scientific exchange was taking place between the US and Germany more than two years after the German attack on Poland.
papers published during the war shows that they had access to some journals until 1942, and in some cases, such as the journal *Nature*, they had access to articles as late as 1944. It is likely that the Germans were able to receive this literature thanks to help from colleagues in neutral countries such as Sweden and Switzerland. Scientists in both countries remained in contact with the Dahlem researchers throughout the war.²¹

This professional isolation, though incomplete, compounded the more obvious difficulties the TMV researchers were facing (bombing and evacuation) by limiting the creative and self-correcting mechanisms of a properly functioning scientific community. For example, on February 19, 1941, Schramm submitted a brief article to the *Berichte der deutsche chemische Gesellschaft*. He stated that experiments by Caspersson and ideas articulated by Freksa in his 1940 article had convinced him that nucleic acid was important in viral replication. Since Freksa’s piece suggested that the nucleic acid served as a kind of template on the surface of the virus, understanding how the two were bound together seemed to be a clue for understanding the process of viral replication. Guided by the belief that the nucleic acid was on the surface of the viral particle, Schramm apparently thought that the separation of the two would not be difficult, and he reported that he had successfully isolated viral protein free of its nucleic acid component by splitting the virus particle enzymatically.²²

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Wendell Stanley immediately tried to replicate the experiment and was unsuccessful. It was for this reason that Stanley had written to Schramm in October, 1941, asking for experimental materials in order to clarify the situation. Stanley was unable to reproduce Schramm's results for the simple reason that Schramm had not obtained nucleic acid-free protein after all. It seems as though some time passed before Schramm realized his error. He never retracted the article, but he did correct himself in print in 1944. By that time, however, the other Dahlem researchers had already cited the article favorably.\(^\text{23}\)

Schramm was the only researcher in the group investigating the biochemical aspects of the virus research. He had numerous co-workers, but there was no independent lab to double-check his work once the war severed his relations with Stanley. In addition, most of his papers (and those of Melchers and Freksa as well) were published in journals that were edited by Butenandt, Kühn, and Wettstein.\(^\text{24}\) Butenandt had trained Schramm, and the two were close friends as well as scientific colleagues, making it unlikely that Butenandt doubted the accuracy of his protégé's work. After all, although a relative newcomer, at that point Schramm was the expert on biochemical virus research in Dahlem, not Butenandt. Thus the wider self-correcting process of the

\(^{23}\) Gerhard Schramm, "Über die Konstitution des Tabakmosaikvirus," *Die Chemie (Angewandte Chemie Neue Folge)* 57 (December, 1944): 109-113. On page 111 Schramm wrote, "Other, as of yet unpublished experiments have shown that in spite of the original opinion, the nucleic acid remains bound to the split particles even after hydrolysis." The article was taken from a lecture given in May of 1943, so it is possible that Schramm corrected himself in public at that point, but since many changes have been made to the text, most notably to incorporate literature from 1944, it is safest to say that the correction was officially made in December, 1944. The original, incorrect article was favorably cited in A. Butenandt, St. Hartwig, and H. Friedrich-Freksa, "Beitrag zur Feinstruktur des Tabakmosaikvirus," *Hoppe-Seyler's Zeitschrift für physiologische Chemie* 274 (1942). Reprinted in Adolf Butenandt, *Werk eines Lebens* (Göttingen: Vandenhoeck & Ruprecht, 1981), 916-25, as well as later articles.

\(^{24}\) Rheinberger, *Virusforschung*, p. 692 and note 111.
scientific community no longer functioned. Furthermore, peer review, ordinarily a means of assuring the quality of published scientific work, had also ceased to be a factor.²⁵

Had normal scientific relations with Stanley in America and with Bawden and Pirie in England been maintained, the Germans would assuredly have been informed of their mistake by early 1942, when Stanley was unable to duplicate Schramm's results.²⁶ Instead, the Dahlem researchers became an isolated and inward-looking group. Their success amidst relative isolation is a credit to their talents as researchers and the importance of interdisciplinary work even in a small community, but the perpetuation of Schramm’s misperception is an even more powerful testimony to the need for an open community of researchers capable of double checking one another’s results.²⁷

4.2.2 Rejoining the Scientific Community

As we have seen, the material situation in Tübingen was as good as one could possibly expect in postwar Germany, which meant that it was sufficient but certainly not ideal. Relations with the scientific community were similar. The TMV group was not entirely isolated from foreign scientists, but contact depended upon the personal relationships the German scientists and their foreign colleagues had developed prior to the war. It also depended upon the willingness of individual Germans to examine critically their own part in the National Socialist regime.

²⁵ For the importance of peer review in scientific work, see Hull, Science as a Process, Chapter 9 “Secrecy and Bias in Science,” and Ziman, Public Knowledge, 102-126.


²⁷ As we saw in Chapter 2, Schramm’s most significant wartime work was his 1943 paper on the splitting and re-aggregation of the TMV particle. By that time, researchers in the other major TMV labs had become preoccupied with war-related research, and paid little heed to Schramm’s TMV work until well after the war. James Watson noted that there was a prejudice against Schramm’s research in the early postwar years—see the discussion in the Epilogue to this study. This demonstrates that the negative effects of isolation did go both ways—the ability of the Germans to carry out excellent research was limited, but the diffusion of their research to other groups was limited as well, compromising TMV research as a whole.
Foreign contact with the German researchers after the war therefore varied widely. On the one extreme, relations with French scientists were almost non-existent. Leading French biologists such as Jacques Monod, André Lwoff, and François Jacob had all fought the Germans during the war; two prominent bacteriologists from the Institut Pasteur, Eugène and Elizabeth Wollman, both Jews, were deported and killed in a Nazi extermination camp.\textsuperscript{28} Collectively, scientists from the Institut Pasteur distanced themselves from the German research community until the mid-1950s.\textsuperscript{29} The French did cultivate close ties with the United States, however, and by the late 1940s, when biologists in America began to make significant breakthroughs using viruses called bacteriophages, French researchers were very closely involved.\textsuperscript{30}

On the other extreme, Wendell Stanley had much in common with the German researchers apart from their shared research interests. In the late 1920s, Stanley spent two years in Munich studying under Heinrich Wieland; Schramm also studied under Wieland at the time, although it is unclear if he and Stanley met then. Stanley read German, making correspondence easier, and he remembered his time in Munich fondly. Whenever he visited Munich in the postwar era he insisted on visiting Wieland’s old house to pay his respects to his former mentor.\textsuperscript{31} Correspondence between Schramm and Stanley resumed in 1946, after which a steady stream of re-prints and pre-prints of articles flowed between Tübingen and Stanley’s new lab in Berkeley. This assistance

\begin{footnotesize}
\begin{enumerate}
\item Ute Deichmann, \textit{Biologists under Hitler}, trans. Thomas Dunlap (Cambridge MA: Harvard University Press, 1993), 311-312. For example, an important international colloquium on bacteriophage was sponsored by the Pasteur Institute and held at the Royaumont Chateau in 1952; only two Germans, Carsten Bresch and Wolfhard Weidel, both of whom had Delbrück’s seal of approval, were invited to attend. Interestingly, both presented in English, not German. See \textit{Annales de l’Institut Pasteur} 84 (1953).
\item Interview with Klaus Munk, Heidelberg, July 20, 2000.
\end{enumerate}
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was enormously beneficial since the German researchers had trouble accessing English language scientific literature for some time after the war. Thanking Stanley for a batch of reprints in July 1947, Gernot Bergold wrote that the German researchers were still "working under damnd [sic] poor conditions," and closed by saying "It is too bad that we still don't get the American literature."\textsuperscript{32}

Years later, Georg Melchers stated that Schramm's 1941 paper had convinced Stanley he was a sloppy scientist and not to be trusted.\textsuperscript{33} Melchers thought, "Because he did not explicitly correct his mistake to Stanley, Stanley and his colleagues continued to mistrust Schramm's results on many occasions."\textsuperscript{34} But Schramm did make a public correction in 1944 (although he did not retract the paper), and in a letter dated August 2, 1946, admitted his error to Stanley and gave him the citation of the article in which the correction appeared. In return, Stanley thanked him and expressed pleasure that the two were in agreement.\textsuperscript{35} Stanley did indeed question Schramm's results whenever the two disagreed, but he eventually did develop respect for him, which Stanley expressed in a letter to several of his colleagues in 1956:

Dr. Gerhard Schramm of the Max-Plank-Institut fur Virusforschung of Tübingen, Germany, is coming to the United States this spring to present his work on proteins and nucleoproteins to the Gordon Conference. He is one of the outstanding German scientists in this field, is actively pursuing investigative

\textsuperscript{32} Bergold to Stanley, July 14, 1947. Stanley Papers, Carton 6, Folder 117. In 1947, Alfred Kühn also complained that he was still not receiving any literature directly from foreign countries. Robert J. Havighurst, "Germany—Diary and Interviews, 1947," Interview with Alfred Kühn, October 6. RAC, Record Group 1.1, Series 717, Box 3, Folder 19.

\textsuperscript{33} Cited in Deichmann, \textit{Biologists under Hitler}, 313-314.

\textsuperscript{34} Ibid., 314.

\textsuperscript{35} Schramm to Stanley, August 2, 1946; Stanley to Schramm, October 9, 1946. Both letters Stanley Papers, Carton 12, Folder 24. Schramm later repeated this correction and went so far as to cite the paper by Stanley and Cohen that proved his own work had been incorrect. Schramm, "Die Struktur des Tabakmosaikvirus und seiner Mutanten," \textit{Advances in Enzymology} 15 (1954): 461. Also see Rheinberger, "Virusforschung," 688.
work, and speaks English quite well... I heard him lecture at the Third International Congress of Biochemistry in Brussels and he has some exceedingly interesting material which he presents quite well.\footnote{Stanley wrote this letter on January 20, 1956, as an effort to arrange a number of speaking engagements for Schramm during his visit to America. Stanley was quite successful, and colleagues from Caltech, UCLA, Stanford, and several other schools invited Schramm to speak and pledged a small sum of money to help cover his travel expenses. Stanley papers, Carton 12, Folder 24.}

Participation in international scientific conferences was quite limited for the German researchers after the war. The first important postwar meeting on proteins and nucleic acids was held in England as a symposium of the Society for Experimental Biology. None of the German researchers was invited to present material, despite his research in the field. In 1948, the 8th International Congress of Genetics was held in Stockholm. Several Germans were invited to attend, provided they had not openly supported the Nazi Party. This group of seven included three representatives from Tübingen: Melchers, Freksa, and Anton Lang. The president of the Society, Hermann Muller, made it clear that he did not hold all German scientists responsible for the crimes of the Nazis, but he did express misgivings about the level of influence that former Nazis were beginning to have in the universities in the Western Zones of occupation.\footnote{See Chapter 3, section 3.3.3.}

In 1949, the First International Congress of Biochemistry was held in Edinburgh; in this truly massive meeting, no Germans were invited as participants. However, Heinz Fraenkel-Conrat, an émigré then on a Rockefeller Fellowship, said that he met several German researchers at the conference, and so again their isolation was not complete.\footnote{Undated, unpublished autobiography of Heinz Fraenkel-Conrat, titled “The Lives and Achievements of a Family of Natural Scientists over the past 150 year period,” page 52. Heinz Fraenkel-Conrat Papers, Bancroft Library, University of California, Berkeley, Call # 2000/79z, Carton 2. A program for the First International Congress of Biochemistry is in carton 3 of the collection. It lists hundreds of members of which not one German is named. According to Ute Deichmann, Hans Krebs, an émigré then working in Britain, supported the admission of German participants to this conference, but was unsuccessful. Deichmann, “The Expulsion of German-Jewish Chemists and Biochemists and their Correspondence with Colleagues in Germany after 1945: The Impossibility of Normalization?” in Science in the Third Reich, ed. Margit Szöllösi-Janze (Oxford: Berg Publishers, 2001), 269.} Assessing
the situation in 1949, Melchers said, “Our contact with foreign colleagues is happily quite good.” Melchers mentioned that he had been to three major meetings and that the Tübingen institutes had received a number of visitors over the past few years.\(^{39}\) Here one must keep Melchers’ baseline for comparison in mind. Given the lost war, occupation, and the precedent of the scientific ostracism of Germany after World War I, even the limited contact that the Tübingen researchers had in the late 1940s was for him surprising and welcome.\(^{40}\)

4.2.3 Relations with German Émigrés

Since many of the talented scientists driven from Germany by the Nazi regime had achieved great success by the late 1940s, their relationships with their colleagues who had remained in Germany became an important part of the integration of German scientists into the international scientific community. The overall unwillingness of German scientists to confront their past or simply to admit wrongdoing hampered reconciliation from the beginning. Ute Deichmann has shown that within the biochemistry community, the refusal of German scientists to accept responsibility for the past prevented the widespread normalization of relations between the two groups.\(^{41}\) In their self-pity, the Germans failed to appreciate both the suffering of their exiled colleagues and the extent to which international opinion condemned those who had openly cooperated with Hitler.\(^{42}\) The Germans did not apologize on an individual level,

\(^{39}\) Melchers to Dr. K. Mothes, September 30, 1949. Melchers Papers, Ordner 1.

\(^{40}\) In general, the isolation of the German scientific community after World War II was not as pronounced as it had been after the First World War. See Thomas Stamm-Kuhlmann, “Deutsche Forschung und internationale Integration 1945-1955,” in Forschung im Spannungsfeld von Politik und Gesellschaft: Geschichte und Struktur der Kaiser-Wilhelm- Max-Planck-Gesellschaft, eds. Rudolf Vierhaus and Bernard vom Brocke (Stuttgart: Deutsche Verlags-Anstalt, 1990), 886-909.

\(^{41}\) Deichmann, “The Expulsion of German-Jewish Chemists,” 270-271.

\(^{42}\) This is most poignantly illustrated by the postwar relationship between Lise Meitner and her former co-worker Otto Hahn. It is discussed by Ruth Lewin Sime in Lise Meitner: A Life in Science (Berkeley: University of California Press, 1996) Chapters 13-15.
and at the institutional level, no effort was made to compensate émigrés for the losses they suffered as a result of their expulsion.\textsuperscript{43} Nevertheless, the response of the émigré community was varied and surprisingly positive, with the type of response depending upon prior personal relations as well as common scientific interests.

Butenandt’s relationship with German-Jewish émigrés was very strained after the war, but he was able to resume positive relations with Carl Neuberg, his predecessor as director of the Kaiser-Wilhelm-Institut für Biochemie. In general, there is no evidence that Butenandt demonstrated any kind of anti-Semitic behavior during the 1930s, and he actually tried to help Neuberg, a dismissed Jewish scientist, in a financial dispute with the Kaiser-Wilhelm-Gesellschaft.\textsuperscript{44} As a condition of his contract with the KWG, concluded in 1913, Neuberg had pledged 30 per cent of his supplemental income to the society to cover costs of research for other parties carried out within his institute. In 1937, I.G. Farben gave him RM 50,000 for the transfer of a patent, of which he was obligated to give 30 per cent (RM 15,000) to the KWG. After his unjustified dismissal, Neuberg was understandably reluctant to do so, arguing that the research in question had in fact been paid for by the chemical industry, thereby nullifying the conditions of his contract. He hoped to use the funds to finance his own independent research. Butenandt wrote to the society’s general director Friedrich Glum to suggest that the KWG refuse the money.\textsuperscript{45} Glum responded that the KWG was bound by the terms of its contract to accept the money, but in recognition of Neuberg’s long service he promised to bring the question

\textsuperscript{43} Michael Schüring, whose research is part of the Presidential Commission to study the history of the Kaiser-Wilhelm-Gesellschaft in National Socialism, is one of the first scholars to discuss the lack of compensation of the Max-Planck-Gesellschaft to émigrés. He has presented his preliminary findings in “Ein Dilemma der Kontinuität. Das Selbstverständnis der Max-Planck-Gesellschaft und der Umgang mit Emigranten in den 50er Jahren.” Unpublished manuscript. I would like to thank Mr. Schüring for sharing his findings with me.

\textsuperscript{44} Proctor, “Adolf Butenandt,” 12-15.

\textsuperscript{45} Butenandt to Glum, April 3, 1937. MPG Archive I Abt., Rep. 1A, Nr. 2041/2.
before the next meeting of the society’s Senate. The Senate concluded that the society and its members must fulfill the terms of their contracts and recommended that Butenandt accept the money.\textsuperscript{46}

Neuberg emigrated to America in 1938, bitter about his treatment. After the war, he and Butenandt resumed their correspondence, and Neuberg was of great assistance in helping Butenandt re-establish ties with the American community. In return, Butenandt helped Neuberg receive his pension from the Max-Planck-Gesellschaft and in 1954 suggested that he be awarded the highest award the Federal Republic could bestow, the Grosses Verdienstkreuz.\textsuperscript{47}

This act of reconciliation must be balanced with numerous others in which Butenandt demonstrated self-pity and insensitivity bordering on the callous. In a 1946 letter to a former colleague who had emigrated to England in 1935, Butenandt tried to elicit sympathy for the fate of the German people. It seemed not to have occurred to him that others had suffered as well; his addressee had married a Jew who had lost 26 family members in the Holocaust, a fact that was known to him.\textsuperscript{48} Despite his generosity toward Neuberg in the 1930s, Butenandt’s postwar reconciliation undoubtedly stemmed as much from a desire to rehabilitate his own reputation as it did from the desire to correct an injustice.\textsuperscript{49}

\textsuperscript{46} Glum to Butenandt, April 22, 1937. MPG Archive I Abt., Rep. 1 A, Nr. 2041/2; “Niederschrift über die Sitzung des Senats der Kaiser-Wilhelm-Gesellschaft zur Förderung der Wissenschaften am 21 Juni 1937,” pp. 4-5. It is hardly surprising that Glum was unsuccessful in pleading this case before the Senate; he had previously had difficulties with the National Socialists, and in the middle of 1937 was dismissed as general director in favor of Ernst Telschow. See Kristie Macrakis, \textit{Surviving the Swastika: Scientific Research in Nazi Germany} (Oxford: Oxford University Press, 1993) 100-102.

\textsuperscript{47} Deichmann, “Expulsion,” 262-264. Also see Proctor, “Adolf Butenandt,” 31-33.

\textsuperscript{48} This exchange is discussed in Proctor, “Adolf Butenandt,” 6-7.

\textsuperscript{49} Interestingly, Butenandt is one of the very few German scientists who referred to the expulsion of Jewish scientists as an injustice; this was in regard to the case of James Franck. Deichmann, “Expulsion,” 275 note 36.
Fortunately there were many émigrés who were willing to help their German colleagues after the war. Their help was necessary because they played such an important part in the emergence of molecular biology in America and England after the war. Their impact was disproportionately greater than their numbers, which of course were significant enough. This situation can be explained, at least partially, by the high caliber of the émigré scientists as well as the role of the Rockefeller Foundation in supporting their research, encouraging interdisciplinary work, and stressing the importance of biology. The historian Klaus Fischer suggests that social marginalization may have played a part as well. He recognizes a synergy or feedback between displaced scholars struggling to find a place for themselves in a new land and the new discipline of molecular biology, an innovative field struggling to establish its own space of authority relative to older disciplines.\(^{50}\) The displaced scholars were thus drawn to the new discipline, and by serving as a nucleus and tapping into America’s other well-established scientific communities, they were able to create a critical mass of intellect that transformed biology in the first decades after World War II.

Of these émigré scholars, none was more important than Max Delbrück.\(^{51}\) Born in 1906, Delbrück’s education was that of a physicist. From 1932 to 1937 he had worked in Lise Meitner’s section of the Kaiser-Wilhelm-Institut für Chemie in Dahlem, where he became interested in applying physics to biology. His most significant work of this period came in 1935, when he co-published a theoretical paper with Karl Zimmer and


\(^{51}\) The only English language biography of Delbrück is Ernst Peter Fischer and Carol Lipson, Thinking About Science: Max Delbrück and the Origins of Molecular Biology (New York: W.W. Norton and Company, 1987).
Nicolai Timoffeef-Ressovsky on the molecular basis of gene mutation. In 1937, he was awarded a Rockefeller Foundation fellowship to travel to California to learn genetics. At the time he was not planning on emigrating permanently, but he was concerned enough about developments in Germany that he was grateful for the chance to step back and observe them from a distance. When Germany attacked Poland in 1939, Delbrück chose to stay in America, marry his fiancé, and pursue his scientific career there rather than return to Berlin and face the possibility of military service.

In the 1940s, Delbrück began to have a profound impact on biology in the United States. Along with Salvador Luria (also an émigré) and Alfred Hershey, he pioneered the use of bacteria-eating viruses (called bacteriophages, or phage, for short) as model systems for genetic studies. Because of their simplicity, short growth time, and distinct phenotypic characteristics, phage became the ideal tool for extremely high-resolution genetic mapping. The combination of simplicity and experimental utility led Delbrück to suggest that phage would be as useful and important to the study of biology as the hydrogen atom was to the study of physics.

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52 See Chapter 2, Section 2.2.3.

53 Beyrchen, Scientists under Hitler, 46. Since Delbrück had not been driven from Germany, his political loyalties were briefly suspect. In September, 1939, Warren Weaver of the Rockefeller Foundation wrote to Thomas Hunt Morgan at Caltech to inquire about Delbrück’s situation. Morgan wrote back: “I think I can answer quite easily the questions you ask concerning Delbruck [sic]. He is not expatriated and not Jewish and, in theory at least, he could return to Germany. On the other hand I can say on my own responsibility that I think he is entirely out of sympathy with the present Nazi organization in Germany, and with his very liberal and democratic views he might very easily find himself in a tight place if he returned to Germany under the present conditions. He is not, then, in a technical sense, a deposed scholar but he is undoubtedly a scholar under very distressing circumstances.” Warren Weaver to Thomas Hunt Morgan, September 20, 1939; Morgan to Weaver, September 27, 1939. Both letters RAC, Record Group 1.1, Series 200, Box 200, Folder 164.

54 Stent, Nazis, Women, and Molecular Biology, 290.

This comparison reveals a great deal about Delbrück’s intentions. He was not content to introduce a new model system into biological research, but instead wanted to transform biology into a science more like physics. He believed that there were biological laws just as there were physical laws, and if one were to find simple enough experimental systems one could eventually uncover these laws. Delbrück went to great lengths to impress this attitude upon all of his co-workers. He founded an annual summer workshop at Cold Spring Harbor, New York, the purpose of which was to introduce young researchers to phage genetics.\textsuperscript{56} The workshop also served as a kind of indoctrination to Delbrück’s style of thinking, as well as a rite of passage into a clique of researchers who prided themselves on their independence from traditionally minded thinkers in biology. Gunther Stent has recently recalled his own experience at the summer Cold Spring Harbor phage course:

By the time the course was over, I felt I had become an expert phagologist. I had also imbibed the conceit of the phage group that there was no point in paying attention to the work of our predecessors or of contemporaries external to the “Church,” as the French microbiologist André Lwoff, referred to the coterie of Pope Max’s disciples. Reading publications lacking the Church’s imprimatur was worse than a waste of time: The unsubstantiated claims based on poorly designed experiments presented by such confused heathen outsiders would just put wrong ideas in the True Believer’s head.\textsuperscript{57}

After a number of years as a member of the faculty of Vanderbilt University in Tennessee, Delbrück returned to Caltech in the late 1940s. He became one of the most important figures in postwar biology because of his personality, leadership, and organizational style, and by the 1950s, no molecular biologist’s training was complete without at least a brief stay at Caltech.\textsuperscript{58}

\textsuperscript{56} Delbrück, “Oral History,” 69.

\textsuperscript{57} Stent, Nazis, Women, and Molecular Biology, 310-11.

Delbrück had met most of the TMV researchers during his own years in Dahlem, and he continued to have especially positive feelings toward Melchers. Delbrück returned to Germany on a number of occasions in the 1940s and 1950s and was almost single-handedly responsible for bringing phage genetics to the Federal Republic of Germany. Delbrück sent scientific literature to his German colleagues, helped arrange opportunities for young German scientists to study in Pasadena, and eventually used his influence to help reform the German universities and make them more compatible with the kind of team-based research that was essential in molecular biology. Without Delbrück’s assistance, the first sustained phage research in Germany, done by Carsten Bresch and Wolfgang Eckart in Berlin, would not have been possible.

Other émigrés helped as well. Hans Gaffron was a biochemist who had worked with Neuberg and von Wettstein. He emigrated in 1937, and in 1939 began a career at the University of Chicago. He, like Delbrück, helped provide material and financial assistance to his former colleagues after the war. In early 1946, Gaffron helped collect the signatures of 177 prominent scholars of Austrian and German origin to support an appeal for the humane treatment of the German people; this extraordinary document was sent to every member of the US Congress. Fritz Lipmann, who had emigrated to the United States from Denmark in 1939 and later shared the 1953 Nobel Prize with Hans Krebs, very graciously helped a number of young German researchers by allowing them

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60 Bresch and Eckart heard a lecture by Delbrück in 1947, after which they began phage work, with Delbrück’s support, in a lab in Berlin. Later Bresch received a position at the Max-Planck-Institute for Physical Chemistry in Göttingen, where one of the directors was Delbrück’s brother in law, Karl-Friedrich Bonhoeffer. It was here that the first doctoral studies in phage genetics in Germany were done under Bresch’s supervision. Interview with Carsten Bresch, Freiburg, June 9, 2000.

61 Gaffron to Delbrück, December 10, 1948, and Delbrück to Gaffron, July 11, 1949, both in Delbrück Papers, Carton 8, Folder 17.

62 A copy of the Appeal is in the Gaffron Correspondence, Delbrück Papers, Carton 8, Folder 17.
to work in his lab at the Rockefeller University in New York. Of these, the most notable was Hans Georg Zachau, one of Butenandt’s pupils, who was the first person to sequence transfer RNA correctly.\footnote{Fritz Lipmann, “A Long Life in Times of Great Upheaval,” \textit{Annual Review of Biochemistry} 53 (1984) 1-33; Hans G. Zachau, “Life with tRNA, chromatin, immunoglobulin genes: recollections of a German molecular biologist,” \textit{Comprehensive Biochemistry} 41. The American Robert Holley received a share of the 1968 Nobel Prize in physiology or medicine due to his own work on the sequencing of tRNA, published a couple of months before Zachau’s. However, Holley’s sequence contained errors that were corrected in the 1970s while the sequence published by Zachau in the 1960s was correct.}

There were scholars abroad whose relationship with the Germans was not exclusively positive. One of these was Max Perutz, an Austrian émigré then at Cambridge, whose crystallography research on the structure of hemoglobin earned him a share of the Nobel Prize in chemistry in 1962. Perutz knew of Schramm’s membership in the SS and had no reason, personal or professional, to overlook it. Perutz remembered that he even excluded Schramm from the European Molecular Biology Organization because of his political past.\footnote{Telephone interview with Max Perutz, August 8, 2000. It is with regret that I write that Max Perutz died on February 6, 2002.} Perutz’s own experiences as an émigré did \textit{not} cause him to reject the German scientific community as a whole, though. He worked with Freksa to found the EMBO and developed an extremely positive working relationship with Gerhard Braunitzer, a young biochemist who had also worked with Schramm.\footnote{Braunitzer worked on the amino acid sequence of hemoglobin while Perutz studied its three-dimensional structure via x-ray crystallography. Perutz remembered an occasion when Braunitzer arrived in Cambridge and was very excited because he had found a deletion of five amino acids in the alpha chain of the molecule. He and Perutz then looked at Perutz’s model and were able to see where the amino acids were missing, which was a very exciting moment; it demonstrated that the chemistry of Braunitzer’s approach and the pure physics of Perutz’s had complemented one another beautifully. Telephone Interview with Max Perutz, August 8, 2000.}

Finally, many German scientists emigrated to other countries in the years immediately after the Second World War, providing yet another link between the German researchers and the international community.\footnote{For example, Gernot Bergold emigrated to Venezuela and was replaced by Werner Schäfer in 1948.} Of these, perhaps the most important for
the Tübingen group was Anton Lang, a botanist and close friend of Melchers. Lang left in February, 1949, and eventually accepted a position at Caltech. From there he and Melchers maintained a friendly and voluminous correspondence about all matters personal and professional, which provided Melchers with an insider’s account of the most recent developments on the West Coast.

4.2.4 From Correspondence to Personal Exchange

While exchanging letters, attending conferences, and receiving the latest literature were important, the most essential element in cementing scientific relations was personal exchange. Only by spending extended time in other laboratories could researchers make friendships, establish professional contacts, and acquire the tacit knowledge necessary to master new experimental techniques. As we have seen, such visits were not possible for the German researchers in the immediate postwar years. At the same time it quickly became clear that American scientists were at the forefront of biological research and that an extended stay there would soon be a necessary stage in the career of a young scientist. As Peter Starlinger, who did his doctoral work in the 1950s, remembers, “When I was a graduate student in the Max-Planck-Institut für Virusforschung in Tübingen, the scientific gospel came from the United States.”

Therefore when exchange with foreign institutes resumed in the late 1940s, America was usually the destination of choice. Since the Federal Republic did not have the money to send students to study abroad or to create exchange programs, the first few academic exchanges were infrequent and dependent upon luck and personal contacts. By the late 1950s, additional funding and established exchange programs made study abroad much more common.

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The first of the Tübingen researchers to spend a significant period abroad was Wolfhard Weidel, who started his career studying chemistry and medicine. In 1938 he began to work with Butenandt at the KWI for Biochemistry, where he remained for the next 11 years. He was never part of the TMV group, but during the war years he worked with Butenandt and several others on biochemical genetics using insects as models. The most significant work published by this group, the statement that individual genes coded for the creation of individual enzymes, was a part of Weidel’s doctoral research. In Tübingen he became an assistant in the institute for Biochemistry and continued his medical studies, achieving promotion to doctor of medicine in 1948.

Despite his close association with Butenandt and his faithful performance of his military service, Weidel never joined the Nazi Party. Therefore, when Max Delbrück was searching for a bright, politically unburdened young researcher to invite to Pasadena, Melchers was able to recommend Weidel without reservation. Butenandt was angry that Delbrück had asked Melchers for a recommendation rather than Butenandt, and told Weidel that if he were to accept Delbrück’s offer, there would be no position for him.

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69 Research developed in Weidel’s doctoral dissertation, completed in 1941 and entitled “Chemische Untersuchungen über die zur Augenpigmentbildung führende Reaktionskette bei Insekten” was published in several articles in 1940 and 1941 with Butenandt and Erich Becker as co-authors. The most important was Butenandt, Becker, and Weidel, “α-Oxytryptophan als “Prokynurenin” in der Augenpigmentbildung führenden Reaktionskette bei Insekten,” *Die Naturwissenschaften* 28 (1940): 447. Weidel, Butenandt, and Becker have been credited with being the first to articulate what would later be called the “One Gene, One Enzyme” theory usually associated with George Beadle and Edward Tatum. Their work did not become nearly as widely known as Beadle and Tatum’s Nobel Prize winning work for several reasons. First it was published in German when the German research community was largely isolated, but second and more importantly, Beadle and Tatum’s announcement was based on work with a much more promising experimental system, the mold *Neurospora*. In any case, the German researchers did not pursue this line of inquiry very aggressively in the 1940s, allowing Beadle and Tatum to set the agenda in the field. See accounts in Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community 1900-1933* (Chicago: University of Chicago Press, 1993), 90-91; Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994), 228-238, and Lily E. Kay, “Selling Pure Science in Wartime: The Biochemical Genetics of G.W. Beadle,” *Journal of the History of Biology* 22 (1989): 73-101.
when he returned. Weidel chose to leave anyway, and spent the 1949-1950 academic year at Caltech, where he learned phage genetics in the Delbrück style and established ties with the most significant figures in the emerging field of molecular biology, particularly Gunther Stent (yet another émigré). Having lost his position with Butenandt, Weidel returned to Tübingen in 1950 as a scientific assistant under Melchers in the Max-Planck-Institut für Biologie. He distinguished himself first in phage genetics and later in studies of the cell membrane of the bacterium *Escherichia coli*.

Not all the young researchers in Tübingen could count on a personal invitation from Max Delbrück, and given the scarcity of travel stipends, they could not depend upon institutional support, either. Therefore luck played a factor, as it did in the case of Klaus Munk, who had begun work with Butenandt and Schramm in 1948. Munk happened to have family in Ann Arbor, Michigan, one of whom was acquainted with Thomas Francis Jr., one of the University of Michigan’s leading virologists. Through this fortuitous personal connection, Munk was able to spend the year 1951-1952 in Francis’ laboratory, the first German to work there after the war. As with Weidel, this stay was essential for meeting American scientists and learning new techniques. For example, in Ann Arbor Munk learned to work with a new model virus, the Herpes-simplex virus.  

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70 Stent, *Nazis, Women, and Molecular Biology*, 349-352. Stent became friends with Weidel during the latter’s visit to Caltech and his memoir therefore contains the most detailed information about Weidel’s personality, background, and Caltech stay that I have found in English or German. Stent’s is the only account of the conflict between Weidel and Butenandt that I have found, but it fits perfectly with the other facts available, especially Butenandt’s publication of Weidel’s research under his own name and Butenandt’s acrimonious postwar relationship with Melchers. Stent also reports that Weidel’s political past was not as clean as Delbrück thought; technically Weidel was a member of the SA because a youth group to which he belonged was absorbed into the SA. This led Stent to believe that Weidel “was not merely a generic Boche but a certified, card-carrying Nazi.” 350. In truth membership in the SA was much more nebulous than membership in the party itself, and Weidel’s explanation makes sense. Furthermore the SA lost its prominence when Hitler liquidated its upper leadership in 1934, after which Himmler’s SS became the armed thugs of the movement. Given Weidel’s conservative political opinions, I believe that the fairest assessment is like so many others he was not in favor of the Nazi party, but did not publicly distance himself from it either.

71 Interview with Klaus Munk, Heidelberg, July 20 and 22, 2000; Klaus Munk, “Jeder lebt in seiner Zeit—Meine Errinnerungen als Internist und Virologe,” in *Emeriti Erinnern Sich: Rückblicke auf die Lehre und* 197
By the early 1950s, regular exchange programs were established that made the process somewhat less arbitrary. Among these, the Fulbright Program, begun in 1946, soon became one of the most important academic exchange programs between the Federal Republic of Germany and the United States. In 1953, German students first became eligible to spend yearlong research trips in the United States sponsored by the Fulbright Commission of the Federal Republic of Germany. Included in this first class of students was a young physicist from Göttingen named Alfred Gierer.

Gierer was born in 1929. The war and occupation had interrupted his formal education, so largely on his own initiative he studied for the Abitur, which he received without a complete Gymnasium education. He was interested in physics and began his post-secondary studies in Göttingen at the Max-Planck-Institut für Physik with Werner Heisenberg’s associate, Karl Wirtz, as his adviser. Wirtz told Gierer that if he were younger, he would have studied proteins rather than nuclear reactions; as a result of his encouragement, Gierer focused on the physics of biological processes, particularly the jumping of protons across hydrogen bonds. Linus Pauling had just announced the discovery of the alpha-helix motif in protein structure and became Gierer’s new scientific role model, replacing Heisenberg. In 1952, Gierer and Wirtz traveled to Tübingen, where Pauling was lecturing on the alpha helix on Butenandt’s invitation. After the lecture, Gierer met Friedrich-Freksa, who was a close friend of Wirtz. In 1953, Gierer completed his Ph.D. in physics and was awarded his Fulbright grant to study in the US. His first choice was to study at Caltech, but he was sent to MIT, his second choice, where he


72 For example, in 1952 the Deutsche Forschungsgemeinschaft began a new program of stipends to support research of German scholars abroad. “Bericht der deutschen Forschungsgemeinschaft über ihre Tätigkeit vom 1. April 1952 bis zum 31. März 1953,” 53-55.

73 Interview with Alfred Gierer, Tübingen, May 23, 2000.
worked on the thermodynamics of enzyme reactions. As with Weidel and Munk before him, Gierer's time in America was essential for his training and development as a scientist.

When he returned to Germany, he hoped to work in a biology institute. At the time, the Tübingen group was looking for a student with a background in physics, which seems to have been suggested by Delbrück, who thought it was very important that physicists and engineers be involved in the new biology. He therefore found it shocking that there were no physicists as part of the Tübingen group and that the physicists in Göttingen were not involved in biology. Melchers communicated this need to Wirtz in a letter in 1954, and so when Gierer returned and expressed his desire to work in biology, Wirtz immediately though of Tübingen. Freksa, the most open-minded intellectual of the entire Tübingen group, was happy to accept Gierer, thereby initiating an exceedingly fruitful mentor/student relationship.

The other side of academic exchange, the long-term stay of Americans in Tübingen, developed more slowly for several reasons. Few Americans had the necessary German language skills, and few desired to leave the generally better material and financial situation of American labs. There were exceptions, of course. Professor Emil Witschi of the University of Iowa spent 1948-49 as a visiting professor at Tübingen University, sponsored by the Rockefeller Foundation. While in Tübingen, Witschi worked with Kühn, developed a very positive relationship with Butenandt, and published

74 Melchers to Karl Wirtz, April 22, 1954. Melchers Papers, Ordner 6.

75 Interview with Alfred Gierer, Tübingen, May 23, 2000.

76 Interview with Peter-Hans Hofschneider, Munich, August 2, 2000; E. Kellenberger, "Erinnerungen zu den Anfängen der Molekularbiologie in Europa." Speech given before the Max-Planck-Institute for Biology, Schloss Ringberg, November, 1988. According to Kellenberger, who worked in Switzerland, in the 1950s when American students went abroad to learn the biological sciences they went to Cambridge, Paris, Copenhagen, and Genf—in short, everywhere but Germany. I would like to thank Peter-Hans Hofschneider for providing me with a copy of this speech.
an article in the German journal *Die Naturwissenschaften.* On the whole, though, American scholars did not begin regular research stays in Tübingen until the late 1950s. More often than not, the Germans had to content themselves with short visits from some of the most important figures in the field in order to keep themselves informed and networked. As mentioned above, Linus Pauling presented his work in Tübingen in the early 1950s. The strong contact with Wendell Stanley resulted from repeated personal visits from him and his colleagues. Heinz Fraenkel-Conrat, a Jewish émigré who would begin working with Stanley in 1952, visited in 1951 while on a Rockefeller Fellowship in Europe, and Stanley himself visited Tübingen in 1955 and 1957.  

An important connection with Watson and Crick developed after the publication of their model of DNA in 1953. Watson had been interested in TMV prior to his DNA work and maintained this interest even as he began work on the double helix model.  

His work with TMV made him familiar with Schramm and the work of the Tübingen community, and Watson wrote a very important article on the helical shape of TMV shortly after the publication of the structure of DNA. This article depended entirely upon the assistance of Watson’s English colleagues, but he soon developed a positive working relationship with the Tübingen researchers, especially Gierer and Schramm. Watson encouraged Rosalind Franklin to continue her X-ray crystallography studies on

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78 Butenandt to Stanley, December 20, 1951, Stanley Papers Carton 6, Folder 199; Stanley to Klaus Munk, 1955, Stanley Papers Carton 10, Folder 73; Stanley to Schramm, September 27, 1957, Stanley Papers Carton 12, Folder 24.


TMV, and she visited Tübingen to obtain samples, which she and her co-workers later used to confirm some of Schramm’s results. Francis Crick recalls that the English researchers had closer contact with Americans than with the West Germans, but that this relationship was the result of shared research interests rather than a rejection of the West German research community. Crick also emphasized that he and his colleagues did develop a very strong relationship with Gierer, whose interest in nucleic acids and background in physics closely mirrored Crick’s own.

The relations of the Tübingen TMV group with colleagues in Britain were not universally cordial, however. In the early 1950s, the third major center of TMV research was in England, centered on Frederick Bawden and N. W. Pirie. Bawden and Pirie worked closely together, had co-published extensively, and were quite critical of the German work after the war. When James Watson asked Pirie about Schramm’s work in 1952, Pirie said that it should be dismissed. At that time Pirie’s opinion was exceedingly important. The occasion of his discussion with Watson was the 1952 Symposium of the Society for Experimental Biology, organized by Bawden and Pirie themselves. The subject of the symposium was viral replication, and, according to Watson, there were more than four hundred microbiologists in the audience, making it one of the most significant scientific gatherings in Europe. None of Tübingen’s leading virologists presented his work.


83 Telephone interview with Francis Crick, November 9, 2000.

84 Watson, *Double Helix*, 72.

85 Although I have tended to write of Bawden and Pirie as though they were a collective entity, they were very different in personality type and their behavior in their disputes with other TMV researchers was distinct. Bawden was much more cordial and outgoing, and his correspondence with Stanley has an air of
Bawden and Pirie’s behavior raises the question of whether or not anti-German sentiment was a factor inhibiting the acceptance of the German researchers after the war. According to Francis Crick, who knew Bawden and Pirie well, it was not. Crick saw no evidence that Schramm’s political past was held against him, and in his opinion, Bawden and Pirie simply saw themselves as the experts on TMV and were very critical of all other research on the subject, regardless of national origin. For their part, Bawden and Pirie certainly did not single the Germans out for criticism; they were extremely negative in private and public criticisms of the work of both Wendell Stanley and Heinz Fraenkel-Conrat as well, giving credence to Crick’s interpretation of Bawden and Pirie’s relationship with the Germans.\footnote{In a letter from Bawden to Stanley, dated September 27, 1937 the two disagreed on whether or not TMV was a protein. Bawden said “Not unnaturally, I think we are right and you are wrong. Apparently, from a letter in this week’s Nature, Best also thinks our way. We had no real intentions of hoisting you on your own petard, or of taking shots at you with your own ammunition, but if you will produce the strongest evidence against your own theories you must expect somebody to use it.” On several occasions Stanley indicated that he felt Bawden’s critiques were unduly harsh; see letter Bawden to Stanley, March 8, 1938 as well as Bawden to Stanley, Feb. 14, 1940, where Bawden says “I was sorry to hear that you thought I ‘panned’ you in my book, especially as I was under the impression I had treated you lightly.” All three letters Wendell Stanley Papers, Carton 6, Folder 85. Gunther Stent writes “In his supercilious Cantabrigian English, Bawden treated Stanley like a colonial bumpkin: he couldn’t forgive the Ohio-born Stanley for having gotten the Nobel Prize despite this error and for having made the public-relations-effective claim that TMV is a ‘living molecule.’” \textit{Nazis, Women, and Molecular Biology}, 366. Pirie was also publicly contemptuous of Stanley and his co-worker Heinz Fraenkel-Conrat. In a symposium on the Origins of Biochemistry held in 1978, Pirie launched an extensive critique of a talk given by Fraenkel-Conrat, at one point saying “You quoted Knight as having shown amino acid differences. I wonder if you have ever read that paper; if so, you should again.” \textit{In The Origins of Modern Biochemistry: A Retrospect on Proteins}. Volume 325 of the Annals of the New York Academy of Sciences. Edited by P.R. Srinivasan, Joseph S. Fruton, and John T. Edsall. New York: New York Academy of Sciences, 1979, p. 317.}

4.3 \textit{Scientific Research after the War}

4.3.1 Postwar Virus Research

The evacuation, dispersal, and personnel changes resulting from the lost war certainly imposed limitations on the research done in Tübingen, but these factors in no
way altered the basic goal of the TMV group—to use viruses as experimental models for understanding genes. They had always known that this would be a long-term process, which the war had delayed significantly. They began by looking closely at TMV itself. Early postwar work concerned the structure of the virus particle. Building upon this work they turned to the biological function of the viral sub-components. It was at this point that their research began to transcend plant virology and have significance for all of biology. The final step in their program was to apply this knowledge by using variation in TMV strains to illustrate how the biological message of genes becomes living tissue. Of course this program appears much neater in hindsight than it did in the late 1930s or in the postwar years. Nevertheless, the goal was always to use TMV as a model system, and that process began with understanding the basics of the virus itself.87

The goals of the group were restated in a lengthy 1946 article co-authored by Melchers, Schramm, and Freksa.88 The three had completed research for this paper during the war years, and submitted it for publication before the end of hostilities. Like earlier papers, it reflected the importance of interdisciplinary work and the strategy of comparison between viral strains, but it was focused more narrowly on drawing parallels between viral mutants and genetic mutation. The authors devoted most of their attention to the task of tracing changes generated in host plants to chemical changes in the virus particles themselves. The most challenging element of the work was to demonstrate that different symptoms in the host plants could be linked to changes in the virus particles and were not the result of environmental influence or the interaction between virus and host plant.

87 I would like to thank Karl-Wolfgang Mundry for helping me to understand the TMV research in this framework.

By comparing mutants with normal strains of the virus, they found that certain characteristics, such the sedimentation constant in the ultracentrifuge, were essentially the same, while serological characteristics varied from strain to strain. Therefore, although the various strains were similar, slight differences in the makeup of the viral particles could account for the difference in observed symptoms. They also determined that there were sub-units on the virus that could be altered independently of one another.\(^89\) The presence of sub-units within the viral particles complicated matters by opening the question of whether viruses could best be thought of as models for individual genes or for chains of genes.\(^90\) Either way, the strategy chosen in 1937 still seemed to be viable.

Of the individual researchers, Melchers faced the greatest material difficulties in the postwar era. After the evacuation in 1943, he had no experimental greenhouses until the completion of his new institute in 1950, meaning that for seven years he was unable to carry out controlled genetic experiments on a very large scale.\(^91\) The situation was extraordinarily frustrating for him, for during this time he was occupied with the importance of mutation and its significance as a way of illustrating genetic change. In Melchers' thinking, mutation was the best way to produce the variation necessary to carry out genetic experiments using TMV. The most certain way to increase variation was to find a way to induce genetically stable mutations in TMV (that is, mutants that could reproduce themselves), which would give experimenters a host of strains to utilize in comparative experiments. Such comparisons might in turn make it possible for the researchers to link changes in viral symptoms to changes in the viral nucleoprotein.\(^92\)

\(^89\) Ibid; see summary, 221-222.

\(^90\) Ibid, 220.

\(^91\) Letter, Melchers to Dr. K. Mothes, September 30, 1949. Melchers Papers, Ordner 1.

\(^92\) In his talk at the 8th International Congress of Genetics in Stockholm in 1948, Melchers stated “Differences in the symptoms shown by different virus strains must be traced back to differences in the chemical constitution of the virus particles.” “Über Mutationen beim Tabakmosaik-Virus,” Proceedings of
Toward the end of the war, Melchers attempted unsuccessfully to use x-rays to induce mutations in TMV in a series of unpublished experiments. After the war, he split his research interests between TMV and the physiology of flower formation. When his greenhouses were finally finished, he assigned the task of mutation to one of his doctoral students, Karl-Wolfgang Mundry. Beginning in 1950, Mundry attempted to induce mutations in TMV *in vitro* by exposing the virus to x-rays and ultraviolet radiation. This part of his research was unsuccessful, but in the second part of his thesis, Mundry showed that one could increase the rate of mutation of TMV *in vivo* by increasing the temperature at which the host plants were kept.

Freksa published on a number of topics immediately after the war. He wrote a review article for the American intelligence survey of German science in which he restated the ideas from his 1940 paper on complementary genetic replication. He also took an interest in developments in America and in the late 1940s wrote reports on the latest work by Linus Pauling and George Beadle. Much of his own research in the early 1950s was co-published with his student Fritz Kaudewitz and focused on the use of

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95 Hans Friedrich-Freksa, “Kräfte beim Aufbau biologischer Struktureinheiten,” in *FIA Review of German Science 1939-1946. Biophysics Part I.* Published by the Office of Military Government for Germany Field Information Agencies Technical, 1948, pp. 44-49. Freksa’s model for genetic replication will be discussed in more detail later in this chapter.

radioactive isotopes in protein biosynthesis experiments utilizing amoebas and paramecia. He himself did very little direct research on TMV during this time.

Schramm was by far the most prolific of the three, authoring or co-authoring roughly forty publications in the first postwar decade. Most of these concerned the structure of TMV, although he also did exploratory work on other viruses such as influenza, phage, and fowl-plague virus. In his TMV research, Schramm returned to the strategy of studying structure by breaking the protein down into its polypeptide sub-units. His most significant work appeared in 1947, when he produced a detailed follow-up to his 1943 paper on the splitting and re-aggregation of TMV. His results were essentially the same, but this time he provided a more thorough set of experimental data. Schramm clearly showed that in a weak alkali solution the TMV particle split into eight sub-units of specific size, some of which contained nucleic acid and some that did not. When placed in a mild acidic solution the sub-units re-aggregated into a particle that was similar in size and shape to the original virus but was incapable of infection. This experiment showed that the overall shape of the virus particle was determined by the aggregation of its sub-units into an energetically favored configuration and was not guided by the viral RNA. Since the process involved the assembly of sub-units of varying sizes in a precise order rather than the random aggregation of equally sized pieces, Schramm called it “selective polymerization” (auswählenden Polymerisation) and compared it with laboratory experiments carried out on other proteins of high molecular weight. The implication was that the undirected aggregation of TMV sub-units

97 The work on fowl plague virus was co-published with Werner Schäfer, the Institute’s expert in animal virology.

illustrated a principle that might be quite common in the formation of proteins in higher organisms and therefore was of general significance.\textsuperscript{99}

In the early 1950s, Schramm pursued this strategy with the assistance of Gerhard Braunitzer, who had studied chemistry with Butenandt before joining Schramm’s division in 1950. Inspired by Frederick Sanger’s successful determination of the sequence of amino acids in the hormone insulin, they broke the viral sub-units down even farther. They suspected that each sub-unit was composed of similar, repeating, polypeptide chains. Since amino acids linked together in through the formation of a peptide bond between the C (carboxylic acid) end of one and the N (amino group, NH\textsubscript{2}) of the other, each chain would have one unlinked C and one unlinked N group at its ends.\textsuperscript{100} Schramm and Braunitzer attempted to identify the chemical groups at the N terminal of the TMV polypeptide. The results of their initial publication were flawed due to their method for preparing the TMV sub-units for analysis.\textsuperscript{101} Their work was immediately questioned by researchers in Wendell Stanley’s lab, leading to an acrimonious debate over the composition and number of terminal groups in TMV. By 1955, however, the investigators had determined among themselves that the TMV coat

\textsuperscript{99} Ibid., 120, 256. The desire to extrapolate the TMV results to other organisms shows that Schramm was not interested in TMV for its own sake but was intent on using it as a means to an end. He clearly articulated this in the introduction to the first half of the article, saying “Tobacco mosaic virus is a nucleoprotein that possesses previously determined characteristics of living creatures, for example, the ability of self-replication and of mutation. Therefore all progress in the determination of the constitution of its protein means progress toward the discovery of these basic phenomena of living things.” Quote p. 112.

\textsuperscript{100} Within proteins, amino acids are linked together by a reaction that eliminates a water molecule and forms a bond between the CO group at the end of one amino acid and the NH group at the end of the second. This CO-NH bond is called a peptide bond, and the two linked amino acids are then called a dipeptide. When many amino acids are linked together through a linear series of peptide bonds they are called a polypeptide. Proteins in turn consist of one or more polypeptide chains.

protein was composed of roughly 2,500 polypeptides that were probably identical, and they had identified the first few amino acids at both terminals.\textsuperscript{102}

Despite the West German researchers’ aspirations toward universality, their postwar TMV research had little impact on the scientific community as a whole outside of the relatively small circle of plant virologists. There were several reasons for this situation. Most of their papers were published in German-language journals with limited foreign circulation at a time when English had become the lingua franca of the international scientific community. Submitting articles to journals edited by people like Melchers, Butenandt, and Kühn was one way to for the Tübingen researchers to ensure publication, but in doing so they also reduced the potential audience for their work. However, interested parties, such as Wendell Stanley and his co-workers in Berkeley, could and did remain current with the German research so one must not over-estimate these limitations.\textsuperscript{103} The most decisive factor was the nature of the research itself, which was not particularly interesting to investigators outside the field of plant virology. In the mid-1950s, the Tübingen researchers had made some very interesting suggestions but had not yet achieved their goal of using TMV to address universal biological questions—they were still answering questions about TMV. While this was a necessary and anticipated stage in their overall research program, at that time they had little to offer to the rest of the scientific community. Even Delbrück, who could hardly be accused of not understanding German or not caring about German science, wrote in 1952 that the TMV

\textsuperscript{102} For a brief summary of this work see Schramm, Braunitzer, and J.W. Schneider, “Terminal Groups of Tobacco Mosaic Virus,” \textit{Nature} 176 (1955): 456-457. For a discussion of the disputes between the two labs during the course of this research see Creager, \textit{Life of a Virus}, 266-273.

\textsuperscript{103} In the late 1940s, Stanley moved to the West coast and established a lab at the University of California, Berkeley, where he remained for the rest of his career. The move is examined in Angela N. H. Creager, “Wendell Stanley’s Dream of a Free-Standing Biochemistry Department at the University of California, Berkeley,” \textit{Journal of the History of Biology} 29 (1996): 331-360.
research was "of mild interest only." In order to produce work that was of more general interest, the Tübingen researchers had to succeed in their third area of inquiry, that of the function of the viral sub-components.

4.3.2 Tübingen and the Double Helix

In her pioneering work on biological research under National Socialism, Ute Deichmann studied the aftereffects that twelve years of Nazi rule had on scientific research in the postwar era. She found that the Federal Republic of Germany was almost completely lacking in molecular genetics research until the late 1950s. She attributed this situation to the isolation of Germany during and after the war, which had prevented the Germans from participating in the exciting developments taking place in America in the 1940s. The few pockets of research that did exist, Weidel in Tübingen and Bresch in Göttingen, were made possible only by the direct intervention of Delbrück.  

Deichmann's work, as well as that of Ernst Peter Fischer and Carol Lipson, shows that postwar Germany was lacking in modern biological research in a more general sense and was significantly isolated from the rest of the biological research community. Deichmann noted the example of the Freiburg geneticist Friedrich Oehlkers, who as late as 1957 did not believe in the chemical basis of heredity. Fischer and Lipson wrote, "The double helix was news in Germany in 1954. Werner Reichardt had never heard of it, nor had the great Otto Warburg." These authors imply that it was not just molecular genetics in the Delbrück style that was lacking in Germany, but quality molecular biology research in general.

104 Delbrück to Karl-Friedrich Bonhoeffer, November 24, 1952. Delbrück Papers, Carton 4, Folder 5.

105 Deichmann, Biologists under Hitler, 305-317.

106 Ibid., 304.

107 Fischer and Lipson, Thinking About Science, 263.
As in so many other instances, Tübingen was the exception to this generally accurate rule. The researchers there were informed of new developments in biology, and they certainly did not remain ignorant of the most important new finding, the announcement of the Watson-Crick model of the structure of DNA in early 1953. In fact, Freksa was already discussing it publicly before an assembly of the Max-Planck-Gesellschaft in November, 1953.108 He clearly was aware of the importance of the model since he described its implications for genetic replication in some detail. He also cited the 1952 Hershey-Chase experiment as background for a shift in emphasis from proteins to nucleic acids as the genetic material.109

Because of his own belief that DNA had a complementary replication mechanism involving both proteins and nucleic acids, Freksa was the most prepared of all the Tübingen researchers to accept the Watson-Crick model. While still in Dahlem, Freksa had been inspired by physicist Pascual Jordan’s efforts to uncover the physical basis of biological heredity, and Freksa turned his own attention toward the respective roles of proteins and nucleic acids in gene replication. In a 1940 paper, he suggested that complementarity between the nucleic acid and protein components of a chromosome might explain gene replication. Freksa thought that the electrically positive nucleic acid might attract an electrically negative protein to either side of itself, a protein that was in some way structurally complementary. The protein in turn could serve as a negative template on which a new strand of nucleic acid could be assembled. In his proposal, the gene was a true nucleoprotein, with both parts playing a role in reproduction.110 This is

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109 See note 123 for more on the content and significance of the Hershey-Chase experiment.

not to claim that Freksa anticipated the Watson-Crick model by thirteen years. He did not. He was, however, predisposed toward a complementary mechanism for genetic replication and was therefore receptive to the model suggested by Watson and Crick. According to his student, Alfred Gierer, Freksa needed only to read the first three sentences of Watson and Crick’s paper to understand the importance of the structure they suggested.\textsuperscript{111}

It is entirely possible that many of the Germans who heard Freksa did not share his enthusiasm and that many others were completely unaware of the paper, which makes the reaction to the double helix in West Germany the same as in the rest of Europe and the United States. Not everyone in the world was immediately aware of the double helix. For example, the American protein researcher Paul Zamecnik remembers that he did not learn of it until 1954 which suggests that researchers who were interested in nucleic acids became aware of Watson and Crick’s work rather quickly.\textsuperscript{112} Those, such as Warburg, Oehlkers, and Zamecnik, who investigated other topics, learned of the double helix structure only later.

Watson and Crick’s model was not universally accepted during the first years after its publication, as historian Frederick Holmes has demonstrated in his recent book \textit{Meselson, Stahl, and the Replication of DNA: A History of “The Most Beautiful Experiment in Biology.”} Concerning the situation in 1953, after Watson presented the model at the annual Cold Spring Harbor meeting, Holmes wrote:

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\item[(Seattle: University of Washington Press, 1974), 114-115, and Deichmann, \textit{Biologists under Hitler}, 212-213.]
\item[\textsuperscript{111} Interview with Alfred Gierer, Tübingen, May 23, 2000.]
\end{itemize}
\end{footnotesize}
The prevailing approach among those who had read the *Nature* articles or had heard Watson speak was probably to await with open minds for further information—unless, like the plant physiologist Barry Commoner, who, according to Watson's recollection, 'hated the talk,' they adamantly opposed Watson and Crick's strategy of ignoring the protein component of the gene.\(^{113}\)

Holmes cites the 1958 version of Joseph Fruton's biochemistry textbook, in which the double-helix model of the structure of DNA was described as "ingenious speculation," as evidence of the continuing skepticism toward the acceptance of nucleic acids as the genetic material.\(^{114}\) Such resistance to the double helix was hardly confined to the Federal Republic of Germany.

Watson and Crick had not proven anything; they had simply described in detail the structure of a molecule, but the structure they illustrated was so evocative that many intuitively accepted their model and its implications for genetic replication. Those who chose not to believe required substantial proof to alter their positions. Holmes convincingly shows that the demonstration of the semi-conservative nature of DNA replication by Meselson and Stahl provided such a piece of evidence. However, it was not the only piece of evidence to emerge in the years immediately after 1953. In 1956, the Tübingen researchers themselves produced a significant piece of evidence that supported a nucleic acid version of the gene.

### 4.3.3 Nucleic Acids

In the early 1950s, the German researchers began to rethink their conceptualization of the physical nature of the gene. To that point, a nucleoprotein version of the gene, as defined by historian Robert Olby, had characterized the thinking of the Tübingen community.\(^{115}\) Schramm in particular held to this model, for good

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\(^{115}\) In a December, 1952, discussion on the nature of genes, Butenandt stated that they were nucleoproteins, which allowed him to elaborate on the comparison between genes and viruses, calling viruses "vagabond
reason. In his comparisons of different strains of TMV, Schramm found that the nucleic acid portion of the virus did not vary chemically from strain to strain, and he found only slight amino acid changes. Schramm attributed these slight changes to differential folding of the various polypeptide chains. With no convincing evidence to the contrary to change his thinking, he, like Stanley, continued to believe that proteins were the more important part of the two genetic components.\textsuperscript{116} Shortly thereafter, though, the Tübingen researchers began to produce evidence that enabled them to reconsider the roles of protein and nucleic acids in heredity. This work marked the point at which the West German TMV research program, initiated in 1937, began to make significant contributions to general molecular biology research.

Freksa continued to believe in the complementary replication scheme he first proposed in 1940 until well after the war, restating it in print on at least two other occasions. He published a short piece on forces in the construction of biological structural units for the FIAT Review of German Science; an edited version was later published in the journal \textit{Angewandte Chemie} in 1948.\textsuperscript{117} Freksa’s general model remained the same—he continued to believe that the nucleic acid was located on the surface of the TMV particle and therefore served as a negative template upon which a protein was assembled due to electrostatic attraction. There was one significant difference

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\item \textsuperscript{116} Schramm, “Die Struktur des Tabakmosaikvirus,” 479-481. We now know that even though the overall chemical composition of the nucleic acid did not vary appreciably from TMV strain to TMV strain, the information content expressed by the sequence of bases did. In the early 1950s, neither Schramm nor Stanley was thinking in terms of information content—they were thinking in terms of the biological specificity of molecules with different chemical compositions and structures. For Stanley’s influence on Schramm’s thinking, see Rheinberger, “Virusforschung,” 689-690.
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\item \textsuperscript{117} Freksa, “Kräfte,” and “Eine Modellvorstellung des Vorgangs der Selbsvermehrung,” \textit{Angewandte Chemie} 60 (1948): 22-23.
\end{itemize}
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in his postwar thinking, however, he believed that there were differences in specificity among nucleic acids. That is to say, DNAs and RNAs from different organisms could vary in composition; they were not necessarily all identical, as many investigators believed at that time.\(^\text{118}\) Freksa began to think that differences in nucleic acids might account for the differences in proteins. He believed that, at the very least, differences in nucleic acids might cause the protein chain to fold up in a slightly different manner and lead to changes in its three-dimensional shape and therefore in its function.

This idea was quite new at the time, and Freksa based his thinking upon a very important paper published in America at the height of the war. In 1944, Oswald Avery, Colin MacLeod, and Maclyn McCarty identified DNA as the substance responsible for the transformation of pneumococcal bacteria from one strain to another.\(^\text{119}\) Their work showed that DNA, not protein, was the significant factor in determining heredity traits in bacteria. At a time when most investigators still believed that protein was the key to understanding genes, Avery, MacLeod, and McCarty's work was not widely accepted or believed. Critics suggested that their DNA fractions were contaminated with a small amount of protein that was actually responsible for the change.\(^\text{120}\) Nevertheless, the paper eventually did have a significant impact, even though it took several years to manifest itself. The paper inspired Erwin Chargaff to shift the emphasis in his lab toward the study of DNA. The regularities in DNA structure discovered by Chargaff in the late 1940s and 1950s provided vital clues for the determination of the structure of DNA by

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\(^{118}\) Freksa, "Kräfte," 49.


\(^{120}\) For a discussion of the reception of this work and the arguments leveled against it see Maclyn McCarty, *The Transforming Principle: Discovering that Genes are Made of DNA* (New York: W.W. Norton and Company, 1985), 213-235.
Watson and Crick in 1953. Avery, MacLeod, and McCarty's paper thus marked the beginning of a significant shift in the way that biologists understood the gene on a physical/chemical level, although few recognized this change at the time.

With this in mind, it is striking that Freksa understood the significance of the paper shortly after its publication. The paper did not persuade him to give up his belief in genes as nucleoproteins, but it did induce him to ascribe more influence to nucleic acids. Freksa wrote:

An important consequence of this hypothesis is that the nucleic acids of various nucleoproteins must lead to specific differences between them, at least in how they are folded. Prior to 1944 there had been no research in this direction. In the meantime, the investigations of Avery (1944) have shown that specific differences of nucleic acids exist among the various strains of pneumococcus.¹²¹

Freksa had clearly grasped the implications of the Avery paper—that DNA might be responsible for differences in an organism's characteristics. However, he was not immediately prepared to abandon his belief in the nucleoprotein version of the gene. Instead he accepted the Avery paper's results critically and did his best to reconcile them with his own preconceptions, which is more than many other researchers at the time were willing to concede. Freksa's familiarity with the Avery paper in the 1940s, when it was not widely discussed among geneticists, is further evidence that Tübingen was not cut off from all of the latest American scientific literature. It also shows that the researchers there were not ignorant of important advances, nor were they unwilling to consider new ideas on the nature of the gene.¹²²

Freksa's interest in TMV seems to have declined in the early 1950s as his research shifted toward collaborative work in microbiology with his student Fritz

¹²¹ Freksa, "Kräfte," 46.

¹²² In this paper Freksa thanked both Butenandt and Schramm for ongoing discussions regarding his work, which suggests that they too were aware of Avery's findings.
Kaudewitz. The Watson-Crick paper rekindled his interest in 1953, and when Alfred Gierer joined Freksa's division of the Institute for Virus Research in November, 1954, Freksa steered him toward research on the function of the viral sub-components. Gierer's first, unpublished, experiments attempted to discern whether RNA, or protein, or both determined the length of the TMV particle. There were no immediate answers to this question, but the work allowed Gierer to become familiar with the physico-chemical features of TMV, its components, and their interactions.

Schramm was a strong believer in the primacy of proteins over nucleic acids in the postwar years. While Freksa was willing to concede both a role in heredity, Schramm was initially quite reluctant to do so. According to Melchers, when the three of them worked together on parallel mutants of TMV, Schramm was of the opinion that it was simply not possible for the differences between the strains to be attributed to differences in their nucleic acids.\textsuperscript{123} As noted above, to Schramm the experimental evidence seemed to support such an interpretation. Schramm was interested in the structure of nucleic acids, DNA in particular, but at that time he did not anticipate their centrality in the storage and transmission of biological inheritance.\textsuperscript{124} It is unclear if the announcement of the double helix model of DNA had as immediate an effect on him as it did on Freksa, for Schramm did not cite it very often in his own research. What is clear is that by the mid-1950s his strategy of breaking down and re-aggregating the virus particle had begun to suggest that protein was not the genetic material after all.

\textsuperscript{123} Melchers, "Phytopathogene Viren," 116.

\textsuperscript{124} When Klaus Munk worked with Schramm in the late 1940s much of their research centered on splitting up DNA and analyzing its components in the ultracentrifuge as a strategy for learning its structure. Munk, "Jeder lebt in seiner Zeit," 448; also see Schramm, Munk, and W. Albrecht, "Über die enzymatische spaltung der Desoxyribonucleinsäure aus Thymus und der Ribonucleinsäure aus Hefe," \textit{Zeitschrift für Naturforschung} 7b (1952): 10-18.
In 1955, Schramm and two co-workers, Wolfram Zillig and G. Schumacher, published a very significant paper to this effect. Their experimental strategy was similar to that of Schramm's 1943 and 1947 papers—the dissolution of TMV in an alkaline solution, the analysis of the products, and their eventual re-aggregation.\(^{125}\) In this experiment they broke the virus into four fractions of varying compositions. They then tested the fractions for biological activity by applying them to tobacco leaves and found that two were capable of reproducing themselves—they had the activity of an intact virus. The protein of both the infectious fractions had been degraded while their RNA molecules were intact. Electron microscope pictures of one of the fractions were striking—they showed several pieces of protein held together by a thread of what the researchers took to be RNA.

By this time Schramm and his colleagues had understood that the TMV RNA molecule was located within the virus particle, not on the surface as they had assumed for many years. This was the result of work done by Peter Starlinger, a brilliant student completing his doctoral dissertation in Tübingen in 1955.\(^{126}\) Starlinger's work allowed the authors to interpret the results of their infectivity tests and electron micrograph photos correctly. They knew that the infectious fraction contained RNA and degraded protein, and since they knew also that the RNA was inside the molecule and was only a small percentage (9 per cent) of the entire fraction, it seemed obvious to them that RNA was the thread holding the infectious nucleoprotein fraction together.


\(^{126}\) Schramm, Schumacher, and Zillig, "Über die Struktur des Tabakmosaikvirus. III Mitt.," 490-491.
The fact that a degraded TMV particle with its complete RNA core was able to reproduce itself led the researchers to a number of exciting hypotheses. First of all, they compared their work with that of the Hershey-Chase experiment. Published in 1952, this experiment had shown that when phage infected bacteria, the majority of the protein portion remained on the surface of the bacteria while the viral DNA was injected into the cell. The virus appeared to be little more than a protein syringe designed for injecting nucleic acid into a bacterial cell. Although Hershey and Chase stated their results in very tentative terms, their work strengthened the case for DNA as the carrier of heredity. Schramm and his co-workers suggested that the role of the protein in TMV was to stabilize and protect its RNA core, just as the protein coat of phage protected its DNA.

The most important suggestion of their paper is worth quoting in detail:

Chemical experiments, for example the acetylation of the free amino groups or the removal of the terminal threonine residues, have shown that it is possible to alter the protein of TMV by cutting into it without losing the viral infectivity. The attempts illustrated here show that it is probable that one can even break off a piece of the protein without the infectivity diminishing. These facts provide grounds for supposing that among plant viruses, in a fashion similar to the phages, the nucleic acid plays the decisive role for intracellular multiplication and that the protein only serves to stabilize these sensitive molecules.

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127 This has since become one of the most famous experiments in the history of molecular biology. Alfred Hershey and Martha Chase used radioactive phosphorous to label the DNA of the phage T2 and radioactive sulfur to label the virus’s protein. They were then able to track each component separately during the process of infection. Shortly after mixing the labeled phages with bacteria, they agitated the mixture with a Waring blender. They found that the labeled DNA had gone into the cells while the labeled protein had remained on the surface and had been sheered off during the agitation. This seemed to indicate that the DNA was the important component in infectivity, since most of the protein remained outside the cell. Alfred D. Hershey and Martha Chase, "Independent Functions of Viral Protein and Nucleic Acid in the Growth of Bacteriophage," *Journal of General Physiology* 36 (1952): 39-56. Although seemingly a convincing experiment, Hershey and Chase were very tentative in their conclusions and few immediately recognized the importance of the experiment. Many believed that that piece by Avery, MacLeod, and McCarty was more decisive and a better overall piece of research. James Watson later claimed that the Hershey-Chase paper convinced him that DNA was the hereditary material, but Frederick Holmes has argued that Watson and Crick had already set on this course and used the Hershey-Chase experiment to justify a position they had already taken. Holmes, *Meselson, Stahl*, 58-59.

128 Schumacher, Schramm, and Zillig, "Über die Struktur des Tabakmosaikvirus. III Mitt.," 490-491.

129 Ibid.
Thus, in the summer of 1955, researchers in a second division of the MPI for Virus Research had made a decisive turn toward the nucleic acid component of genes. In this case, the shift seems to have been the result both the influence of new findings such as the Hershey-Chase experiment as well as a decade long series of experiments. Exciting though these suggestions were, at this point the researchers had not proven anything, except that infectivity resulted from a combination of protein, albeit degraded protein, and nucleic acid. Decisive evidence would not be available until the researchers found a way to separate the two components without damaging the delicate RNA molecule.

4.3.4 Infectious RNA

In late 1955, Freksa encouraged Gierer to combine his own talents and interest in RNA with those of Schramm in order to reach concrete answers regarding the function of the viral sub-components. The result of their collaboration was a stunning paper produced in early 1956 that marked the most significant contribution of the Tübingen community to general molecular biology research.\(^{130}\) By modifying a chemical technique for protein isolation for use with TMV, they were able to separate the RNA from the viral coat protein without destroying it. When they assayed the RNA on tobacco leaves, they found that it generated lesions just as the intact virus did, albeit at a greatly reduced rate attributable to the damaging of some of the RNA during the process of separation. These results showed that viral reproduction was entirely a consequence of RNA, although the protein certainly played a role in protecting the RNA molecule. Gierer and Schramm stated their conclusions unambiguously:

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Nevertheless, on the basis of the preceding experiments it appears as though the nucleic acid is the one component of TMV that can actually bring about the occurrence of reproduction. Consequently its characteristics as the carrier of hereditary factors (Erbfaktoren) is clearly analogous to the role of DNA in bacteriophages and transforming factors.\textsuperscript{131}

The experiment was convincing for several reasons. After separating the RNA from the protein, Gierer and Schramm used serological evidence to show that the protein content of the preparation was less than .02 per cent, too low to account for the level of infectivity observed. They then used a number of controls to show that not even this tiny percentage of protein was responsible for the viral reproduction. Very small amounts of TMV anti-serum were known to inactivate the virus by reacting with the viral coat protein. The anti-serum did not react with free nucleic acid. Gierer and Schramm treated their RNA preparation and regular TMV with anti-serum and compared the results. Regular TMV was almost completely inactivated while the RNA preparation was scarcely affected. As a further control, they treated both with ribonuclease, an enzyme that literally cuts up RNA molecules. In this set of experiments, the activity of the TMV sample, protected by its coat protein, was unchanged while the exposed RNA sample was completely inactivated. This combination of evidence meant that the researchers could state their conclusions with considerable assurance.

The infectious RNA work complemented that done in Wendell Stanley’s lab by Heinz Fraenkel-Conrat. In 1955, Fraenkel-Conrat and his co-workers claimed that they had split TMV into its protein and nucleic acid components, neither of which was biologically active on its own. They then recombined the two into an active virus particle, which suggested to them that infectivity was the result of the combination of nucleic acid and protein.\textsuperscript{132} In early 1956, Fraenkel-Conrat substantially modified these

\textsuperscript{131} Gierer and Schramm, “Die Infektiosität,” 142.

\textsuperscript{132} Heinz Fraenkel-Conrat and Robley C. Williams, “Reconstitution of active Tobacco Mosaic Virus from its inactive protein and nucleic acid components,” \textit{PNAS} 41 (1955): 690-698.
conclusions in a brief report to the *Journal of the American Chemical Society*. Here he
argued that infectivity was a property of nucleic acid, not protein.\footnote{Heinz Fraenkel-Conrat, "The role of the nucleic acid in the reconstitution of active tobacco mosaic
virus," *Journal of the American Chemical Society* 78 (1956): 882-883.} He said that in
follow ups to his reconstitution experiments he had found that his own nucleic acid
preparations showed low levels of activity, and concluded "Virus rods could not be found
to account for this residual activity and it is now regarded as a characteristic property of
the nucleic acid itself."\footnote{Ibid., 883.} Since he reached the same conclusions using different
techniques (for example, his lab used a different chemical procedure for the isolation of
TMV RNA), the infectious RNA work was all the more persuasive.

Given the importance of priority in scientific publication, the fairest way to assess
the appearance of the two sets of research is to say that the research they discussed was
"independent and essentially simultaneous" as Wendell Stanley did in his letter
nominating Fraenkel-Conrat for the Nobel Prize.\footnote{Wendell Stanley to the Nobel Committee for Chemistry, December 22, 1960. Stanley Papers, Carton 5,
Folder 12, p. 4.} However, Stanley's purpose in this
letter was not to present a balanced account but to present his own colleague in the best
possible light, making it problematic as a historical source.\footnote{In her recent book *Who Wrote the Book of Life?* Lily Kay suggests that Stanley's letter to the Noble
Committee is a good source for understanding the scientific competition between the Berkeley and
Tübingen research groups. See note 142, p. 363.} For example, to strengthen
Fraenkel-Conrat's candidacy, Stanley attempted to establish clear priority for Fraenkel-
Conrat since his paper was received on January 20, just over three weeks before Gierer
and Schramm's paper was submitted on February 13. Stanley also wrote "The nucleic
acid he [Fraenkel-Conrat] found to be infectious and to carry all genetic information
required for its own replication and for that of its homologous protein."\footnote{Stanley to Nobel Committee, p. 1.} While
submitted earlier, Fraenkel-Conrat’s brief paper contained no data to verify his claim. He did not publicly present evidence until June of 1956, and at that point even expressed caution, saying “...our tentative conclusion at present is that the protein may after all have a minor role in the replication process.” Therefore, if we are to depart from Stanley’s concession that “the two researches were independent and essentially simultaneous,” the most accurate way to do so is to note that Fraenkel-Conrat was the first to make a credible public claim that viral nucleic acid was infectious while Gierer and Schramm were the first to provide evidence for such a claim.

Collectively the infectious RNA work helped solidify a new biological paradigm that identified nucleic acids as the genetic material. Since the research dealt with RNA, it complemented the Watson/Crick model of DNA. Furthermore, this experiment was the first case in which isolated nucleic acid of any kind had been used unambiguously to demonstrate biological activity, giving it a certainty that previous papers had lacked. Such certainty was very significant in the mid-1950s, when many researchers had not yet accepted that nucleic acids were the genetic material. Robert Olby argues that the infectious RNA work allowed Francis Crick to articulate the Central Dogma of molecular biology, in which biological information flowed from nucleic acids to proteins, but not the other way. In assessing the importance of the work years later,

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138 Fraenkel-Conrat presented his work at a symposium at Johns Hopkins University that took place between June 19-22, 1956. His talk was published as H. Fraenkel-Conrat, Beatrice A. Singer, and Robley C. Williams, “The Nature of the Progeny of Virus Reconstituted from Protein and Nucleic Acid of Different Strains of Tobacco Mosaic Virus,” in A Symposium on the Chemical Basis of Heredity, ed. William D. McElroy and Bentley Glass (Baltimore: The Johns Hopkins Press, 1957), 501-512. Of special interest is the discussion with Schramm that follows on pages 512-517.

139 According to Judson, “With these experiments, Fraenkel-Conrat and Schramm brought the viruses that contain RNA into the now all-inclusive family of organisms whose heredity is determined by their nucleic acids. They completed the grand sequence of experiments on the contrasting roles of nucleic acids and proteins that had begun with Oswald Avery.” Horace Freeland Judson, The Eighth Day of Creation: The Makers of the Revolution in Biology (New York: Touchstone Books, 1979), 300.

140 Olby, Path to the Double Helix, 434.
Crick himself said that the infectious RNA work was not "just dotting I's and crossing T's" but instead was significant fundamental research reinforcing a major shift in biological thought.\textsuperscript{141} Max Perutz said that it was "a good and decisive piece of work," because even in 1956, few people were convinced that nucleic acids really made up genes.\textsuperscript{142}

Others quickly recognized the significance of the work. At a symposium on viruses held in March, 1956, Robley Williams, also from Berkeley, presented some of Fraenkel-Conrat's findings. Upon hearing his report, Bawden replied, "Dr. Williams' statement that the nucleic acid on its own can infect is likely to be as important as any other statement we are going to get at this meeting. We urgently need some quantitative information so that we can assess the evidence for it."\textsuperscript{143} In 1958, Fraenkel-Conrat and Schramm shared the prestigious Lasker Prize (often referred to as America's Nobel Prize) for the discovery. As we have seen, Stanley thought the discovery worthy of the Nobel Prize, but not even his enthusiasm for his own candidate could cause him to dismiss completely the contributions of his German colleagues. He wrote "there would be no miscarriage of justice if the Prize should be divided [between Fraenkel-Conrat and Schramm]."\textsuperscript{144}

\textbf{Conclusion}

The position of the Tübingen TMV researchers within the international scientific community changed dramatically in the first decade after the war. The expulsion of Jewish scientists hurt the German research community as a whole but did not prevent the continuation of isolated pockets of quality research such as the TMV group. The

\textsuperscript{141} Telephone interview with Francis Crick, November 9, 2000.

\textsuperscript{142} Telephone interview with Max Perutz, August 8, 2000.

\textsuperscript{143} Wolstenholme and Millar, eds., \textit{CIBA Foundation Symposium on The Nature of Viruses}, 35.

\textsuperscript{144} Stanley to Nobel Committee, 4.
outbreak of war between Germany and the United States temporarily severed significant
ties between the Germans and the broader scientific community, hindering the ability of
the smaller German community to carry out research effectively. In contrast to the
aftermath of World War I, after the Second World War the German community was not
completely isolated, allowing the rebuilding of relations to resume quickly. Acceptance
of the Germans depended not only upon professional factors such as shared scientific
interests, but on personal relationships as well. The latter made the normalization of
relations with Jewish émigrés variable and in some cases difficult. The TMV researchers
remained aware of advances in the field and made significant contributions to the
ongoing revolution in biological thought by the mid-1950s. More than anything else it
was the significance of their work for all of biology that gained them recognition and
acceptance abroad. In the following years they built on this foundation by contributing to
the determination of the Genetic Code and identifying themselves with the newly
emerging discipline of molecular biology.
Chapter 5:
Tübingen and the Genetic Code, 1957-1966

Introduction

The Tübingen researchers followed the infectious RNA work with a decade of quality TMV research, much of which was directed toward determining the relationship between the sequence of bases in nucleic acids and the corresponding sequence of amino acids in a protein. Known as the Genetic Code, this relationship emerged as the most interesting and challenging problem in molecular biology after the confirmation of the Watson-Crick model of DNA. The successful solution of this last great problem in molecular biology was the result of a massive effort involving the work of dozens of labs worldwide focusing on diverse aspects of nucleic acids and protein synthesis. Within this overall effort, the contributions of the Tübingen researchers were decisive and significant.

The Genetic Code, conceptualized in terms of the transfer of information from nucleic acids to proteins, provided both the subject and the language that helped to forge the identity of the new field of molecular biology. Although many have retrospectively viewed molecular biology as the annexation of biology by physicists, the discipline actually emerged from the fusion of a number of research traditions, some dating back to the nineteenth century. New approaches based on physics and the information sciences helped provide the language of molecular biology, but traditions like biochemistry provided many of the tools of the new discipline.
The first generation of postwar researchers in Germany was assimilated into this English-speaking molecular biology community with an intimacy that their senior colleagues could not duplicate. The younger researchers spent more time abroad and were able to establish their careers as the molecular revolution in biology was taking place. Therefore, the West German research community looked very different in the late 1960s than it had in the immediate postwar era. The scientists spoke and wrote English, admired the American research style, and spent many of their formative years in American labs.

The new biology, couched in metaphors from the information sciences, appeared to the German public to be very different from previous studies of biology and inheritance. Given the centrality of biology to Nazi racial ideology, such a break with the past was a necessary part of the social acceptance of molecular biology in the Federal Republic of Germany. Here the Germans often differed from their Anglo-American colleagues, who spoke freely of the possibilities of applying molecular biology to problems of public health and heredity. Instead, the West German researchers tended to present their work in more abstract and ideal terms. Their caution in this respect suggests that overall, the society of the Federal Republic of Germany was reluctant to embrace technocratic solutions for social and biological problems.

5.1 TMV as a Model System

5.1.1 Induced Mutation

Melchers had been searching for a way to create TMV mutants since the 1930s, hoping that the study of biological variation among mutants would lead to the solution of genetic problems. The infectious RNA work showed that the proper target for these investigations was the viral nucleic acid, but the production of stable mutants, that is, mutants that could reproduce themselves, still faced a number of potential obstacles. Random disruptions, such as those caused by bombarding the nucleic acid with radiation,
were more likely to inactivate the delicate RNA molecule than to produce a viable mutant. In any case, the production of mutants in such a fashion would not necessarily reveal anything about the mechanism of mutation.\textsuperscript{1} A more promising alternative was chemical mutagenesis, since chemical reactions could be exactly repeated and their products analyzed. Mindful of the fragility of the RNA molecule, the researchers did not consider reactions requiring high pH values, high temperatures, or long reaction times.

Schramm and his co-workers began the search for an appropriate chemical mutagen by reviewing the activity of well-known substances with RNA.\textsuperscript{2} In the 1940s, Schramm (and Stanley as well), had found that nitrous acid (HNO\textsubscript{2}) inactivated TMV.\textsuperscript{3} When they renewed their investigations in the postwar years, the Tübingen researchers found that nitrous acid inactivated the RNA molecule without splitting it. When they analyzed the reaction products, they found that nitrous acid de-aminated three of the four nucleotide bases, adenine, guanine, and cytosine. (That is, it removed freely accessible amino, or NH\textsubscript{2} groups, from the bases.)\textsuperscript{4} They speculated that the de-amination of some

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\textsuperscript{2} Much of this work was actually done by Heinz Schuster, who had joined Schramm’s division in 1954. Schuster had been born on October 14, 1927, in the town of Eltville on the Rhine River just west of the city of Mainz, and his high school years were interrupted by two and a half years of military service and time in a POW camp. From 1947 to 1954, he studied chemistry at the University of Mainz, supporting himself by working in labs at both BASF and Höchst. His doctoral work was on the biochemistry of muscle contractions. Upon conclusion of his university studies, he began work as a research associate (“Wissenschaftlicher Mitarbeiter”) with Schramm in Tübingen, where he became interested in the structure, function, and mutability of nucleic acids. Biographical material on Schuster comes from his own Lebenslauf, MPG Archiv, II. Abt., Rep. 1A, MPI für Molekulargenetik/Schuster, and his obituary (Nachruf) by Thomas A. Trautner in Jahrbuch der Max-Planck-Gesellschaft (1997): 194-195.

\textsuperscript{3} Research on the reaction of nitrous acid with nucleic acids was quite old, having been pioneered by Albrecht Kossel in the late nineteenth century; see Kossel, K.M.L.A., “Über die Nucleinsäure,” Archiv Anat. Physiol (1893): 157-164. I would like to thank Karl-Wolfgang Mundry for informing me of the importance of Kossel’s work.

of the RNA bases, specifically the conversion of cytosine to uracil, a naturally occurring nucleotide base, might result in mutation instead of deactivation.5

Turning speculation into reality, however, required extensive testing of the treated RNA on different strains of tobacco plants, which was not a simple process. Such investigations required growing large amounts of tobacco under sterile conditions to prevent mixed infections, and therefore required extensive space in greenhouses that were impeccably maintained. The most challenging aspect of working with the virus in a host plant was estimating viral activity by counting and identifying lesions produced in the leaves of the tobacco, since the sensitivity of the tobacco to the virus can vary from plant to plant, and even from leaf to leaf on the same plant.6 Consequently, applying Schramm and Schuster’s results required a variety of skills, including knowledge of the biochemistry of nucleic acids, reaction kinetics, extensive training in plant virology, and practical experience identifying and comparing viral symptoms across various subspecies of tobacco. In order to pursue the experiment, Gierer worked with Karl-Wolfgang Mundry, who had recently returned to Tübingen as a research associate under Melchers at the Max-Planck-Institut für Biologie.7

Mundry had studied botany in Göttingen from 1946-1949 under the direction of Richard Harder. In 1947, Mundry encountered the term “virus” during a literature search

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6 These difficulties were discussed at length by Samuel G. Wildman, “The Process of Infection and Virus Synthesis with Tobacco Mosaic Virus and Other Plant Viruses,” in The Viruses... Volume II: Plant and Bacterial Viruses, see esp. 5-6.

in the Göttingen library, and his subsequent work with TMV was launched by a seemingly simple question to his adviser—"What is a virus?" Harder admitted that he did not know, but he suggested that Georg Melchers in Tübingen might. After a 600-km bike ride and a lengthy interview, Mundry secured a position with Melchers. In his doctoral work, as mentioned above, Mundry had attempted to increase the mutation rate in TMV, and the results had been inconclusive. After finishing his degree in 1954, he spent three years as head of the virology department of the Institut für Landwirtschaftliche Technologie und Zückerindustrie (Institute for Agricultural Technology and the Sugar Industry) at the Technical University in Braunschweig. In 1957, he accepted an offer from Melchers to return to Tübingen and began experiments on the entry of TMV into host cells.8

In 1957, Gierer and Mundry met in a seminar on the topic of mutagenesis in TMV. They discussed the possibility of using a tobacco strain known as Java (*Nicotinia tabacum*), which Mundry had used for his doctoral work, as a host plant. The advantage of this species was the way that it reacted to infection from the most common variety of TMV. Upon infection, the plant's leaves showed faint chlorotic (yellowish) spots, with a very few necrotic lesions (focal points of infection where the plant cells were actually destroyed by the virus, leaving dead spots in the plant tissue). Naturally occurring mutants of TMV tended to produce a much higher number of these necrotic lesions. Gierer and Mundry hoped that artificially created mutants might behave similarly, providing them with a way to determine if treatment with nitrous acid did in fact create genetically stable mutants. Everyone—Schramm, Melchers, Gierer, and Mundry—was

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skeptical. Nevertheless, the two younger scientists received encouragement and support, including space in Melchers' notoriously sterile greenhouses.\(^9\)

Gierer and Mundry exposed the common strain of TMV, as well as isolated RNA, to nitrous acid according to the procedures developed by Schuster and Schramm. The treated components, along with untreated virus (to serve as a control) were then applied to Java tobacco.\(^10\) The results were dramatic. Within three days, hundreds of necrotic lesions appeared on the leaves of the plants that had received the treated RNA. Two days later the leaves had collapsed entirely; the response was so overwhelming that the researchers thought they had accidentally used a different strain of the virus which normally produced this kind of necrotic lesion.

They then repeated the experiment under milder conditions to allow for a kinetic analysis of the reaction. This analysis, done by Gierer, determined that the nitrous acid altered approximately one nucleotide base per RNA molecule.\(^11\) Therefore, the removal of an amino group from a single nucleotide base was sufficient to create a mutant

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\(^9\) Quite unexpectedly, Melchers suggested that if the experiment were successful, Gierer and Mundry should publish without the names of the division directors listed as senior authors, as was common practice. Schramm agreed. The only condition was that the work was to be published in the *Zeitschrift für induktive abstammungs- und Vererbungslehre*, at the time the oldest genetics journal in the world. Melchers was the editor of the journal and was concerned because its circulation had declined precipitously and he hoped that publishing Gierer and Mundry's results would help to revive it. Mundry, "TMV in Tübingen," Interview with Karl-Wolfgang Mundry, Stuttgart, May 25, 2000; also see section 5.2.3, later in this chapter.


\(^{11}\) Gierer had already used reaction kinetics to make a number of important discoveries regarding the structure and function of the TMV RNA molecule. In 1957-1958, he demonstrated that TMV contained one loosely coiled, single-stranded molecule of RNA, and that the entire molecule was necessary to cause infection in a host plant. Alfred Gierer, "Structure and Biological Function of Ribonucleic Acid from Tobacco Mosaic Virus," *Nature* 179 (1957): 1297-1299; "Größe und Struktur der Ribosenucleinsäure des Tabakmosaikvirus," *Zeitschrift für Naturforschung* 13b (1958): 477-484; and "Die Größe der biologisch aktiven Einheit der Ribosenucleinsäure des Tabakmosaikvirus," *Zeitschrift für Naturforschung* 13b (1958): 485-488.
phenotype. From this result, the researchers surmised that the exact size of the smallest unit of genetic mutation was the single nucleotide base. Eventually, they determined the mechanism of the nitrous acid/RNA reaction and provided a precise understanding of the molecular basis of mutation for the very first time.

Other labs soon confirmed Gierer and Mundry's results. There was, however, some skepticism, hardly surprising given the significance of the claims of the two researchers. That the skepticism was from Frederick Bawden, long-time critic of non-English TMV research, was hardly surprising, either. In a 1959 paper in Nature, Bawden wrote that Gierer and Mundry had not proven that they had created mutants. Instead, Bawden argued that previously existing mutants of TMV that were resistant to the nitrous acid had been observed after other, less resistant strains, had been inactivated during the course of the experiment. Gierer and Mundry had in fact taken precautions against this contingency, and so Bawden's critique was without merit, but his position as one of England's leading plant virologists ensured publication of his work by England's leading scientific journal. Furthermore, since most of Gierer and Mundry's supporting data had

12 Gierer and Mundry, "Production of Mutants," 1458. For a summary of the Tübingen TMV work up to the late 1950s, see Alfred Gierer, "Recent Investigations on Tobacco Mosaic Virus," Progress in Biophysics and Biophysicsiology 10 (1960): 299-342.

13 The general effect of undissociated nitrous acid (HNO₂) with a nucleotide residue (R) and an amino group (NH₂) is as follows: HNO₂ + R-NH₂ yields R-OH + H₂O + N₂; that is, the amino group is substituted by a hydroxyl group.

The specific effects of the nitrous acid reaction on RNA are as follows:

<table>
<thead>
<tr>
<th>Base</th>
<th>Alteration:</th>
<th>Net Effect:</th>
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<tr>
<td>Adenine</td>
<td>Hypoxanthine</td>
<td>Converts A to G</td>
</tr>
<tr>
<td>Cytosine</td>
<td>Uracil</td>
<td>Converts C to U</td>
</tr>
<tr>
<td>Guanine</td>
<td>Xanthine</td>
<td>None (Xanthine replicates like Guanine)</td>
</tr>
<tr>
<td>Uracil</td>
<td>None (no NH₂ group)</td>
<td>None</td>
</tr>
</tbody>
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been published in an obscure German genetics journal, very few had access to the data that invalidated Bawden’s critique. To make matters worse, the editors of Nature refused to publish a detailed response by Mundry. He and Melchers then turned to Lindsey Black, one of the editors of the journal Virology, for help.\textsuperscript{15} Black quickly published Mundry’s reply to Bawden, and his willingness to take such measures indicated that he, too, felt that Bawden’s critique was unjustified.\textsuperscript{16} Heinz Fraenkel-Conrat agreed. After reading Mundry’s response in Virology, he wrote to Melchers that Mundry had been too mild, saying, “The stupidity of Bawden’s paper seemed enormous to me, and I have said so wherever I had a chance.”\textsuperscript{17}

5.1.2 Primary Structure

In 1957, in a talk before England’s Society for Experimental Biology, Francis Crick articulated two ideas that already existed, in less precise form, in the minds of many of the scientists working on nucleic acids and proteins. The first idea, called the Sequence Hypothesis, assumed that the sequence of bases in a nucleic acid determined the sequence of amino acids in the corresponding protein; at the time this idea was widely held. The same could not be said of the second point, which Crick called the Central Dogma. The Central Dogma stated that the flow of information in biological processes was unidirectional. Information, stored in the sequence of nucleic acids, could be transferred from one nucleic acid to another, or it could be transferred from a nucleic acid

\textsuperscript{15} Interview with Karl-Wolfgang Mundry, Stuttgart, May 25, 2000; Melchers to Lindsey Black, October 17, 1959; Black to Mundry, October 21, 1959. Both letters Melchers Papers, Ordner 14.


\textsuperscript{17} Heinz Fraenkel-Conrat to Georg Melchers, December 23, 1959. Melchers Papers, Ordner 15.
to a protein, but the opposite was not true. In Crick’s phrasing, “once ‘information’ has passed into protein it cannot get out again.” [Emphasis in original.]

This paper was doubly significant. On a practical level Crick established protein synthesis as the central problem of biological research. On a theoretical level, he established terms from cybernetics and linguistics as the language of the new biology. From the late 1950s onward, ideas that had previously been expressed in biochemical terms were increasingly couched in terms of information storage, retrieval, and transmission. Lily Kay has provided a fascinating account of the importance of images drawn from cybernetics and information sciences—in particular portraying the sequence of nucleic acids as a kind of language or code, DNA as an information storage device, and protein synthesis as the transfer of information—in providing theoretical coherence to the diverse set of practices that emerged as molecular biology in the late 1950s/early 1960s. Angela Creager’s study of TMV as an experimental model confirmed Kay’s findings. She wrote, “The assimilation of ‘information’ into research discourses and practices became part of the uneasy cohabitation of biochemists and self-described molecular biologists in the burgeoning world of biomedical research in the late 1950s and 1960s.” In the case of the Tübingen researchers, Christina Brandt has noted that the use of these images allowed the Tübingen researchers to associate their work with that of the Americans. All of these scholars ascribe considerable importance to metaphor as a

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conceptual tool as well as a way to negotiate meaning between groups with diverse backgrounds, in this case between various groups within the biomedical community.\textsuperscript{21} By replacing an older language with one from the new field of cybernetics, investigators in the developing field of molecular biology identified themselves and established their own disciplinary space. Workers in labs around the world began to think of themselves as molecular biologists and to conceive of protein synthesis as the transfer of genetic information.\textsuperscript{22}

The sequence of amino acids in a protein, known as the protein’s primary structure, determines the three dimensional shape of the protein and therefore the protein’s biological function.\textsuperscript{23} The Sequence Hypothesis implied that determining the primary structure of a protein could also be used as a first step toward mapping the protein back to its nucleic acid gene. Techniques for determining the amino acid sequence of proteins were already in existence, having been pioneered through Frederick Sanger’s successful sequencing of the hormone insulin in 1953. Workers in Schramm’s division had been at work on the primary structure of TMV since the mid-1950s, as had Wendell Stanley and his colleagues in the Berkeley virus lab.\textsuperscript{24} In the late 1950s,

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for impressing upon me the importance of information science metaphors in the discourse of the Tübingen researchers during conversation in Berlin in April, 2000.
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\textsuperscript{21} According to Lakoff and Johnson, “Metaphorical imagination, is a crucial skill in creating rapport and in communicating the nature of unshared experience.” George Lakoff and Mark Johnson, \textit{Metaphors We Live By} (Chicago: University of Chicago Press, 1980), 231.

\textsuperscript{22} Kay, \textit{Who Wrote the Book of Life}, 244.

\textsuperscript{23} Schramm’s student F. Alfred Anderer demonstrated this experimentally in 1959. Anderer denatured the protein (broke the various chemical bonds giving it its three-dimensional shape) and showed that the resulting amino-acid chain regained its original three-dimensional form of its own accord. Stanley praised this work as “the first clear-cut demonstration” that the shape of the virus protein depended entirely on the sequence of its amino acids. F. Alfred Anderer, “Reversible Denaturierung des Proteins aus Tabakmosaikvirus,” \textit{Zeitschrift für Naturforschung} 14b (1959): 626; Stanley’s comments from letter, Wendell Stanley to Hans H. Weber, December 12, 1966. MPG-Archiv, II. Abt., Rep. 1A, Anderer/Virusforschung.

\textsuperscript{24} See Chapter 4, section 4.3.1. Determination of the end group amino acid residues of the coat protein was in truth the first step toward determination of the entire amino acid sequence of the protein.
Schramm’s interests turned more toward exploring the pre-biotic origins of life, and F. Alfred Anderer assumed leadership of protein sequencing efforts in Schramm’s division. Anderer had studied chemistry at Tübingen University before joining Schramm in 1953. His doctoral work, completed in 1957, concerned the structure of TMV coat protein, and Schramm encouraged him to pursue its primary structure. By this time, the Tübingen and Berkeley teams had shown that TMV coat protein contained approximately 2,200 peptide chains and that the sequences at either end were identical for all the chains. Both labs then began to work their way inward from the two known terminal sequences.

These two labs were soon joined by a third, this one in Tübingen’s Max-Planck-Institut für Biologie. There Heinz-Günter Wittmann, who worked in Melchers’ division, had become interested in TMV protein structure as well. Born in 1927 in East Prussia, Wittmann’s high school education was interrupted when, at the age of 17, he was drafted into the Wehrmacht. He was interned in a POW camp and released after the war, when he finished his high school education and began a practical study in agriculture with hopes of returning to his East Prussian homeland. The division of Germany made a return impossible; however, Wittmann’s agricultural studies stimulated his interest in biology, which he then studied at the University of Tübingen in the early 1950s. He completed his doctoral work on phage genetics under the direction of Weidel, after which Wittmann spent the 1956-1957 academic year as a postdoc in Berkeley’s Virus Lab.

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26 The steps involved in sequencing the TMV protein were articulated by Fraenkel-Conrat in 1957 and were generally applicable to sequencing efforts in both Berkeley and Tübingen. The steps were as follows: 1) Degrade the protein enzymically; 2) Separate fragments from degradation; 3) Determine composition and sequence of these smaller fragments; 4) Fit sequences of fragments together on basis of overlap supplied by using different enzymes to produce different sets of fragments. Heinz Fraenkel-Conrat and K. Narita, “Degradation and structure of tobacco mosaic virus,” in Symposium on Protein Structure, ed. Albert Neuberger (New York: John Wiley & Sons, Inc., 1958), 256-259.

When he returned to Tübingen to continue his work under Melchers, Wittmann established his own independent TMV group in the Institut für Biologie. Although Melchers had encouraged work between his own division and those in the Institut für Virusforschung, he did not want to become too dependent on the biochemical expertise of Schramm’s division. He therefore sent Wittmann to Butenandt’s Institut für Biochemie to learn protein sequencing from Gerhard Braunitzer, who had moved to Munich with Butenandt in 1956. Wittmann brought an extraordinary energy, persistence, and organizational skill to this work. He built two amino acid sequence analyzers from simpler parts and used them to complement the lab’s two commercial sequencers. By 1960, the Max-Planck-Institut für Biologie had amino acid sequencers running twenty-four hours a day. To assist Wittmann, Melchers also attempted to interest Brigitte Liebold, Braunitzer’s best sequencer, in coming to Tübingen.

The story of how Melchers’ Institute eventually acquired Liebold’s skills is a fine illustration of the importance of non-scientific factors in the human process of science. Her doctorate was in chemistry, and initially she was not interested in leaving the

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28 F. Alfred Anderer recalls that there was some tension between the two Tübingen Max-Planck-Institutes by the late 1950s. This may have resulted from personal differences between the directors, especially Melchers and Schramm, for there is evidence that Schramm attempted to take undue credit for Gierer and Mundy’s nitrous acid work and that this annoyed Melchers. On January 5, 1959, Dr. Karl Maramorosch wrote to Melchers regarding a lecture Schramm had recently given at the Rockefeller Institute in New York. Maramorosch was already aware of the nitrous acid work and was dismayed to hear Schramm present it as though it was his own research. Maramorosch wrote “At first I felt astonished, then embarasssed [sic], and finally rather angry... Later I did my best to inform my colleagues of other Max-Planck-Institutes, particularly of one where Mundy works, and also of the existence of Gierer-like coworkers of Schramm.” Melchers acknowledged that such appropriation of credit happened quite often (perhaps referring to the fact that Schramm won a share of the Lasker Prize and Gierer had not?) but that cooperation was still the best way to achieve good results. He did indicate that this case was particularly upsetting because the younger researchers, who were trying to build up their reputations, would not get the credit they deserved. This may have convinced Melchers that his own TMV researchers would be better served by remaining independent. Maramorosch to Melchers, January 5, 1959; Melchers to Maramorosch, January 9, 1959. Both letters Melchers Papers, Ordner 13.

29 Melchers wrote to Butenandt that he had never met anyone who could carry out such a program with the efficiency of Wittmann. He described Wittmann’s organizational talents as “truly extraordinary” (wirklich ganz ungewöhnlich). Melchers to Butenandt, November 18, 1960. Melchers Papers, Ordner 15.
Biochemistry Institute in Munich for a Biology Institute in Tübingen. When Wittmann came to work in Braunitzer’s lab, he and Liebold soon became romantically involved. Consequently, in the autumn of 1960, Brigitte Liebold, soon to be Brigitte Wittmann-Liebold, joined Melchers’ division in the Institute for Biology. That November, Butenandt wrote to Melchers to congratulate him and to express amusement at the way his need for a talented protein sequencer had been fulfilled.\footnote{Butenandt to Melchers, November 15, 1960. Melchers Papers, Ordner 15.} Thanks to the abilities of the Wittmanns, there were now two first-rate protein-sequencing labs in the small city of Tübingen.

In 1959, Wittmann and Braunitzer had split the TMV coat protein into twelve unique, identifiable fragments. They then determined the composition (but not the sequence) of these fragments and suggested that the complete protein contained 157 amino acids.\footnote{H.G. Wittmann and G. Braunitzer, “Isolation and Composition of All Tryptic Peptides of TMV,” Virology 9 (1959): 727-728.} Anderer and his colleagues published their first effort in early 1960; their work was significant because they identified the sequence of the amino acids within one of the polypeptide fragments, not just the amino acid content of the fragment. Later that year they published a preliminary draft of the entire primary structure of the TMV coat protein.\footnote{F.A. Anderer, E. Weber, and H. Uhlig, “Die Reihenfolge der Aminosäuren im Protein des Tabakmosaikvirus I: Die N-terminale Aminosäurefolge von Position 1-41,” Zeitschrift für Naturforschung 15b (1960): 79-81; F.A. Anderer, H. Uhlig, E. Weber, and G. Schramm, “Primary Structure of the Protein of Tobacco Mosaic Virus,” Nature 186 (1960): 922-925.} Shortly afterward, the Berkeley group published its own sequence, correcting that of the Germans in several areas. Most notably the Berkeley group had found that the protein contained 158, not 157 amino acids.\footnote{For a concise review of the Berkeley group’s efforts (that also gives full credit to the contributions of the German researchers) see Heinz Fraenkel-Conrat, “Contributions from TMV Studies to the Problem of Genetic Information Transfer and Coding,” in Molecular Genetics. Part 1, ed. J. Herbert Taylor (New York: Academic Press, 1963), 484-7. The best secondary account is Creager, Life of a Virus, Chapter 7.} Stanley wrote to inform the Tübingen
researchers of his group's findings prior to their publication and indicated that he did not think the discrepancies between the findings of the two labs were significant. Schramm agreed and suggested that the labs exchange samples if they continued to have trouble reaching a consensus.  

By 1962, the German researchers had largely accepted the findings of the Berkeley group. There were several amino acids in the sequences proposed by both Berkeley and Tübingen that had not been positively identified, but since none of these uncertainties was common to both sequences, researchers in both cities could conclude with confidence that they had determined the entire amino acid sequence of the protein. In fact, such was the agreement between the groups in Germany and America that Stanley and Schramm had even discussed publishing a final joint paper on the subject. The persistence of minor discrepancies precluded such a publication. When improved techniques made a more definitive analysis possible, it was published as a joint project, but one between Anderer and the Wittmanns, not Tübingen and Berkeley.

5.1.3 Tübingen and Berkeley

The relationship with Berkeley had become the most important connection between Tübingen and the American scientific community. By the early 1960s, it had

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matured into a relationship of equals, very different from the late 1930s and immediate postwar years when Stanley’s word had simply been accepted as the gospel. In 1961 Stanley could say, “A continuous, close, and cordial collaboration exists between Tübingen and Berkeley.”38 Rudolf Rott, who worked in the Institut für Virusforschung under Werner Schäfer in the late 1950s, remembers that the Berkeley/Tübingen relationship was extremely important for both labs and helps account for the excellence of research done in both cities.39

The Berkeley/Tübingen relationship illustrates the importance of the simultaneous existence of cooperation and competition within the TMV community. Cooperation between Berkeley and Tübingen was widespread and generous. Not only did the researchers exchange reprints and materials—they also shared experimental data and techniques, often in advance of publication.40 This interaction brought the talents of a number of researchers with different backgrounds and skills together to tackle problems of common interest and made the combination of the three labs much more effective than any of them alone would have been. From the German perspective, the situation was a marked improvement over the relative isolation of the war years and allowed them to broaden their intellectual horizons considerably.41

38 Stanley’s comments are at the end of Gerhard Schramm’s article “Mutagenesis in Viruses,” 44.


40 This is best illustrated by an exchange of letters between Melchers and Fraenkel-Conrat in 1959; Fraenkel-Conrat to Melchers, July 3, 1959; Melchers to Fraenkel-Conrat, August 27, 1959; Fraenkel-Conrat to Melchers, September 30, 1959, and Melchers to Fraenkel-Conrat, October 17, 1959. All letters Melchers Papers, Ordner 14.

41 Fraenkel-Conrat also suggested that the success in the sequencing of the TMV protein was due to the fact that multiple labs were competing with one another to complete the sequence. Heinz Fraenkel-Conrat, “Protein Chemists Encounter Viruses,” in The Origins of Modern Biochemistry: A Retrospective on Proteins, ed. P.R. Srinivasen, Joseph S. Fruton, and John T. Edsall (New York: New York Academy of Sciences, 1979), 311.
Naturally, the investigators in all three labs competed as well. All wanted to gain prestige and credit for themselves at the expense of the others, which only added to the thoroughness with which they pursued their research. Stanley’s Nobel Prize nomination letter for Fraenkel-Conrat demonstrates just how important individual rather than shared credit was, especially when two labs had a claim to the same work. The investigators also did not hesitate to correct one another when necessary and over the years did so with varying degrees of courtesy and professionalism.

Between the three labs relationships varied, and professional respect was often, but not always, accompanied by personal affection. Stanley was closest with Schramm; although their correspondence was usually rather formal, on occasion Stanley addressed his German colleague with a warm familiarity. For example, after suffering a broken hip, Stanley conveyed his admiration for Schramm, who had previously suffered a similar accident while skiing. Stanley wrote:

Dear Gerhard;
You have been on my mind quite a bit during the past month for a reason that you would hardly suspect. Just a month ago I had an unfortunate accident on the stairs at the Faculty Club which resulted in a broken hip...During the time that I was in the hospital I had a rather thorough realization of what you must have gone through, and I must say that my admiration for your fortitude now knows no bounds.\(^{42}\)

In contrast to Stanley, Fraenkel-Conrat tended to be closer to the researchers in Melchers’ Division, particularly with Wittmann and Mundry. Wittmann’s year in Berkeley undoubtedly was a factor here.

There was, however, clear animosity between Fraenkel-Conrat and Schramm, which had existed at least since the mid-1950s.\(^{43}\) Melchers’ first instinct was to blame


\(^{43}\) Interview with Thomas and Barbara Hohn, Basel, July 19, 2000.
the bad relations between the two men on Schramm’s past membership in the SS. Fraenkel-Conrat was, after all, a Jewish émigré. Anton Lang, Melchers’ good friend on the West Coast, did not think that this was the case, and told Melchers that Schramm’s recent visit to Berkeley had been a pleasure for all involved (of course, during Schramm’s visit Fraenkel-Conrat had been conspicuously absent). Gunther Stent, himself a Jewish émigré from Berlin, wrote to Max Delbrück that Fraenkel-Conrat had not even been aware of Schramm’s Nazi past until Stent told him in about it in 1957. Fraenkel-Conrat himself later described the relationship as follows: “Schramm’s lab continued to do good work in our shared field, and over [added in pencil, ‘over the years’] I felt like, I presume, tennis stars do as they meet and compete again and again at various sites of the world. So one might say: We and Gerhard Schramm and his colleagues were friendly enemies.”

The two men had very different personalities—Schramm was extremely outgoing and confident in himself, while Fraenkel-Conrat tended to be introverted and more of a loner. The two had disagreed with one another on the end-group analysis of the TMV polypeptides and disputed the priority of the infectious RNA work. Before 1956, Schramm believed that Fraenkel-Conrat did not take his work seriously, and Schramm later publicly questioned the merit of Fraenkel-Conrat’s elegant reconstitution experiments. The two men therefore had sufficient personal and professional reasons to dislike one another, in addition to any historical factors that may have come into play.

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46 Anton Lang to Melchers, June 26, 1956. Melchers Papers, Ordner 9. Apparently, Wendell Stanley had some lingering doubts about Schramm’s talents as well, at least up through the mid-1950s. According to one of Schramm’s colleagues, during his 1956 visit to Berkeley, Stanley asked him to repeat the infectious
5.1.4 Comparative Sequencing

Once these investigators had determined the amino acid sequence of one strain of TMV, they began to explore the possibility of comparing the sequences of different strains to see if such a comparison revealed anything regarding the relationship between nucleic acid and amino acids. There are four different nucleotide bases in a given type of nucleic acid and twenty different amino acids in proteins, and since nucleic acids direct the synthesis of proteins, there exists a relationship in which the set of four bases specifies individual amino acids. Because of the discrepancy in numbers, more than one nucleic acid base must correspond to each individual amino acid. By the late 1950s, many researchers favored triplets of nucleotides as the basic unit of the Genetic Code, since in any triplet there are 64 possible combinations of bases, more than enough for a unique correspondence with the twenty amino acids. Whether or not different triplets might code for the same amino acid was an open question. Researchers were also unsure if the triplets might overlap. That is to say, some thought that it was possible that the last base of one triplet could simultaneously serve as the first base of the next triplet. Crick

RNA work entirely. Schramm apparently felt as though he was being examined, and was not fully accepted until he could replicate the experiment and talk his Berkeley colleagues through the entire process. Interview with Klaus Munk, Heidelberg, July 20, 2000. Schramm called Fraenkel-Conrat’s reconstitution experiments into question at a 1956 symposium on biological heredity. See Heinz Fraenkel-Conrat, “The Nature of the Progeny of Virus Reconstituted from Protein and Nucleic Acid of Different Strains of Tobacco Mosaic Virus,” in A Symposium on the Chemical Basis of Heredity, ed. William D. McElroy and Bentley Glass (Baltimore: Johns Hopkins University Press, 1957), 501-512. Schramm’s comments follow on 512-514.

47 George Gamow published one of the first coding schemes in 1954. It was based on structural features of DNA that later proved to be mistaken; nevertheless, the basic idea of his paper, that nucleotide triplets coded for individual amino acids, that the triplets overlapped one another, and that the code was degenerate with multiple triplets coding for the same amino acid, continued to influence researchers such as Crick until the late 1950s. George Gamow, “Possible Relation between Deoxyribonucleic Acid and Protein Structure,” Nature 173 (1954): 318.

48 There are two possible kinds of overlapping code. In a strongly overlapping code, each triplet would share two bases; allowing the letters A, B, C, and D to stand for the four bases, in such a code the sequence BCACDDA would code for the successive triplets BCA, CAC, ACD, CDD, DDA. In a partially overlapping code, where successive triplets share one base, the same sequence would code for the triplets BCA, ACD, DDA.
was not one of them, and in 1957 he and his colleagues argued for a Genetic Code in which the basic unit was the non-overlapping nucleotide triplet.49

Crick’s model was the starting point for Heinz-Günter Wittmann’s own investigations into the Genetic Code. Wittmann found Crick’s suggestion idea persuasive, but demonstrating it experimentally was exceedingly difficult. The most direct solution would have been to sequence the nucleic acids of relatively simple organisms, then to sequence their proteins, and then attempt to match the two. There was, however, no reliable technique for nucleic acid sequencing in the late 1950s, nor was it clear that all the nucleic acid in an organism’s genome represents proteins. Researchers therefore had to proceed along indirect paths.50 In 1958, the nitrous acid mutants created by Gierer and Mundry provided just such a path for Wittmann, and within a year he was exploring the possibility of using differences between TMV mutants as a way of determining elements of the Genetic Code.51 The German TMV researchers had finally found a way to use variation in TMV to approach significant biological problems.

Gierer had used reaction kinetics to show that the nitrous acid reaction corresponded with one de-amination, or “hit,” per strand of RNA. The mutants produced through this procedure would contain only one RNA change and therefore at most one


corresponding amino acid exchange. Wittmann and his co-workers hoped that by comparing the amino acid sequence of different TMV strains they would be able to find specific instances where one amino acid replaced another. Since they already knew that nitrous acid altered RNA by substituting guanine for adenine and uracil for cytosine, they would then be able to map RNA substitutions to amino acid exchanges.

To further this goal they launched a “100 variants” program for the year 1961. They hoped to identify at least a hundred different naturally occurring and nitrous-acid mutants and to begin a comparison of their primary structures. Unlike the previous work by Gierer and Schramm, or by Gierer and Mundry, this project did not entail any dramatic new insights or clever experimental breakthroughs—instead, it depended on hard, repetitive, yet precise, work. Correctly sequencing the primary structure of one strain of TMV had occupied three labs for the better part of a year. Expanding the program one hundred fold required that the experience gained to that point had to be combined with flawless sequencing techniques, first rate organizational talent, and persistence bordering on the outright stubborn. Furthermore, the correct identification of mutant viruses and their isolation from other strains prior to analysis required the very best skills in handling tobacco plants and extensive space in sterile greenhouses. The Wittmanns and their colleagues were the only group to possess all of these abilities.


53 Melchers to Adolf Butenandt, November 18, 1960. Melchers Papers, Ordner 15.

54 Lack of attention to proper controls in isolating and characterizing mutants sometimes plagued the Berkeley group. On at least one occasion, they isolated what they thought was a new mutant, which in fact was a strain they had already isolated and identified themselves. They continued for some time to work thinking that what was really the same virus was two different strains. This incident, and the controls that the German workers used to avoid such problems, was discussed in H.G. Wittmann, “Proteinuntersuchungen an Mutanten des Tabakmosaikvirus als Beitrag zum Problem des Genetischen Codes,” Zeitschrift für Vererbungslehre 93 (1962): 491-530.
The discipline and hard work of the group first began to pay dividends in the late summer of 1961. At the Fifth International Congress of Biochemistry held in Moscow that August, Wittmann was able to present some preliminary conclusions, even though the complete sequence of the common strain of TMV was still incomplete. For his evidence, he relied upon the analysis of amino acid exchanges in the polypeptide subunits of 100 strains of TMV, some of which were spontaneous mutants and some of which were produced through the nitrous acid method. Not all of the mutants showed amino acid differences, but those that did were consistent with the triplet model proposed by Crick, allowing Wittmann to offer tentative experimental support for Crick’s suggestion.\(^{55}\) Wittmann’s research also corroborated one of the most exciting papers presented at the meeting, that of Heinrich Matthaei and Marshall Nirenberg.\(^{56}\)

\(^{55}\) H.G. Wittmann, “Studies on the Nucleic Acid...,” 249. Although space prevents a detailed discussion of Wittmann’s strategy, in essence it was as follows. Since only two of the four bases were affected by nitrous acid, and each could only change to one different base (and they could not revert) the number of possible changes for any triplet of bases was actually quite limited, and the experimental evidence fell into the narrow limitations necessitated by Crick’s theoretical model.

\(^{56}\) Heinrich Matthaei was a German postdoc who had been awarded a NATO Fellowship to work in America from late 1960 until early 1962. In the US, he worked as an assistant for Marshall Nirenberg at the National Institutes of Health in Bethesda, Maryland. The two were working on cell physiology by using radioactive amino acids to trace the process of protein synthesis in a test tube. In May of 1961, while Nirenberg was visiting Berkeley, Matthaei added the synthetic nucleic acid poly-u, which consisted of only repeating uracil nucleotides, to the system. To his surprise, it facilitated the production of a protein consisting of only repeating phenylalanine amino acids—he had shown a clear correlation between a specific amino acid and a specific sequence of nucleic acid for the very first time. When presented in Moscow, the experiment caused an incredible commotion since it presented a direct strategy for approaching the Genetic Code. If other short, precisely known stretches of nucleic acid could be produced, they too could be used in cell-free protein synthesis experiments as probes for specific amino acids. Lest in the excitement, however, was the support given to Nirenberg and Matthaei’s research by that of Wittmann. Wittmann had shown that certain amino acids could be converted into phenylalanine by the nitrous acid reaction. This indicated that the nucleic acid sequence that coded for phenylalanine must contain either guanine or uracil. Though neither complete nor definitive, this corroborated Matthaei’s experiment and was very convincing because it was produced independently in a completely different system. Although he was in California at the time of the crucial experiment, Nirenberg was eventually awarded a share of the 1968 Nobel Prize for physiology or medicine for the poly-u work. Although it is standard practice for the head of a lab to take credit (or blame) for the work produced by it, a rift developed between the two men; Matthaei felt betrayed, and made this very clear to his colleagues when he returned to Germany. Interview with Heinz Schaller, Heidelberg, July 21, 2000. The best secondary accounts of the work of the two men are Lily Kay’s Who Wrote the Book of Life, 246-256, and Horace Freeland Judson’s The Eighth Day of Creation, 470-89. For Matthaei’s own recollections, see “Heinrich Matthaei im Gespräch.” Interview between Heinrich Matthaei and Ralf Hahn in Forschung und Technik in Deutschland nach 1945, ed. Peter
Wittmann published a more thorough discussion of his group's work in 1962. By that time, they had analyzed 117 mutants among which they had found 41 amino acid exchanges. Analysis of the exchanges allowed the investigators to reach several conclusions regarding the Genetic Code.\(^{57}\) They produced a rough estimate of the size of the active portion of the RNA, which supported Crick's finding that the Code was indeed a triplet-based code.\(^{58}\) They also provided evidence that the Genetic Code was non-overlapping.\(^{59}\) Their findings showed that the Code was degenerate for at least some amino acids, and that about eighty per cent of the rough correlations produced in their lab were compatible with those produced through cell free systems similar to those used by Nirenberg and Matthaei. In short, the Wittmanns and their co-workers answered questions about the general nature of the code, produced some specific correlations, and confirmed the work of several other labs.

Wittmann also noted that the TMV work was extremely limited in some respects. The most obvious limitation was the nature of the nitrous acid reaction that made the

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58 In 1961, Crick and co-workers had used mapping experiments on phage to provide evidence for a triplet code, but the possibility existed that the code might actually be composed of multiples of triplets; Wittmann's work showed that this was unlikely. In return, Crick's paper cited the work done by Wittmann as well as the Berkeley lab as showing that the Code was non-overlapping. The next year Wittmann used Crick's work to support his own, demonstrating the extent of interdependency between different labs in solving the coding problem. F.H.C. Crick, Leslie Barnett, S. Brenner, and R.J. Watts-Tobin, "General Nature of the Genetic Code for Proteins," Nature 192 (1961): 1227-1232.

59 Karl-Wolfgang Mundry remembers that Fraenkel-Conrat had difficulty understanding problems from a genetic rather than a biochemical perspective, since Fraenkel-Conrat had no training in genetics. This led him to suggest that work with TMV mutants showed that the Genetic Code was overlapping when the Wittmanns had provided a significant body of data showing that it was not. Interview with Karl-Wolfgang Mundry, Stuttgart, May 25, 2000; Heinz Fraenkel-Conrat, "Protein Chemists Encounter Viruses," 315. Also see Melchers to Dr. K.G. Grell, June 7, 1962. Melchers Papers, Ordner 17.
comparative sequencing possible—it affected only two bases, which meant that triplets that did not contain adenine or cytosine (there were eight possible) could not possibly be explored with this strategy. One is therefore tempted to ask why he and his colleagues spent so much time and effort pursuing it when they could not hope their work would be definitive. The answer is straightforward: from the beginning, they knew that the comparative sequencing work would complement and confirm the research of other investigators. The Wittmanns and their colleagues had no illusions that they would be able to crack the Code on their own. They knew that it could be solved only by a cooperative effort utilizing a number of techniques, of which TMV was only one.

Ultimately biochemical techniques provided the evidence for determining the Genetic Code. The Code, completed in the mid 1960s, showed none of the eloquence or sophistication that many theorists had envisioned. In fact it was not a code at all—it was a systematically degenerate set of correlations between nucleic acids and amino acids, nothing more. Since it had arisen over millennia through the contingencies of evolution, it is hardly surprising that the systematic approaches of the theorists failed. Many investigators who had trained in physics imagined that the Code would be graceful and elegant. Influenced by the new discipline of information sciences and working in a Cold War environment where information, coding, and secrecy were vital to national security, they conceived of the Code as a human might design it—from the top down. Nature, however, does not work in such a fashion, and the Genetic Code had emerged from the bottom up in a practical, functional, and yes, sloppy, manner. It was at once simpler and less elegant than many had anticipated.\(^6^0\) The empirical approaches of biochemistry, particularly the thorough, repetitive work of Wittmann and his colleagues, were certainly

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\(^6^0\) For example, Max Delbrück later recalled that the Code turned out to be much simpler than what he had originally envisioned. California Institute of Technology Oral History Project, “Interview with Max Delbrück.” By Carolyn Harding. Pasadena, California, Caltech Archives, 1979.
not the most elegant or graceful scientific approaches, but they were appropriate for a problem that itself was neither elegant nor graceful.

The comparative sequencing work and the contributions it made toward determining the Code earned universal acceptance in the expanding molecular biology community for Heinz-Günter Wittmann and Brigitte Wittmann-Liebold. In 1963, they presented their findings at the Cold Spring Harbor Symposium on Quantitative Biology.\textsuperscript{61} Three years later, when the Code was largely complete, the annual Cold Spring Harbor Symposium was entirely dedicated to it; Crick introduced the meeting and in his address mentioned the importance of TMV on two separate occasions.\textsuperscript{62} Later, the Wittmanns presented a polished retrospective of their own contributions to cracking the Code. While these contributions were testimony to the importance of TMV, the fact that overall the symposium had more than 300 participants, ninety-four presentations, and thirteen panels, helps put the TMV research into perspective. Determining the Genetic Code was a truly massive international effort that had unified a vast network of labs and techniques within the rubric of the new field of molecular biology.

5.2 \textit{The Emergence of Molecular Biology}

5.2.1 The Fusion of Disciplines

The significance of the work on the Genetic Code was immediately recognized with science's top honor—it earned the Nobel Prize in Physiology or Medicine for three labs in 1968.\textsuperscript{63} Members of all three of these labs worked in a tradition that was very


\textsuperscript{63} Nirenberg shared the prize with Har Gobind Khorana, whose research led to the creation of defined sequences of nucleic acid, and Robert Holley, whose lab had sequenced transfer RNA.
much biochemistry, and their importance in solving the most important problem facing 
molecular biology marks the widespread acceptance of biochemistry as an integral part of 
the newly emerging field of molecular biology. In Lily Kay's eloquent phrasing, "The 
semi-permeable boundary between biochemistry and molecular biology had become 
early porous..." The exchange across this boundary was two way: biochemists 
provided many of the experimental techniques that led to the determination of the Code, 
while the self-described molecular biologists contributed a language based on 
information theory and cybernetics. By the late 1960s, "molecular biologists," though a 
very diverse lot, were speaking a common language.65

Despite the importance of biochemistry, its contributions were largely ignored in 
early historical work on molecular biology, much of which was written by scientists 
involved in Delbrück's phage group. Since both phage genetics and biochemistry were 
essential elements of the past of molecular biology, the relatively late inclusion of the 
latter into the history of molecular biology is curious. The most persuasive explanation is 
that although investigators in both were seeking to answer the same questions, only the 
phage biologists saw themselves as doing something fundamentally new.66

The visible influence of the physicists and the striking breakthroughs of the 1950s 
and 1960s justify defining the period as a revolution in biology.67 However, these factors

64 Kay, Who Wrote the Book of Life, 273.

65 The appropriation of the information science discourse by the biochemistry community is a central 

66 While the importance of personnel and techniques from physics should not be understated, Keller has 
argued that the most important element of physics imported into molecular biology was prestige. Physics 
provided the social authority necessary to justify the way that molecular biology conceptualized life—as 
instructions or information encoded in genes rather than a property of the organism itself. Evelyn Fox 
Keller, "Physics and the Emergence of Molecular Biology: A History of Cognitive and Political Synergy," 
Journal of the History of Biology 23 (1990): 389-409. Note 1, p. 489 provides a good summary of the 
literature on physics and molecular biology.

have less justifiably resulted in a tendency among historians to ignore continuity in research programs in favor of new developments. The tendency to focus on the new brought phage to the attention of the historical community before biochemistry, but as we have seen, both were essential parts of the past. Although separate in the 1940s, the shift to a nucleic acid version of the gene gave the phage group a point of connection with other disciplines. By shifting focus from an experimental object to a class of molecules, nucleic acids, it opened up the possibility of cooperation between the stodgy biochemists and the brash newcomers from physics. The same shift from organism to molecules occurred in the TMV group, but became obvious only when the various groups in the molecular biology community began to speak the same language. Biochemistry did not come late to molecular biology, but it did come late to the language of molecular biology.

Both biochemistry and phage genetics were part of a tradition that had marked the life sciences since the late 1800s—the desire to see more deeply into organisms in order to understand their similarities rather than their differences. Consequently, researchers transferred their allegiance from the historical explanations that had dominated nineteenth century botany and zoology to rigorous experimental investigation of biological processes. As nineteenth century biology moved from the field to the laboratory, it began to be characterized by ideals such as “precise delineation of organic phenomena; experimental control over those phenomena; aspiration toward prediction of phenomena”—all of which remained as hallmarks of the biology of the twentieth century. In order to see deeper, to the molecular level, biologists needed new ways of

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68 One cannot refer to revolutions in scientific thought without discussing them in relation to the progression sketched out by Thomas Kuhn’s *The Structure of Scientific Revolutions*. Kuhn’s ideas have a great deal of merit as well as some drawbacks for conceptualizing this period in biology—I will discuss this at length in the conclusion.

seeing. Many of these new ways of seeing, including the use of phage as a model system or the use of x-ray crystallography to discern the structure of large molecules like DNA and proteins, were introduced by physicists. Other new ways of seeing, such as protein sequencing, were derived from the biochemical research tradition.\textsuperscript{70}

The greatest breakthrough in molecular biology research has been a kind of organizational style rather than any one technique or tool. Max Delbrück argued that the accumulation of scientific knowledge over previous centuries had made it simply impossible for any one person to master all the knowledge necessary to make fundamental advances in the physical sciences—specialization was a consequence of the successful accumulation of modern scientific knowledge. Therefore, collaboration between people with different disciplinary backgrounds was essential for the further progress of science.\textsuperscript{71} In the case of molecular biology, specialization bought control and simplicity, but at the cost of comprehensiveness. In order to keep biology from fragmenting into mutually incomprehensible sub-disciplines, the reductionism of laboratory biology had to be complemented with the cooperative networking of specialists.\textsuperscript{72}


\textsuperscript{72} In Angela Creager's words, "The production of knowledge in contemporary experimental biology depends on systems that link together instruments, techniques, materials, specialized reagents, and skilled practitioners." \textit{The Life of a Virus}, 318. More generally, molecularization as a technical process of reduction in the life sciences has been accompanied by a social process of molecularization—namely, the "formation of networks of strategic alliances oriented toward the characterization and deployment of molecules in science and medicine." See the collection of essays edited by Soraya de Chadarevian and
Interpreting the history of molecular biology as the fusion of disciplines, rather than the abrupt overthrow of stodgy old biochemists by swashbuckling molecular biologists, is consistent with the German researchers' own understanding of the development of the field. In a radio address entitled "Molecular Biology as the Result of the Cooperation of Classical Disciplines of the Natural Sciences" ("Die Molekulare Biologie als Ergebnis der Zusammenarbeit klassicher Fächer der Naturwissenschaft"), Melchers argued that the central goal of older fields such as biochemistry or physiological chemistry—the description of biological phenomena in the terms of chemical molecules—was exactly the same as that of molecular biology.\footnote{The talk was given on Südwestfunk on April 10, 1964, and repeated on April 24. A draft of the speech can be found in the Melchers Papers, Kasten 2.} His own career had shown him that geneticists, zoologists, biologists, and biochemists had all made contributions to molecular biology and that the new name should not obscure basic continuities in the experimental tradition. Melchers cited Delbrück often in this speech, and noted that the phage group itself was firmly rooted in research traditions dating back to the nineteenth century. Butenandt and Schramm also conceived of molecular biology as the fusion of traditional lines of research.\footnote{Adolf Butenandt, "Molekulare Biologie als Fundament der modernen Medizin," originally given as a talk at the Bayer-Haus in Munich, November 10, 1965. Reprinted in Adolf Butenandt, \textit{Werk eines Lebens} (Göttingen: Vandenhoeck & Ruprecht, 1981): 837-847, see esp. 842; Gerhard Schramm, "Biochemische Grundlagen des Lebens." Nach einem Vortrag auf der Biologen-Tagung, Muenchen, 1962. MPG-Archiv, Schramm Papers, 0057.}

\subsection*{5.2.2 The Next Generation}

Such collaboration was facilitated by mobility, which, as we have seen, was something the German researchers lacked in the immediate postwar era. By the late 1950s, though, representatives from Tübingen were regular participants in all the most
important international conferences, with Schramm often serving as the representative of the entire group.\textsuperscript{75} He was a good choice since he was actively involved in the research, was extremely personable and outgoing, spoke English well, and presented his lectures with an enthusiasm that was contagious. By all accounts (except for those of Fraenkel-Conrat) he was very well received whenever he lectured abroad. Butenandt had ceased to participate directly in the TMV research even before relocating to Munich, and had turned his interest toward the politics of science in his own country. He became president of the Max-Planck-Gesellschaft in 1960. Freksa, too, had largely turned away from TMV although he continued to be an important source of ideas, support, and encouragement. Finally, Melchers showed no enthusiasm for visiting the United States during the 1950s, despite the efforts of his good friend Anton Lang to convince him otherwise. Melchers did not visit America until 1960.\textsuperscript{76}

Schramm’s travels, though extensive, were also brief. While he presented his research at the most important international conferences, he did his research entirely in Tübingen. Neither he, nor any of the other senior TMV researchers who had been active during National Socialism, spent an extended time (six months or longer) at any foreign lab after the war. The younger researchers, in contrast, took advantage of an increasing number of possibilities for extended research trips to America, which by this time were indispensable parts of their professional preparation. Some of these opportunities came

\textsuperscript{75} Interview with Alfred Gierer, Tübingen, May 23, 2000. Nevertheless, Schramm’s role as representative tended to mask the importance of research carried out by the younger scientists, especially those in Melchers’ division. See note 27.

\textsuperscript{76} In 1959, Lang wrote to Melchers in exasperation, saying “Your fear of this continent appears to be turning into a true psychosis.” (Ihre Angst vor diesem Kontinent scheint schon zu einer wahren Psychose zu werden.) Lang to Melchers, December 9, 1959. Melchers Papers, Ordner 14. Hans Gaffron, who had worked with Melchers under von Wettstein before emigrating to the US in the late 1930s, encouraged Melchers to visit the US as well. Gaffron felt that Melchers’ lack of US experience was starting to have a negative effect on his ability to communicate with his students. Gaffron to Gerald Pomerat, Assistant Director, Division of Natural Sciences and Agriculture, January 16, 1960. Melchers Papers, Ordner 15.
from the German side. In addition to the Fulbright and Deutscher Akademischer Austauschdienst (German Academic Exchange Service, DAAD) programs, the Deutsche Forschungsgemeinschaft (DFG) also encouraged academic exchange, and by the late 1950s was better able to support it financially. For example, Peter Starlinger was able to spend time at Delbrück’s lab in 1958 thanks to DFG funding.\footnote{Max Delbrück to Peter Starlinger, October 22, 1957; Starlinger to Delbrück, May 5, 1958. Both letters Delbrück Papers, Carton 20, folder 16.}

In biology, however, most of the opportunities for research abroad were provided by organizations within the United States itself, resulting in a series of bilateral scientific relationships between south Germany and the West Coast of America. For example, when Gierer went to Caltech for six months in the fall of 1958, he was funded by a Gosney Fellowship granted to him by the University; the Max-Planck-Gesellschaft had only to pay travel costs.\footnote{Naturally Delbrück was instrumental in winning the fellowship for Gierer; see Delbrück to Gosney committee, December 20, 1957; Delbrück to Gierer, December 30, 1957; George Beadle to Gierer, April 3, 1958, and Gierer to Delbrück, April 21, 1958, all Delbrück Papers, Carton 9, Folder 6.} Mundry went to Caltech the following year to work with Renato Dulbecco financed by a stipend from the American National Academy of Sciences, which was given in recognition of the nitrous acid work.\footnote{Interview with Karl-Wolfgang Mundry, Stuttgart, May 25, 2000.} Although not directly part of the TMV story, Weidel had continued to do brilliant research on viral receptor sites on the cell wall of \textit{E. coli} and returned to Pasadena to teach virology during the 1960-1961 academic year.\footnote{MPG-Archiv, Wolfhard Weidel Papers, III. Abt., Rep. 32, Nr. 5.} Two of the other Tübingen researchers served as research fellows on the West Coast as well. Wittmann worked with C.A. Knight at Berkeley during the 1956-7 academic year, and Heinz Schuster worked as a postdoc in Robert Sinsheimer’s lab at Caltech from 1963-1965.\footnote{Lebenslauf, Dr. H.G. Wittmann, MPG Archive, II Abt., Rep. 1A/Max-Planck-Institut für molekulare Genetik/Wittmann; Thomas Trautner, “Nachruf: Heinz Schuster,” 194-195.} Representatives from the
Rockefeller Foundation had not forgotten about the Germans either; it was because of a Rockefeller Fellowship that Melchers finally visited the US in 1960.

This kind of exchange was possible only because of the close cooperation and sponsorship of American scientists. The previous narrative has described how American investigators became interested in the postwar research produced in Tübingen. Because the first generation of West German scientists trained in the postwar era were familiar with the latest techniques and were clearly interested in problems of universal significance, they were welcomed at the best biology labs in the United States. During their stays, they became integrated into the international scientific community to an extent that their advisers could not. Senior researchers such as Schramm, Butenandt, Melchers, and Freksa had all established their scientific careers before the so-called “Golden Age” of biology. Gierer, Weidel, Schuster, and Wittmann, in contrast, had all made their scientific reputations as part of that revolution, as had Delbrück, Watson, and Crick, making the integration and acceptance of the young Germans easier and more thorough than that of their senior colleagues. Undoubtedly the isolation of the war years and the uneasy relationship with some émigrés were factors, but there was also an element of generational change within the scientific community that played a role as well.\footnote{Interview with Thomas and Barbara Hohn, Basel, July 19, 2000.}

This “generational” difference in the German researchers’ relationships abroad is illustrated by the letters of support they received from their colleagues. As part of the process of promoting an individual to the rank of Scientific Member (“Wissenschaftliches Mitglied), the Max-Planck-Gesellschaft solicited letters of support from other members of the scientific community in order to evaluate the scientific merits of the proposed individual. When the MPG promoted Schramm in the early 1950s, the
society requested recommendations for him from only German-speaking members of the European scientific community. Three of the five letters came from Germany and the other two from Sweden, including one from Arne Tiselius. Presumably, the Max-Planck-Gesellschaft officials chose potential references because of their familiarity with Schramm’s work and the assumption that they would respond favorably. The bias toward the German-speaking world would then indicate that in 1952, the year the letters were requested, Schramm’s reputation in Germany and Sweden was excellent. Wendell Stanley seems to have been an obvious choice as well, given his familiarity with Schramm and his prestige in the TMV research community. Since he was not even approached, the choice almost certainly reflects hesitancy and uncertainty regarding Schramm’s reputation, and perhaps that of German scientists in general, outside the German speaking world.

The younger generation of postwar researchers developed a network of professional relationships that was firmly centered on America. In 1963, both Wittmann and Schuster were considered for positions at the new Max-Planck-Institut für molekulare Genetik, to be located in Berlin. In contrast to Schramm, their letters of support came primarily from the United States. Schuster received extremely positive responses from Robert Sinsheimer at Caltech (where he was working as a postdoc at the time) as well as David Schlessinger and Ernst Freese, both at Washington University, St. Louis. Wittmann received favorable responses from Francis Crick, Severo Ochoa (then at the New York University School of Medicine), and Wendell Stanley. All three of these men won Nobel Prizes for work related to molecular biology.

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84 MPG-Archiv, II. Abt., Rep. 1A, Max-Planck-Institut für molekulare Genetik/Schuster and Wittmann.
Alfred Gierer’s recommendations reveal a dual shift in the identity of the German research community. In the late 1950s, the Max-Planck-Gesellschaft decided to make Gierer a scientific member and give him his own division within the Institut für Virusforschung. The scientists chosen to comment on Gierer’s scientific merits were notable both for their national affiliations as well as their prestige. Five of the six individuals who recommended Gierer went on to win Nobel Prizes, and none of the six was from a German-speaking country.85

Gierer’s promotion also represented a change in the disciplinary identity of the West German virus researchers. In accordance with the new identity that was emerging around the study of nucleic acids and proteins (articulated most clearly by the title of the new Journal of Molecular Biology, first published in 1959), Gierer’s new division was named the Abteilung für Molekularbiologie (Division for Molecular Biology). It was the first such institute in the Federal Republic, and was founded at roughly the same time as similarly named institutes in Berkeley and Cambridge. Gierer’s colleagues tailored their letters to recognize this change in nomenclature by evaluating Gierer’s scientific accomplishments in terms of molecular biology rather than virus research or biophysics. Writing in support of Gierer from Cambridge in 1960, Francis Crick stated, “In my opinion, Gierer is the outstanding younger scientist in German Molecular Biology today.” Gunther Stent, of the Department of Virology at Berkeley, wrote that Gierer was “in the forefront of avant garde molecular biology,” and James Watson, then at Harvard, wrote, “There is no doubt that Gierer ranks among the leading molecular biologists of Europe...”86

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85 The Nobel Prize winners who sent Gutachten for Gierer’s promotion were George Beadle (Caltech), Francis Crick (Cambridge), Alfred Hershey (Cold Spring Harbor), Jacques Monod (Pasteur Institute), and James Watson (Harvard), the sixth was Gunther Stent (Berkeley).

5.2.3 Brain Drain

The importance of American references in the evaluation of the talents of West German researchers demonstrates that by the 1960s, America was unquestionably the global center for molecular biology research. Many of the best labs were located in the United States, and overall America was characterized by a dynamic intellectual environment that made it an ideal place for young scientists to begin their careers. In fact, America provided so many outstanding opportunities for young scientists from around the world to further their careers that it threatened to overwhelm the efforts of individual European countries to develop their own indigenous research communities. The problem, widely recognized at the time, was that young scientists would go to America and stay, reinforcing America’s pre-eminence and reducing the intellectual capital of their home countries. Consequently, Europe was rapidly falling behind the United States as far as fundamental biological research was concerned, despite the fact that Europeans had done much of the revolutionary work of the 1950s.87

“Brain Drain” in the direction of America affected all Western European countries, but the West Germans were particularly concerned with its effects because so many of their quality scientists had already been driven away in the 1930s. They could scarcely afford to lose any more. When researchers such as Hans Krebs, Max Perutz, Fritz Lipmann, Gunther Stent, and Max Delbrück made profound contributions to Anglo-American biology in the postwar years, their work served as a constant reminder of the foolishness of Nazi racism. Young Germans traveling abroad were stunned by the

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87 John Krige, “The Birth of EMBO and the Difficult Road to EMBL.” Forthcoming in a special issue of Studies in the History and Philosophy of Biological and Biomedical Sciences. I would like to thank Dr. Krige for sharing a copy of his manuscript with me in advance of its publication. The problem of “Brain Drain” was a global phenomenon; in 1967 a conference on the topic was held in Lausanne, Switzerland, with presenters discussing it in the context of Europe, Africa, and Asia. See the collection of essays edited by Walter Adams, The Brain Drain (New York: The Macmillan Company, 1968).
magnitude of the human potential that their country had squandered (in addition, of course, to the pain and suffering that Germany had caused within Europe). Walter Doerfler, currently of Cologne University’s Institute for Genetics, recalled his first visit to America as follows:

It’s not a secret, everybody knows it, and I’m sure that it is a part of your topic, but a LOT of the very best scientists had left Germany. And we, as the “kids” who had survived the Second World War knew what had happened, but I didn’t really understand until I visited America in 1959. Then I understood what had happened to all of these people and where they all came from... I learned the consequences of what had happened for German science. Of course that was secondary to the human tragedy, but still as a scientist this is the sort of thing that you have to pay attention to.\(^8\)

The appropriation of German scientific talent by the American, British, and Soviet occupations had exacerbated the problem, making the MPG anxious to entice some of the more successful German émigrés, such as Delbrück and Stent, to return to the Federal Republic to lead Max-Planck-Institutes. Both researchers, however, declined, citing personal and professional reasons for choosing to remain in the United States.\(^9\)

Those who wished to promote modern biology within the Federal Republic quickly recognized that America’s superior scientific infrastructure was both a resource and a threat. In a report published in 1958, the Deutsche Forschungsgemeinschaft noted that young scientists had to travel to America to learn the latest advances, but often chose not to return “because in Germany they can find neither institutes nor personal positions

\(^8\) Interview with Walter Doerfler, Cologne, June 23, 2000.

\(^9\) In 1952 Delbrück received an offer to head the MPI for Virus Research in Tübingen, which he declined; in 1956, the MPG discussed asking Delbrück to serve as Alfred Kühn’s successor, but again he declined saying that he did not wish to work in the environment of the MPG. Gunther Stent was offered a position as one of the division leaders of the MPI für molekulare Genetik when it was opened in the mid-1960s, and very seriously considered taking it, but declined for personal reasons. Max Delbrück to Karl-Friedrich Bonhoeffer, November 24, 1952; Bonhoeffer to Delbrück, November 8, 1956; both letters Delbrück Papers, Carton 4, Folder 5. Gunther Stent to Adolf Butenandt, April 2, 1964. Melchers Papers, Ordner 21.
dedicated to these newly developing fields. Several of the Tübingen researchers, including Melchers, Weidel, and Freksa, had contributed to the report.

Despite recognition of the problem at such a relatively early stage, West Germany simply could not make the wholesale changes to the national research infrastructure that would have been necessary to counter the lure of America. The flow of scientists to America continued and actually worsened as the 1960s progressed. In Melchers' papers at the archive of the Max-Planck-Gesellschaft there is a short unsigned report from 1960 detailing (in alarm) several cases of young German biologists who had chosen to take positions in the United States. The Volkswagen Foundation undertook a more systematic analysis of the problem and in 1967 published a lengthy analysis of scientific emigration to America. The authors showed that the problem was greatest in physics and engineering; 49 per cent of émigré scientists were physicists, while only 14 per cent were biologists. Given the relatively small size of the West German biology community, however, especially in the field of molecular biology, these losses were significant. Both of these later reports repeat the conclusions of the 1958 report by the DFG—young German scientists found that there were more opportunities in America, the positions there paid better, and the research style was much more open and collegial than that typically found in a West German research institute. Therefore, despite the fact that close professional relations existed between West Germany and America, theirs was not a relationship of equals, nor was it even close to being so.

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Researchers in both countries recognized the greater professional opportunities that existed in the United States, and the best that senior scientists and politicians in the Federal Republic could hope to do was to recognize and retain quality scientists on an individual basis. The Max-Planck-Gesellschaft often created new institutes, or new divisions within previously existing institutes, for individual scientists in order to entice them to stay. For example, in the early 1960s, MPG leaders feared that Werner Reichardt, a researcher in the field of biological cybernetics, would leave the Federal Republic to take up a position at Caltech. Reichardt’s potential defection was used to justify building a new Max-Planck-Institute in Tübingen, one that would allow Reichardt and his colleagues the chance to explore this important new area of biology in Germany, not the United States. In 1967, the society was successful in drawing Thomas Trautner, who had done his doctoral work in Göttingen under Carsten Bresch, from a position as assistant professor at Berkeley back to the Federal Republic to head one of the divisions of the Max-Planck-Institute für molekulare Genetik in Berlin.

5.2.4 The “Neckarbote”

The changes that marked the maturation of the TMV researchers into respected members of the molecular biology community are best illustrated by changes in the journal Zeitschrift für induktiv Abstammungs- und Vererbungslehre (By the 1950s the title of the journal had been shortened to the Zeitschrift für Vererbungslehre—henceforth

92 Alfred Kühn to Adolf Butenandt, July 1, 1960, and Butenandt to Melchers, August 8, 1960, as well as the collective letter from Melchers, Weidel, and Wolfgang Beermann to Dr. Lehmann of the Biological/Medical Section of the Scientific Council of the MPG regarding Reichardt. All three letters Melchers Papers, Ordner 15.

93 Interview with Thomas Trautner, April 5, 2000. Personal factors could also favor the retention of German scientists; both Heinz Schaller and Walter Doerfler spent lengthy periods at prestigious American institutes in the late 1950s/early 1960s and knew that America had a great deal to offer them on a professional level. Nevertheless both returned to Germany. Schaller to Tübingen and Doerfler to Cologne, because of personal reasons. Interview with Walter Doerfler, Cologne, June 23, 2000; Interview with Heinz Schaller, Heidelberg, July 21, 2000.
The \textit{ZfV} was the oldest genetics journal in the world, and in the postwar era, the task of keeping it running fell to Melchers. The journal had been founded when Germany's prominence in the biological community was uncontested, but its circulation had declined precipitously, and of its few dozen subscribers, very few were interested in virology or molecular biology.\textsuperscript{94} The journal's cost, extensive use of German, and lack of recognition in America further limited its influence.

Melchers' colleagues in America did their best to help maintain the journal but met with limited success. Gunther Stent encouraged his colleagues to submit manuscripts to Melchers, but few had even heard of the journal, and fewer still desired to publish there. To illustrate the declining importance of the journal, Stent said that he had recently published two articles, one in the \textit{ZfV} and one in the \textit{Journal of Molecular Biology}. Although the \textit{JMB} piece was of more limited appeal, it generated roughly 500 reprint requests, while the \textit{ZfV} article, which was of more general interest, was requested by only fifty individuals.\textsuperscript{95} Delbrück also sent promising work to Melchers for publication in the \textit{ZfV}, but his was clearly an effort to prop up the journal rather than an affirmation of its inherent quality. In the letter accompanying one submission, Delbrück cheerily admitted that by sending the article he was "hoping thereby in a small way to help rescue our journal from extinction." A second article, sent about a month later, was proof of Delbrück's "unswerving loyalty to the oldest and weakest journal of genetics."\textsuperscript{96}

From his side, Melchers required his students to publish their best work in the journal, and, as noted above, the \textit{Zeitschrift für Vererbungslehre} is where the lengthy

\textsuperscript{94} Interview with Karl-Wolfgang Mundry, Stuttgart, May 25, 2000.

\textsuperscript{95} Stent also mentioned the high cost of the journal as one of the factors contributing to its weak appeal. Stent to Melchers, 12/27/62. Melchers Papers, Ordner 18.

\textsuperscript{96} Max Delbrück to Georg Melchers, October 15, 1964; Delbrück to Melchers, November 6, 1964. Both letters Delbrück Papers, Carton 15, Folder 30.
version of Gierer and Mundry's work appeared as well as the first comprehensive report of Wittmann's comparative sequencing work. The problem was that researchers abroad would read and cite the shorter, English-language versions of these experiments and often ignore the German versions entirely, despite the fact that these were much more comprehensive and contained more data as well as details on experimental procedures. Submitting articles to the Zeitschrift für Vererbungslehre, or to the Zeitschrift für Naturforschung, which had Butenandt on its editorial board, assured the young German researchers that their work would be published quickly, but did not guarantee that these articles would then be widely read. In fact, Mundry and some of his colleagues took to calling the ZfV the "Neckarbote," which can be roughly translated as "Neckar Messenger" or "Neckar Herald." The Neckar is the river that meanders through the relatively isolated university town of Tübingen, and by associating the journal with its provincial location, Mundry and his co-workers mocked the Neckarbote's insignificance in the fields of virus research and molecular biology.97

One of the problems for the Tübingen researchers was that their main language of publication was becoming increasingly irrelevant to much of the scientific world. The decline of the importance of German after World War II contrasted significantly with the situation at the beginning of the century, when knowledge of German had been a requirement for studying biology. Writing in 1898, the Spanish Nobel Laureate Santiago Ramon y Cajal said that for a career in biology, at least a minimum knowledge of scientific German was necessary since Germany had accomplished more in biology than all other nations combined.98 A half a century later, the situation had changed. Beginning in the 1920s, American biomedical journals began citing German articles less and less


98 Cited in Weyl, "Denkschrift zur Lage der Biologie," 1. Ramon y Cajal shared the 1906 Nobel Prize in Medicine or Physiology with Camillo Gorgi for work on the nervous system.
frequently. The rise of Nazism and the Second World War perpetuated this decline. In 1949, Alfred Kühn recognized that Germany was no longer a leading force in science and that Germans needed to be open to foreign scientists, whether or not they wrote in the language of Goethe. By 2000, the German molecular biologist Carsten Bresch could say with little exaggeration "...if it isn't in English, it doesn't exist these days." While the exact impact of this linguistic shift is difficult to assess, it clearly hindered the dissemination of the findings of the German research community.

In the case of the Neckarbote, Melchers and colleagues solved these problems by transforming the journal in the mid-1960s. In 1963, they gave it the subtitle "An International Journal for Classical and Molecular Genetics," and in 1967 changed the name of the journal to the English Molecular and General Genetics. The editorial board was doubled in size and became much more international, although continental Europe was still heavily over-represented. Delbrück also encouraged Melchers to adopt a more "American" editorial style in which prospective manuscripts would be sent to two different referees who were not directly affiliated with the authors. For years the journal had published articles in both English and German, but the re-naming heralded a

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100 In 1949, Emil Witschi, an American professor visiting Tübingen offered to publish his research in the journal Die Naturwissenschaften. Many of the German researchers were anxious to have the work appear in their journal but insisted that Witschi re-write it in German. According to Witschi's report, Kühn intervened and informed his colleagues that Germany must be prepared to play a secondary role in science until they had demonstrated their willingness to engage their foreign colleagues on a more equitable basis. "Diary of Gerald R. Pomera, 1949," Interview with Prof. Emil Witschi, September 15, 1949. RAC, Record Group 12.2. Diaries, Box 36, p. 322-323; Interview with Carsten Bresch, Freiburg, June 9, 2000.

101 Mastery of English was also a hurdle that the French molecular biologists had to clear; François Jacob remembers his trepidation at presenting his first talk in English in 1952. By that point he said that English was becoming "more and more the sole international language of science." François Jacob, The Statue Within: An Autobiography, trans. Franklin Philip (New York: Basic Books, 1988), 267.

major linguistic shift as the administrative language of the journal was switched to English. As the years passed and Molecular and General Genetics flourished, English became the de facto language of publication. The journal, like the researchers themselves, became heavily “Americanized” in order to remain relevant to the broader scientific community.

5.3 Molecular Biology and West German Society

5.3.1 Technological Pessimism in West German Society

The Tübingen researchers did not have to alter substantially the scope of the research program they had initiated in the 1930s in order to become accepted members of the molecular biology community. Instead, they improved networks of professional relationships in the ways noted above. When they began to speak the same language as other biologists, both literally and figuratively, they were welcomed because they were able to produce reliable scientific data that were relevant to the broader community. Their goal—the description of living processes in terms of chemical molecules—had always been the same as that of other biology labs in Europe and the United States. This basic similarity enabled cooperation in spite of political upheavals. A shared faith in the scientific process and the desire to answer the same questions provided a strong point of connection for researchers from different nations.

In the 1960s, the Tübingen TMV researchers encountered a clash between the values of the scientific community to which they belonged and the country in which they lived and worked. Many of their colleagues in America shared an unbridled sense of optimism in the ability of science to provide guidance and solutions for modern society. Some even believed that the new biology might be used to provide scientific solutions for social problems. Europeans, however, and West Germans in particular, tended to be more critical of the relationship between science and modern society. They did not welcome technocratic solutions to personal issues such as reproduction. In this social
environment, scientific research as a philosophical quest for truth was much more acceptable than science as the pursuit of power for economic, social, or political goals, and the image the Tübingen researchers presented was holistic and deeply intellectual, an island of nineteenth-century German Kultur in the turbulent 1960s.

Germany's relatively late and fast industrialization at the end of the nineteenth century had become a focal point of alarm and pessimism prior to the First World War. It seemed to many as though the best elements of German culture—idealism, wholeness, spirituality—were being subordinated to a myopic, overly rational system in which everything, including people, ultimately constituted nothing more than raw materials for the productive machine of capitalism. The catastrophe of World War II, particularly the slaughter of millions of humans in Nazi Germany's bureaucratic killing machine, seemed to vindicate this spirit of pessimism, allowing it to persist into the postwar era where it was championed by the "Frankfurt Critical School" of sociology. The critique of the Frankfurt school went far beyond the obvious negative side-effects of science, technology, and industrialization, such as environmental degradation, the dehumanization of the workplace, and the production of scientifically advanced weaponry, and struck at the ideology of the scientific/rational world view. Members of the Frankfurt school concluded that industrial technology used the rationality of science as a means to contain the liberating social forces that technology was supposed to make


104 The reduction of humankind to raw material in the assembly line of modern society was a motif in the writings of the German philosopher Martin Heidegger, both before and after the Second World War. See Michael E. Zimmerman, Heidegger's Confrontation with Modernity: Technology, Politics, Art (Bloomington: Indiana University Press, 1990). Within the German scientific community opinions were split on the meaning and purpose of scientific research, and as a result styles of thought ranging from the broadly holistic to the narrowly technocratic existed side by side in the laboratory. Anne Harrington, Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler (Princeton: Princeton University Press, 1996), and Jonathan Harwood, Styles of Scientific Thought: The German Genetics Community, 1900-1933 (Chicago: University of Chicago Press, 1993).
possible. Thus, the rationality of modern science was just a disguise for the “logic of domination” inherent in industrial capitalism, and therefore the entire complex of scientific/technical/bureaucratic values was inherently flawed.\(^{105}\)

This critique was somewhat subdued in the late 1950s as the identity of the new Federal Republic became inextricably bound up with the economic recovery. Economic might, material affluence, and productivity became the touchstones of West German identity, but the underlying tension between German society and the industrial order was never fully resolved. This tension was expressed in the student revolts of the late 1960s, which in many ways were an attack on modern values that echoed the cultural pessimism of the late nineteenth century. In the second edition of his book *The Politics of Cultural Despair*, published in 1974, historian Fritz Stern noted the continuities:

> In the thirteen years of affluence since this book first appeared, the attack on modernity has once again become a dominant theme of our culture. The rebellion of the young—and the not so young—against the emptiness of a materialist age, against the hypocrisy of bourgeois life and the estrangement from nature, against spiritual impoverishment amidst plenty, against the whole “liberal-capitalist system,” has echoed many of the laments of the three critics here discussed.\(^{106}\)

The major difference, Stern noted, was that the recent critics came from the political left, while the thinkers he discussed in his work were from the political right. Regardless, by the 1960s, when investigators established the disciplinary identity of molecular biology, the public in West Germany was very skeptical of science and technology, particularly when these fields touched on issues of human heredity and identity on a very intimate level.\(^{107}\) The scientist as public figure had to tread very carefully in West Germany.

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\(^{105}\) Zimmerman, 217-218.


5.3.2 Eugenics and Human Genetics

Treading lightly on the sensibilities of West Germans entailed avoidance of the legacy of Nazi eugenics, which was not as difficult as one might at first assume. As Chapter 3 noted, the professions that had contributed the most to the development and execution of the Nazi program of racial hygiene and murder—medicine, psychiatry, anthropology, and human genetics—had survived the Third Reich relatively intact. By the early 1950s, human geneticists such as Otmar Freiherr von Verschuer and Hans Nachtsheim had resumed their research at high profile institutions. The subjects of their research, by definition, were human beings, who were much more problematic as experimental systems than were innocuous, cylindrically shaped plant viruses such as TMV. Researchers in the field of human genetics had to acknowledge the social relevance of their work and had to reconcile it with the abuses of Nazi eugenics, while the TMV researchers were under no such obligation. Therefore, there was relatively little resistance to molecular biology research in West Germany in the 1960s.

After World War II, human genetics remained a legitimate branch of scientific research throughout the world, and so its continuing presence in Germany was not an indication of the persistence of Nazi biological racism. Nevertheless, there were enough continuities in personnel (noted above) and ideas to make the acceptance of human genetics in the Federal Republic problematic, particularly when researchers used their work to advocate eugenic measures. Disturbingly, eugenic tracts from the Nazi era were re-published in the postwar era with very few changes. A particularly egregious example was the re-issue, in 1952, of the book Grundzüge der Vererbungslehre: Rassenhygiene und Bevölkerungspolitik by Hermann Werner Siemens, originally published in the late 1930s. Siemens worried that racial fanaticism had tainted the good name of racial hygiene, and hoped that the re-issue of his book would contribute to “bringing ... a level
headed interpretation of the tasks and goals of racial hygiene to a broader audience.” 108 He combined a primer on Mendelian genetics with a racial interpretation of world history that assuredly would have met with the approval of Hitler. In fact, the illustrated graph on pages 102-103 of Siemens’ book was reproduced completely unchanged from a prewar treatise on racial hygiene, Otto Helmut’s Volk in Gefahr. 109 Siemens stressed the threat posed to educated, middle-class persons by the uncontrolled reproduction of the poorly educated and criminal elements of society. In one memorable example, he linked large family size to low social status by correlating the average number of children per family with classroom performance. He called this graph “The Breeding of Stupidity.” (Die Züchtung auf Dummheit). 110

Such continuities in personnel and ideas made it impossible to absolve the study of human genetics, with its close links to eugenics, of the taint of Nazi racial policies. In West Germany, this association hurt the discipline of human genetics in the postwar years, which Max Hartmann, a biologist and leader of a division in the MPI für Biologie, noted as early as 1950. Nachtsheim repeated Hartmann’s findings a few years later, writing that research in human genetics was proceeding in West Germany, but that teaching was in a deplorable state. 111 Nachtsheim attributed this situation to the association of genetics with the Nazis, an association he was unwilling to accept. Nachtsheim repeated the argument that no good scientists had taken part in Nazi crimes.

108 Hermann Werner Siemens, Grundzüge der Vererbungslehre Rassenhygiene und Bevölkerungspolitik (Munich: J.H. Lehmanns Verlag, 1952), quote from Forward; also see 102-103, 110-133.


110 Siemens, Grundzüge, 128.

only frauds and opportunists; as evidence he mentioned that the Nazis had created no new university positions for human genetics, only for racial hygiene. This lack of positions in the universities, perpetuated in the postwar years, meant that very few students were training in human genetics. To remedy the situation, Nachtsheim advocated a long-term program to create new university positions and attract new students.\textsuperscript{112}

While Nachtsheim’s judgment of the relationship between science and National Socialism was certainly self-serving and deceptive, insofar as it obscured his willingness to work with the regime, his assessment of the state of human genetics in the Federal Republic was accurate. In the mid-1950s, according to historian Hans-Peter Kröner, there were only two university chairs for human genetics (occupied by Fritz Lenz and Verschuer) in addition to Nachtsheim’s position in the MPG.\textsuperscript{113} The situation began to change after the Paris Treaty came into effect in 1955, restoring sovereignty to the Federal Republic. Political leaders in West Germany became interested in pursuing atomic energy research and quickly formed a German Ministry for Questions of Atomic Power. The ministry in turn became interested in the effects of radiation on human health and heredity, particularly the mutagenic effects of radiation on human genes. The ministry formed two working groups to investigate the topic in 1956. The two groups (one of which included Hans Friedrich-Freksa) concluded that the state of genetics research in West Germany was unacceptable and recommended further support. By the early 1960s, genetics began to receive more funding and the German Scientific Council advocated that a number of new university chairs be created.\textsuperscript{114}

\textsuperscript{112} Ibid., 204, 201, 207.


\textsuperscript{114} Ibid., 74-80.
West German efforts to support the study of human genetics reflected a global concern regarding the impact of the modern environment on human heredity. Scientists worried that increasing levels of radiation and chemical mutagens in the environment might cause an accumulation of defects in humanity's collective gene pool. Many then reasoned that this increase would become a menace to public health because advances in medicine had effectively removed Darwinian selection mechanisms from human society. Thanks to modern medicine, even those who suffered from extraordinary genetic illnesses could hope to live long lives and possibly even have children of their own. Biologists around the world feared that the combination of an increased mutation rate with a decrease in selective pressures against defective genetic conditions threatened humanity with an ever-increasing genetic burden. The only way to combat this burden, they reasoned, was to take eugenic measures to ensure that those possessed of genetic illnesses did not pass their defective genes on to further generations. Of course, these researchers presented no empirical evidence to document their fears. Their reasoning tended to be completely abstract, and neglected the environmental element of public health issues. Nevertheless, the Atomic Age had breathed new life and new justification into the genetic fears of the early twentieth century.

West German geneticists such as Nachtsheim and Verschuer were enthusiastic proponents of this new eugenics. In 1966, Nachtsheim wrote that twenty years after Germany's experience with totalitarianism, it was time for the public to recognize the scientific importance of eugenics. Modern society had created a dual genetic burden, and he assured his readers that despite all the advances of modern medicine, there was no way to cure genetic illnesses. One could hope to alleviate suffering by treating the illnesses symptomatically, but the only certain way to end these illnesses was to prevent the perpetuation of their genes. He reminded his readers that in medicine, "An ounce of
prevention is worth a pound of cure.” Verschuer also believed in the existence of a genetic burden and along with Nachtsheim advocated birth control, in the form of voluntary sterilization, as the only sure means to combat it. Both believed that the births of unwanted (defective) children could not be prevented in any other manner, though both also believed that such sterilization should be voluntary, not compelled as it was under the Nazis. Therefore the difference, at least in West Germany, was that in the eugenics of the 1960s, sterilization was to be an individual choice, not a matter of public policy.

5.3.3 Man and His Future

Researchers in other countries were not reluctant to advocate the implementation of eugenic policies by the state. There were many molecular biologists who believed that the advances of the 1950s and early 1960s would make such policies even more effective. The most dramatic meeting of the new biology and the old eugenics took place in November of 1962, when the Ciba Foundation invited twenty-seven prominent thinkers, predominantly (but not exclusively) from the biological sciences, to participate in a seminar on the social implications of the recent advances in biology. The point of the symposium was to prepare society for what might come; one of the organizers wrote that the world had been unprepared, socially, politically, and ethically, for the advent of

115 Hans Nachtsheim, Kampf den Erbkrankheiten (Franz Decker-Verlag Nachf. GmbH, 1966), 96. Nachtsheim’s quote is literally “Prevention is better than healing,” but I have chosen to translate it into the familiar English idiom to make the text read more smoothly.


nuclear power. By organizing this symposium, the Ciba Foundation hoped to prepare society in advance for the power of applied biology, and of necessity the talks given were entirely speculative. The next year the papers presented at the symposium were published, along with general discussion, as the book *Man and His Future*.\(^{118}\)

The English biologist Sir Julian Huxley set the agenda for the meeting with his opening talk, "The Future of Man—Evolutionary Aspects." He noted several important issues that the symposium participants were to discuss—over-population, pollution, and poverty, just to name a few. He also addressed, at length, the idea of the genetic burden of humankind. He framed his talk within the idea of progressive evolution, both biological and social. For him, progressive evolution was a characteristic of human history, but he suggested to his colleagues that in recent years human evolution had begun a period of reversal. He argued that the accumulation of genetic mutations caused by the modern environment was producing degeneration rather than progress. He then claimed, "The improvement of human genetic quality by eugenic methods would take a great load of suffering and frustration off the shoulders of evolving humanity," and that "Eventually, the prospect of radical eugenic improvement could become one of the mainsprings of man's evolutionary advance."\(^{119}\)

Although all of the topics mentioned by Huxley were discussed during the symposium, it was this optimistic view of eugenics, affirmed by many other participants, which became the meeting's most enduring legacy. Hermann J. Muller followed Huxley with a discussion of voluntary eugenics, which he called germinal choice. Muller said that, in truth, the twenty per cent of the population possessing the highest number of

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\(^{119}\) *Man and His Future*, 1-22, quotes from 17, 21.
defective genes should not be allowed to reproduce at all. Realistically, he suggested that the best way to achieve this goal would be to educate people regarding the hazards of the genetic burden, and to create sperm banks containing the gametes of genetically desirable persons. These sperm banks were to provide the genetically impoverished with an alternative to their own defective DNA. Prospective parents could choose from sperm donors who would provide favorable characteristics for their offspring, allowing them to raise their own families without contributing to the overall genetic burden.\textsuperscript{120}

Joshua Lederberg complemented Muller’s eugenics with the newer concept of \textit{euphenics}, which he defined as the controlled manipulation of human chromosomes and segments of DNA in order to improve the genetic makeup of individuals. In essence, he became one of the first to articulate what we today would call genetic engineering.\textsuperscript{121} In the ensuing discussion, Francis Crick added his own perspective on biological ethics to the mix of eugenics and euphenics. Crick foresaw no ethical limitations to the programs proposed by Lederberg and Muller. In fact, Crick argued that in terms of humanist ethics, people did not automatically have the right to have children—children and the family were not private affairs but rather the business of the state.\textsuperscript{122}

While some agreed with Crick, this agenda of biological intervention and manipulation did not receive the universal endorsement of the symposium participants. Jacob Bronowski dissented by saying that he saw no evidence of degeneration in

\textsuperscript{120} Hermann J. Muller, “Genetic Progress by Voluntarily Conducted Germinal Choice,” in \textit{Man and His Future}, 252-253.

\textsuperscript{121} Joshua Lederberg, “Biological Future of Man,” in \textit{Man and His Future}, 265. Lederberg later said that his intention was more to spark discussion than to advocate any plan for applying molecular biology to human society. Interview with Erhard Geissler, Berlin, March 29, 2000.

\textsuperscript{122} “Discussion: Eugenics and Genetics,” in \textit{Man and His Future}, 275. It is also worth noting that Linus Pauling also believed that the human race was in a process of genetic degeneration and interventionist policies based on biology would eventually need to be implemented. See discussion in Lily E. Kay, \textit{The Molecular Vision of Life: Caltech, the Rockefeller Foundation, and the Rise of the New Biology} (Oxford: Oxford University Press, 1993), 274-275.
humanity at all; biologically speaking, people were no worse off than they had been fifty years earlier. He therefore believed that Crick, Lederberg, and Muller were advocating solutions for a problem that simply did not exist. He cautioned, “Without a much deeper analysis, the unguarded transfer to ‘society’ of ideas proper to individual responsibility can mislead us into talking—and selling—moral nonsense...That such nonsense has proved saleable, especially in Nazi Germany, should warn us against evaluating our plans for the race solely in terms of technical feasibility.”

When Man and His Future was translated into German as Das umstrittene Experiment: Der Mensch (The Controversial Experiment: Humankind) Bronowski’s caution resonated much more deeply with the West German audience than did the optimism of the others. Many West German scientists were outraged at the eugenic suggestions. Verschuer was immediately critical of the book despite his own advocacy of eugenics. He resented the exclusionary nature of the Ciba group (since no human geneticists had been invited), and rejected the presumption of the symposium participants that their narrow circle was qualified to make choices regarding the individual rights of the rest of society. Since Verschuer had worked with Josef Mengele, his credibility as a commentator on bioethics was questionable. Nevertheless, despite his hypocrisy, Verschuer did identify the element of the Ciba symposium that was contentious in West Germany after the Nazis—the appropriation of personal decisions by technocratic experts.

Actually, the Ciba symposium had been even more exclusive than Verschuer noted. Twenty-six of the twenty-seven participants were from America or England (the

123 "Discussion: Eugenics and Genetics," 286.

124 Verschuer, Eugenik, 69-75.
last was from France), and all were male.\textsuperscript{125} Although the entire symposium was speculative and the ideas discussed were \textit{not} meant as a plan of action, the willingness of this small, elite group to remove personal decisions, such as child bearing, from the private to the public sphere struck a nerve in the West German intellectual community. The Ciba participants believed that they had the right to determine the fate of individuals because in their minds they had reduced human, social problems into technical problems, the solution of which required the skills of a technically skilled elite, namely themselves. To many West Germans, it seemed as though scientific rationalism was attempting to turn individual humans into mere cogs in the machine of the state once again.\textsuperscript{126}

Verschuer was not the only person to object. In 1969, a collection of essays by a diverse group of West German scholars was published in response to the Ciba volume. The most incisive essay, "Science, Medicine, and Humanity, seen from the Perspective of Medicine," by medical anthropologist Wilhelm Kütemeyer, compared the ideas of the Ciba symposium with those that had made the rationalized, bureaucratic mass murder of the Holocaust possible. The author argued that, like the Nazis, the Ciba scientists saw no difference between people and other living things and therefore felt they could do as they wished with people. He feared that a new program, similar to that of the Nazis, was emerging under the guise of reductionistic science, and that unlike Nazism, it was not confined to continental Europe.\textsuperscript{127}

\textsuperscript{125} At least two of the participants, Albert Szent-Györgyi and Fritz Lipmann were émigrés from continental Europe who had resided in America since the 1940s.

\textsuperscript{126} Serving as a further cultural brake on technological optimism was the re-emergence of Faust as a cautionary tale after the war. This began with the publication of Thomas Mann's \textit{Doktor Faustus} in 1947, after which the Faust theme, that of a pact with the devil for the achievement of mastery over the world, became a metaphor for the excesses of modern society. For example, in 1958 Günther Schwab used a permutation of the Faust legend as a satirical commentary on the environmental crises facing Germany. See discussion in Raymond H. Dominick, \textit{The Environmental Movement in Germany: Prophets and Pioneers, 1871-1971} (Bloomington: Indiana University Press, 1992), 152-153.

The West German TMV researchers shared these reservations toward the optimism of their colleagues in the United States and Britain. Certainly the possible medical applications of molecular biology were not lost on them in a general sense. Butenandt’s close connections with the chemical industry, both before and after the war, reflected his faith in the importance of industry for making the fruits of scientific research widely available, and on several occasions he argued for tighter connections between science and industry.\textsuperscript{128} Schramm, too, believed that molecular biologists had much to offer medicine. They had already revealed molecular basis of certain disorders such as sickle-cell anemia and phenylketonuria, and promised to provide strategies to combat illnesses caused by bacteria and viruses. Schramm singled cancer out as a disease that would likely benefit from the further interaction of medicine and molecular biology.\textsuperscript{129} Schramm and Butenandt both drew the line at eugenics and the deliberate alteration of the human genotype, however.

While not mentioning the Ciba volume by name, Alfred Gierer rejected the idea of the biological improvement of humankind in a public lecture given in 1966. Butenandt, for his part, did mention the Ciba volume by name, and he found it shocking. He expressed grave reservations about the willingness of the volume’s contributors to assess the value of human beings based solely on intellectual criteria and in doing so to ignore the spiritual, social, and religious facets of human existence.\textsuperscript{130} In 1965, Schramm gave a radio talk that refuted the ideas of the Ciba volume almost point for point,

\textsuperscript{128} See Chapter 6, Section 6.2.1.


although he, too, did not mention the book by name.\textsuperscript{131} Titled "The Further Development of Humankind" (Die Weitere Entwicklung des Menschen), the talk took as its starting point the foolishness of trying to predict the future along biological lines. Schramm argued that this was impossible for any one individual, much less for an entire population—complexity in biological phenomena precluded strong determinism. Like Butenandt, Schramm believed that intellect should not be the sole criterion for evaluating the worth of a human being. In fact, he went so far as to suggest that too much intelligence in a population might be counter productive. Compassion and the willingness to accommodate ever-increasing numbers of people would perhaps be more useful traits for individuals in the future. He then pointed out the considerable practical and moral difficulties implicit in Lederberg's vision of eugenics, and lastly, he found the idea of selective breeding via sperm banks disgraceful. The parallels with artificial insemination of livestock were not lost on him, and he cautioned, "A farmer can judge which cow is the best. But who can say who shall do the same for future generations of people?"\textsuperscript{132}

5.3.4 The Public Role of Molecular Biologists in West Germany

The skepticism of the three German scientists discussed above indicates that despite their immersion in the molecular biology community (very thorough in the case of Gierer, less in the case of Schramm, even less for Butenandt), they collectively maintained an attitude towards science and the world that was sometimes at odds with that of many of their American and British colleagues. Integration into the community did not mean homogenization, but rather the creation of intricate webs of professional

\textsuperscript{131} "Die weitere Entwicklung des Menschen." The typescript of the speech in the MPG Archives has "3 VI Rundfunk 65" written on it, indicating it was meant for the radio on June 3, 1965. MPG-Archiv, Schramm Papers, Nr. 0076.

\textsuperscript{132} Ibid., 109-110, 112, 114-115, quote from 115.
and personal relationships among unique, autonomous individuals. Personality characteristics shaped by specific shared historical experiences, such as the lost war, persisted, providing yet another level of diversity within the scientific community.

I shall not postulate the existence of national personality characteristics, but I do think that experiencing National Socialism first hand and growing up in war torn Germany left an impression on the Tübingen researchers. They simply could not be as optimistic as the Americans or British could be after the war. For Americans, science was the “endless frontier” that had provided the weapons for the victorious war against dictatorship. In Germany feelings toward science and technology were much more ambiguous, even at the height of the Wirtschaftswunder.

In this climate, the TMV researchers embraced the role of the scientist as bearer of culture rather than the scientist as technocratic manager. The former had a long and rich tradition in Germany; in the nineteenth and early twentieth centuries, natural scientists, and physicists in particular, had not only been experts in their own fields of research, but had served to unify philosophical questions with the natural sciences and to present the results to the educated community. They thus achieved a role of cultural leadership outside of their own specialization and in their comprehensiveness were seen to embody the values of German Kultur. The English translation “culture” does not do justice to the rich implications of the idea of Kultur, which entailed a depth of learning and a universality of thought that raised Germany above the narrow “Civilization” of France or the gross materialism of the United States. Perhaps the clearest example of the

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133 The phrase is from the title of an essay written by Vannevar Bush, Franklin Roosevelt’s science adviser during World War II, as a preface to a proposal to create a peacetime office for scientific research, the National Research Foundation. See G. Pascal Zachary, *Endless Frontier: Vannevar Bush, Engineer of the American Century* (Cambridge: MIT Press, 1999).
scientist as cultural authority was Einstein, who "Taken to personify this spirit...ended up being transformed in the public imagination into a sage of universal significance."\textsuperscript{134}

Certainly none of the Tübingen researchers became sages of universal significance. They did not have the recognition, nor did biology have the kind of awe-inspiring power that physics did in the postwar era. Nevertheless, they used a variety of media (radio, popular press, and public lectures) to present an image to the educated West German public that exemplified the trends discussed above. However, the discussion that follows is not intended to imply any kind of coordinated effort by the TMV workers to reclaim moral and cultural legitimacy for the physical sciences. The trends I have identified resulted from a common historical experience, a common interest in interdisciplinary research, and a common conception of the scientist as a cultural figure. They were likely not a conscious creation, and the scientists concerned would probably not have identified their philosophical discussions as being particularly German, not even when events such as the Ciba symposium highlighted national differences in philosophical framings of biology. For the TMV researchers, engaging philosophical issues and presenting their ideas to the educated public were not the actions of a good West German scientist—these were the actions of a good scientist.

As director of the first eponymous research group for the study of molecular biology in the Federal Republic, Alfred Gierer presented science as an intellectual endeavor that was humble, broad, and yet stunningly effective. In a public lecture given in June, 1966, entitled "The 'Secret Code of Life'—Heredity and Molecular Biology,"

Gierer framed his research in the context of the search for the essence of life—what was it that separated living from non-living material? He identified the characteristics of reproduction and metabolism as the two factors that characterized life, and of these two, molecular biology was targeted at providing a physical understanding of the process of reproduction. He illustrated the progress that had been achieved to that point, providing a detailed picture of the physical structure of DNA, the steps that led to the understanding of the genetic code, and recent research on genetic regulation.

Gierer recognized that physical explanations of complex biological phenomena had the potential to disillusion or even alienate large numbers of people by reducing life itself to a mechanical apparatus. He therefore sought to assure his audience that an understanding of the process of biological reproduction at the molecular level could not explain human emotions, feelings, and understanding. Such phenomena emerged at higher levels of organization and could not be approached by reducing them to their basic components. Gierer even suggested that the boundary between the intellectual and physical worlds might constitute a boundary on the ability of humankind to understand itself biologically. Molecular biology, despite the stunning advances of the previous decade, could not provide all the answers.

After becoming President of the Max-Planck-Gesellschaft in 1960, Butenandt was less involved in science done at the laboratory bench than he was in the politics of science. As president, he strove to present the work of the MPG to a broader audience and to illustrate the significance of scientific research for West German society. Since he had not been a direct participant in the most successful phase of the TMV research, he tended not to discuss its scientific results in as much detail as the others did.

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136 Ibid., 144-145.
Nevertheless, Butenandt’s efforts to popularize science in the Federal Republic provide a clear vision of science as an intellectual quest, as the manifestation of human inquiry at its highest and most pure stage.

One of Butenandt’s greatest concerns was the tendency of some scientists to measure the value of human beings through exclusively rational means while excluding the social, religious, and spiritual elements of human existence.\textsuperscript{137} Faced with the task of promoting science in a society that looked skeptically upon it, Butenandt, like Gierer, strove to portray science as the highest form of human cultural undertaking; he deliberately attempted to resurrect the tradition of scientists as bearers of culture in German society. The idea of pure science was very useful in this context as well.

Butenandt understood \textit{Wissenschaft} not in the narrow sense of “science,” but in the broader sense of “scholarship” that we have already noted.\textsuperscript{138} Just as he knew that his own research had pure and applied aspects that were in inextricable from one another, he also knew that the various different methods of systematic human inquiry overlapped and were not mutually exclusive. In another talk, originally delivered in 1956, he said, “There is only one science, however much its problems and methods may divide it up into various disciplines. Whether we speak of the natural sciences or of the humanities, their ultimate common goal is the answer to the question: What is the nature of man and of the universe of which he is a part?”\textsuperscript{139}


\textsuperscript{138} In German universities, faculties are divided between the natural sciences (Naturwissenschaften) and the intellectual sciences (Geisteswissenschaften), or humanities.

\textsuperscript{139} Adolf Butenandt, “The Meaning and Purpose of Scientific Research.” Originally presented in 1956 before the Metallgesellschaft AG in Frankfurt. Reprinted in \textit{Werk eines Lebens}, 201-207, quote from 202. The speech is given in English, so the above quote is not the author’s translation.
While recognizing the importance of research for practical ends, Butenandt had always valued the intellectual quest of "pure" science highly, stressing the utilitarian aspects of his work only when it was financially or politically expedient. He understood that taken to extremes, single-minded pursuit of scientific and technical progress for their own sake would impoverish rather than enrich the human condition. He continued:

There can be no doubt that because of such one-sided assessments of "value", the humanities have sunk considerably in public estimation, in comparison with the natural sciences. Therein lies a danger which must not be overlooked and which is very disquieting to anyone who knows that the humanities and the natural sciences derive their strength from the same striving after knowledge, from the same search for the truth.\textsuperscript{140}

Of course, Butenandt's relationship with the Nazis will always cast suspicion on postwar pronouncements such as those cited above, and his critics may raise the point that he was simply attempting to absolve German science of any wrongdoing during the Third Reich. The critics' concern is understandable and legitimate. However, in this context it is beside the point, since I am interested in seeing how Butenandt promoted science in a socially acceptable manner, not the reasons for which he promoted it, which were certainly complex and self-serving. Throughout his life, Butenandt was consistent in his belief in science as a higher calling that merited prestige, freedom, and extensive financial support for its members. What is intriguing is that in the Nazi era, Butenandt believed it was necessary to convince his audiences of the social utility of pure research, while in the Federal Republic, respect for the scientist as a cultural figure allowed him to focus on purely intellectual aspects.

Of all the Tübingen researchers, Schramm probably devoted the greatest energy toward reconciling the natural and human sciences before the eyes of the West German public. He gave numerous talks, both public and private, in the 1960s. Some of his

\textsuperscript{140} Ibid., 206.
speeches were broadcast on the radio, while others were printed in non-scientific periodicals. Schramm stressed the importance of findings from the cybernetics and information sciences for providing the conceptual apparatus necessary for interpreting the new biology. Since information could flow from molecule to molecule, as it did in Crick's conception of protein synthesis, Schramm saw information as pattern independent of substance. Information was not something that could be measured in terms of length or volume, but only in terms of its content. This new conceptualization of information provided Schramm with a scientific way to link the material world with that of the abstract, which then became the basis for his efforts to unite the physical sciences and philosophy.

In his 1962 talk, "Biochemical Foundations of Life" he introduced his audience to the importance of the new concept of information as applied to the biological sciences. Nucleic acids, through their unique sequences, stored and transmitted information. This information, in turn, shaped living material and gave it the pattern and characteristics that we recognize as life. He literally believed that the storage and transmission of information was what differentiated living from non-living material. In a later talk, he compared the transmission of biological information with human language, arguing that the ideas expressed in human language were entirely independent of the medium in which they were expressed, whether that was the letters in which they were written or the sound waves that carried human speech. The comparison with speech allowed him to approach the idea of human thought and ideas, since thoughts are expressed in the spoken

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141 Gerhard Schramm, "Biochemische Grundlagen des Lebens." Talk before the Biology Conference, Munich, 1962. MPG-Archiv, Schramm Papers, Nr. 0057. This folder also contains a shorter version of the talk that was broadcast on South German Radio (Süddeutsche Rundfunk) on February 24, 1963, as part of the series "Living Science" (Lebendige Wissenschaft). These ideas are developed even more fully in his talk "Informative Nucleinsäuren," which was given on February 28, 1964. Schramm Papers, Nr. 0068.

and written word, yet they are simultaneously independent of these physical manifestations. He pushed this comparison to the limit, even suggesting that human memories might be stored in sequences of nucleic acids in the brain.\textsuperscript{143} This was the ultimate example of ideas made real; Schramm thought he had reconciled the material world with that of ideas/spirit (Geist). He concluded the talk as follows:

“If I might try to put these thoughts in order in a way consistent with the currents of our present time, I would say that one consequence of materialism is that it must eventually lead back to idealism. This will be an idealism that can be differentiated from that developed by the thinkers of previous centuries by its mathematical attire, one that is free from romanticism and sentimentiality.”\textsuperscript{144}

Schramm’s quest to root molecular biology in a philosophical tradition that was thousands of years old began in earnest with this talk, given before the Catholic Academy in Munich in 1965. In this speech, Schramm resorted to Plato’s allegory of the cave and suggested that Plato’s theory might be a useful starting point for understanding the relationship between the information in a living creature’s DNA and its physical body. Schramm argued that the information contained in DNA could be seen as the equivalent of Plato’s ideal forms—pure idea, pattern independent of material. Schramm then pushed the comparison by saying that the living beings created under the direction of DNA were the equivalent of everyday objects in Plato’s philosophy—imperfect, mortal, physical reflections of the ideal form. Schramm, however, could not accept Plato’s philosophy at face value. For Plato, the ideal was reality; for a scientist like Schramm, the physical world was reality. In Schramm’s estimation he had actually made an improvement on Plato by reconciling form and matter. In Plato’s world forms were independent; in Schramm’s, forms took physical shape, their essence transmitted by

\textsuperscript{143} Ibid., 14.

\textsuperscript{144} Ibid., 15.
information bearing molecules such as DNA. The form contained within this genetic message, though theoretically eternal, in reality could and did change on a regular basis; Schramm was not about to argue that nucleic acids did not mutate. Mutation, for him, was yet another advantage of molecularizing Plato, allowing for a world that was dynamic and evolving rather than static.

Schramm’s public lectures therefore represent a systematic effort to unify the various facets of human inquiry. Although forced in some respects, his vision was a true synthesis; language and information shaped our physical being, and our intellectual experience emerged from the patterns that shaped our bodies.\textsuperscript{145} His fusion of his own research in nucleic acids, his sophisticated discussion of cybernetics, and his recurring invocation of the giants of classical philosophy (Plato, Descartes, and Kant) truly presented the image of scientist as scholar. He tried to demonstrate that all elements of human investigation, the physical sciences and humanities, were indeed one science, one body of scholarship. Such breadth and unity were the characteristics of true German Kultur.\textsuperscript{146}

\textsuperscript{145} Gerhard Schramm, "Das Phänomen des Geistes aus der Sicht der molekularen Biologie." MPG-Archiv, Schramm Papers, Nr. 0080. This is obviously a piece that Schramm cared about very much; there are several drafts present in his papers in the Max-Planck-Gesellschaft Archives, and handwritten notes indicate that versions of this talk were given at the Freiburg Catholic Academy on February 26, 1966, and at the Conference of the Catholic Academy on March 16, 1967. It was also broadcast on Südwestrundfunk and published in a posthumous collection of essays edited by his co-worker F. Alfred Anderer. See Gerhard Schramm, \emph{Baupläne des Lebens}, ed. F. Alfred Anderer (Munich: R. Piper & Co. Verlag, 1971).

Since Schramm was discussing matters of the human spirit and speaking before religious academies, his own religious beliefs are relevant to this discussion. In the detailed questionnaire of his career present in the MPG archive, Schramm left the space for religious affiliation blank. When I asked his colleague F. Alfred Anderer about Schramm's religious beliefs, Anderer recalled that religion had not been very important to Schramm. Anderer said that Schramm’s relationship to religion was very similar to that described by historian Peter Gay in his memoir \emph{My German Question}. Gay was from a Jewish family in Berlin and never had a strong commitment to the Jewish faith. This comparison leads one to question whether Schramm’s intentions were to reconcile faith and science or to allow science to annex areas of inquiry traditionally reserved for religion. MPG-Archiv, Schramm papers, Bd. 1, Interview with F. Alfred Anderer, Tübingen, July 28, 2000; Peter Gay, \emph{My German Question: Growing Up in Nazi Berlin} (New Haven: Yale University Press, 1998).

\textsuperscript{146} In his study of the development of modern scientific knowledge, Jean-François Lyotard argued that there are two legitimizing narratives—the French narrative of knowledge leading to the emancipation of humanity, and the German narrative of the unity of all the sciences. Jean-François Lyotard, \emph{The...
Conclusion

After the war years, the Tübingen TMV researchers found it easier to present their work as a unified, consistent whole rather than having selectively to stress pure and applied aspects in order to negotiate support. Their research on the Genetic Code earned them the respect of their peers abroad, and they assimilated the information science discourse used by researchers in labs around the world. When the Code was finally complete, they spoke the same language as did the rest of the newly formed molecular biology community. They were able to use this same imagery when presenting their work to the West German public, but on these occasions, they presented their work as part of a uniquely German cultural tradition. They chose not to present their research as the scientific solution to pressing social problems, unlike many of their contemporaries.

They were also not required to; the threat of America’s overwhelming lead in scientific research meant that the TMV researchers were welcomed and supported by virtue of their scientific merit and did not have to pitch their work to the government as they had in the Third Reich. This newfound intellectual freedom allowed them to separate their research on heredity from the legacy of Nazi biological racism, and they were therefore not subject to the skepticism that greeted technocratic, utopian visions of engineering away social problems by engineering humankind itself. Despite these successes, West Germany’s overall contributions to the revolution in biology were decidedly small, a situation that was partially explicable by structural deficiencies in the German research infrastructure that were only belatedly (and partially) overcome by the late 1960s. We shall turn our attention to the institutional context of West German biological research in the next chapter.

Chapter 6:

Restructuring Research in the Land of the Economic Miracle, 1950-1972

Introduction

As I suggested in Chapter 3, thoroughgoing decentralization characterized the reconstruction of German research. During the occupation, officials from the Länder, the universities, and the Max-Planck-Gesellschaft all used the centralization of research under Nazi Germany as a rhetorical weapon with which to defend their own autonomy.\(^1\) Their efforts corresponded with the Allied wish to decentralize power structures within Germany and were therefore initially welcomed by officials in the occupation. The Grundgesetz, the constitution of the West German state, guaranteed the freedom of scholarship, research, and teaching, which were defined as cultural activities and therefore under the jurisdiction of the Länder.\(^2\) Nevertheless, the idea of decentralization

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posed difficulties for the Federal Republic. The system in place by 1950 was ideal, perhaps, for an occupied country trying to allay fears of its resurgence, but it was not appropriate for a sovereign state attempting to compete in the technologically advanced economy of the 1960s and 1970s. As the years passed, the coordination, funding, and direction of research activities proceeded increasingly at the Bund (Federal) level rather than Länder (state) level.

Of the resources available to the West German scientific community, qualified people were by far the most precious. As the preceding narrative suggests, keeping the best researchers in the Federal Republic from pursuing career opportunities abroad was a top priority that required creating an attractive research environment in Germany. Naturally, officials in both the Max-Planck-Gesellschaft and the universities hoped to retain the best scientists, and so the competition for scientific talent had a domestic as well as an international character. For a talented few young scientists, the Max-Planck-Gesellschaft offered the best opportunities for education, socialization into the profession, and independent research. By expanding its number of institutes and divisions, the Max-Planck-Gesellschaft could provide excellent accommodations for the best researchers on a case by case basis, and therefore won many of the early competitions.

In the West German universities, however, it was difficult for young people to learn, much less to practice, the new biology. As I have already noted, interdisciplinary cooperation there was rare, students rarely had independence, and few of the professors had any training in the dramatic recent breakthroughs in biology. Recognition of this mismatch in opportunities convinced many biologists that only a reform of the universities would permit modern molecular biology research to take place there. Thus biological researchers initiated a movement to overhaul the structure of the universities nearly a decade before the student revolts brought international attention to the deficiencies of the West German university system.
6.1 The Emergence of a Centralized Research Infrastructure

6.1.1 The American Challenge

In 1957, the Christian Democratic Union and its ally, the Christian Social Union, won an absolute majority (albeit a slim one) in a general election, the only party to do so in a free election in German history. In the electoral campaign, the party leadership capitalized on the dizzying scope of West Germany’s economic recovery by promising “Keine Experimente!” (No Experiments!) in economic policy. The prosperity of West Germany in the late 1950s contrasted so dramatically with the misery of the immediate postwar years that the recovery became popularly known as the Wirtschaftswunder (Economic Miracle). Faith in the miracle and enchantment with the availability of consumer goods characterized many West Germans in the late 1950s. For example, the historian Gerhard A. Ritter recalls that in 1956, when his best friend bore her third child, he visited her but went straight to the basement to marvel at her new washing machine before seeing the baby.³

It is now clear that the prosperity of the miracle was in no way indicative of technological progress in the West German economy, and that “No Experiments!” was exactly the wrong attitude to have in the late 1950s. The export items that fueled the economy of the Federal Republic—automobiles, chemicals, and electrical equipment—were all based on nineteenth-century technologies. The Wirtschaftswunder was decidedly low-tech, based on improvements in the productivity of an already existing

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manufacturing system. By the early 1960s, the lack of a West German presence in high-tech fields such as computers, rocketry, and aeronautics, began to generate serious doubts about the ability of the economy to compete internationally over the long term.

In the 1950s and 1960s, the American military-industrial-university complex drove progress in the above-named areas of high technology to a significant extent. These technologies, which depended upon control and communication, were radically different from the technologies of the nineteenth century, and the size of postwar research projects necessitated a completely fresh style of management. Projects of enormous size and complexity could be managed only with a flexible, interdisciplinary approach, which is now known as systems engineering. Historian Thomas Hughes has interpreted this

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5 This is the subject of Thomas P. Hughes, Rescuing Prometheus (New York: Pantheon Books, 1998); see introduction, 3-14. For a study that contextualizes the newer, information-based technologies of the twentieth century in a long-term framework, see James Beniger, The Control Revolution: Technological and Economic Origins of the Information Society (Cambridge: Harvard University Press, 1986).

6 For example, the Atlas missile project, pursued by the United States in the 1950s, ultimately employed 18,000 scientists and engineers, 70,000 workers, utilized seventeen contractors, two hundred subcontractors, and 200,000 suppliers, all coordinated by 500 technically competent military officers. Hughes, Rescuing Prometheus, 4.
change in managerial style as an organizational revolution comparable to that of
Frederick Taylor earlier in the twentieth century. The meritocratic, interdisciplinary
systems approach to managing complex technological projects became one of the
strengths of the postwar US economy, an often-overlooked, positive outcome of the
military industrial complex. Such a style was rare among the scientific and technical
elite of West Germany’s miracle economy.

Frederick Seitz, President of the American National Academy of Sciences, noted
this difference in a talk given in the early 1960s. Seitz began with a rapid survey of the
history of science and technology, emphasizing the transfer of scientific leadership from
country to country over the centuries, and ended with America as the undisputed leader in
scientific research. Seitz said, “The emphasis on teamwork in science paid enormous
dividends to the United States during World War II when the idea was brought to its
logical climax by the formation of large laboratories composed of a number of teams
which worked together on different aspects of one or more major problems.” Most of
these labs were directly attached to universities. He asserted that this approach, despite
its de-emphasis on individualism, was more effective than that of “the old-fashioned
European institut.” He spoke briefly about the major western European states, but his
single paragraph about research in Germany drew a strong reaction from the scientific
leadership in the Federal Republic:

For a variety of reasons, Germany has been less willing to accept innovation in
the organization of science than any of the other technically advanced western
nations of comparable size. The universities and related research centers still rely
primarily on the institute system which developed nearly a century ago... At the

7 Ibid., 8-13, 302-303. Robert Pool has argued that a more collegial, less stratified management style
during times of crisis has led to the very high reliability of such complex and potentially dangerous
technological systems as nuclear power plants and aircraft carriers. Robert Pool, Beyond Engineering:

8 The talk was published as Frederick Seitz, “Science on the March,” Physics Today 15 (1962): 24-34.
Quote from 28.
end of the war, the academic leaders, who were mainly older men, were anxious to shake off the political interference that they had had to countenance during the period of national socialism and, hence, turned the clock backwards to the best era they had known, namely, that before World War I. Unfortunately, the present system does not permit Germany to absorb its product of young scientists into positions of appropriate responsibility and prestige. As a result, there is a steady loss of scientists to other western countries, particularly the United States.9

The steady brain drain to the United States, discussed previously, lent additional sting to Seitz’s remarks.

Seitz’s talk was quickly translated into German and received a mixed reception in the Federal Republic. Many agreed that the problems Seitz discussed existed, but they did not enjoy having their own shortcomings discussed by an American in so unforgiving a manner. Adolf Butenandt, then President of the Max-Planck-Gesellschaft, said in a 1963 speech that Seitz’s assessment was incorrect. Butenandt believed that Seitz had not adequately accounted for the impact of the physical devastation of the war on West German research, and he refuted the criticism that the West German research system was too rigid or hierarchical.10 To an extent, Butenandt was correct. The TMV researchers demonstrated that collegial research could and did take place in West Germany, but they also demonstrated that such work was the exception rather than the rule.

A report published by the Deutsche Forschungsgemeinschaft in 1964 also addressed Seitz’s critique, and found the Federal Republic’s research structure more wanting than did Butenandt. Based on surveys distributed to West German researchers, the authors of this report concluded that in the Federal Republic, research in chemistry was excellent, but was poor in many other disciplines. The researchers surveyed mentioned the “lost years” of the Nazi era as one source of the difficulties, but they also

9 Ibid., 29; 29-30.

addressed the structure of the universities and the lack of large scale funding as specifically West German deficiencies. In the biological sciences, the authors noted that virus research was strong in the Federal Republic, but that microbiology and molecular biology more generally were under-developed. In particular they noted that in American university departments, organization was more flexible and the relationship between professor and student was much closer and less formal than in German universities. Students were given more independence and responsibility at a young age and therefore were socialized into the profession of science more quickly than was the norm in Europe. The combination of the greater material resources of many US labs and the concentration of scientific talent in America made the US an irresistible draw for young German scientists and engineers of all backgrounds.

While American labs continued to fill out their ranks with talented young workers from Europe, in 1966 West Germany suffered its first postwar recession. Between 1966 and 1967, the unemployment rate tripled. Ludwig Erhard, the patron saint of the economic miracle who had taken over the Chancellorship in 1963, was forced out of office in October, 1966 because of a coalition shift brought about by a dispute over balancing the budget. Over the next several years, the press published numerous accounts of West German failures in rocketry, aerospace, and computing.


12 Clausen, Stand und Rückstand, 12.

13 Interview with Walter Doerfler, Cologne, June 23, 2000; Interview with Thomas Trautner, Berlin, April 5, 2000; Interview with Axel Ullrich, Martinsried, April 17, 2000.


15 These accounts are documented in Krieger, "Zur Geschichte von Technologiepolitik," 257-260.

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combination of recession and perceived technological backwardness led many West Germans to believe there was a fundamental economic inequality between Europe and the United States.\(^{16}\)

A French journalist, Jean-Jacques Servan-Schreiber, articulated the threat of this inequality in his 1967 book, *The American Challenge*.\(^{17}\) His goal was to draw attention to the fact that American business was slowly taking over Europe, while Europeans were doing very little in their own defense. Servan-Schreiber provided statistics illustrating that the support for aerospace and electronics research provided by the American government exceeded that of all western European governments combined, both relatively and absolutely. The number of young people receiving a university education was also much higher in the United States, providing a much greater reservoir of skilled workers, engineers, and managers.\(^{18}\) Not simply an instinctive reaction against American dollar imperialism, the author attributed much credit for the American success to America’s innovative management and organization.\(^{19}\) Servan-Schreiber feared that eventually American business (and culture) would completely dominate the European

\(^{16}\) Peter Katzenstein wrote that the extensive series of reforms following the recession of the late 1960s marked a transition in West German political and economic life, from what he calls the first postwar republic to the second. See Katzenstein, *Industry and Politics*, 5-13.


\(^{18}\) Servan-Schreiber, *Die amerikanische Herausforderung*, 81-89.

\(^{19}\) Servan-Schreiber, *Die amerikanische Herausforderung*; Volker Berghahn has recently written that the interpretation of Americanization as a subtle creeping in of consumer culture overlooks the positive side of the relationship—mass production, new management, and marketing. Servan-Schreiber did not overlook this—in fact, it was this part of the relationship that he found most threatening to European economic independence in the long run. Volker R. Berghahn, *America and the Intellectual Cold Wars in Europe: Shepard Stone between Philanthropy, Academy, and Diplomacy* (Princeton: Princeton University Press, 2001), 210-212.
continent. In response, he called for a much higher degree of cooperation between the European nations. He argued that Europeans needed to increase their efficiency by coordinating research internationally, and that they needed to set goals for the future and strategies with which to achieve them.

Since the book echoed concerns that had been growing in the West German research community for several years, it was immediately translated into German and drew attention at the highest levels.\textsuperscript{20} Franz Josef Strauss, the West German defense minister, wrote the forward to the German translation and supported Servan-Schreiber's overall conclusions. Like Servan-Schreiber, Strauss was not particularly inflammatory, but he recognized that American superiority in science and technology would continue to give the US economy a competitive advantage over that of Europe unless the Europeans worked to reverse the situation. He, too, recognized that individual European nations would not be able to match the scale of American research on an independent basis, leading him to agree that greater international cooperation between European nations was an important priority. Gerhard Stoltenberg, Federal Minister for Scientific Research, criticized some of the author's factual errors, but he agreed that the subject was important and praised the author for presenting a positive solution of European cooperation rather than negative anti-Americanism.\textsuperscript{21} The irony of the "American challenge" of the late


\textsuperscript{21} Gerhard Stoltenberg, "Abendlands Untergang (II)?" Review of \textit{Die amerikanische Herausforderung}, by Jean-Jacques Servan-Schreiber, \textit{In Der Spiegel} (1968 #11): 154-157. In December, 1962, the mandate of the Bundesministerium für Atomkernenergie (Federal Ministry for Nuclear Energy, itself established in the mid-1950s) was expanded to include space research and all projects large enough to warrant joint financing by the Bund and Länder. The expanded ministry was renamed the Bundesministerium für Wissenschaftliche Forschung (Federal Ministry for Scientific Research). Stoltenberg became minister in October, 1965. Stamm, \textit{Zwischen Staat}, 244-255.
1960s is hard to ignore. During the early years of the occupation, the basic infrastructure for West German science had been established when the possibility of an excessively centralized, resurgent German state, was the greatest threat. By the late 1960s, however, the American economy seemed to many West Germans to be the biggest threat, necessitating a steady retreat from the science policies they had pursued in 1945-1950.

6.1.2 The Evolution of the Deutsche Forschungsgemeinschaft

One of the first organizations (other than the Kaiser-Wilhelm/Max-Planck-Gesellschaft) consistently to fund the virus research in Tübingen was the Deutsche Forschungsgemeinschaft (DFG, German Research Society). Melchers received Marshall plan funds via this organization in 1950, and within several years he, Freksa, and Schramm were receiving regular support from the DFG for their virus work. As of 1949, the DFG already had a complex history, and its ongoing transformation in the Federal Republic is illustrative of both the centralization of research and the support of molecular biology in West Germany.

Originally formed in October, 1920, by a group of leading intellectuals and political leaders, the DFG began its existence as the Notgemeinschaft der deutschen Wissenschaft (Emergency Association for German Scholarship). The Notgemeinschaft was an organization for the encouragement and support of German scholarship in the disastrous aftermath of World War I. Representatives from all universities, as well as from the major scientific academies and the Kaiser-Wilhelm-Gesellschaft, became members. In 1930, the name of the organization was changed to the Deutsche Forschungsgemeinschaft, and it began to focus on the coordination of research and the financial support of individual research projects. In keeping with the German tradition of

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22 Kurt Zierold to Georg Melchers, October 14, 1950. Zierold promised DM 8,000 of ERP (European Recovery Program) funds for Melchers to equip his new lab. At the time of the letter, the DFG was still going by the name Notgemeinschaft der deutsche Wissenschaften. Melchers Papers, Order 72 (Deutsche Forschungsgemeinschaft).
the independence of scholarship, the DFG was not an organ of the state but rather an independent, autonomous organization of researchers. During the Third Reich, the DFG became thoroughly Nazified and ceased to function after the war.\footnote{The DFG has attracted a significant amount of scholarly attention. For a concise overview, see Scientific and Academic Life, 29-35. The early years of its existence are thoroughly covered in Notker Hammerstein, Die Deutsche Forschungsgemeinschaft in der Weimarer Republik und im Dritten Reich: Wissenschaftspolitik in Republik und Diktatur 1920-1945 (Munich: Verlag C. H. Beck, 1999), but also see Ulrich Marsch, Notgemeinschaft der deutschen Wissenschaft: Gründung und frühe Geschichte 1920-1925 (Frankfurt: Peter Lang, 1994), Thomas Nipperdey and Ludwig Schmugge, 50 Jahre Forschungsförderung in Deutschland: Ein Abriss der Geschichte der Deutschen Forschungsgemeinschaft 1920-1970 (Bonn: Deutsche Forschungsgemeinschaft, 1970), and Kurt Zierold, Forschungsförderung in drei Epochen: Deutsche Forschungsgemeinschaft: Geschichte, Arbeitsweise, Kommentar (Wiesbaden: Franz Steiner Verlag, 1968).} In the late 1940s, many university rectors as well as the cultural ministers of individual Länder began to see the merit in reviving a DFG-type organization. Given the DFG’s unfortunate history under the Nazis, however, they reverted to its original name, the Notgemeinschaft der deutschen Wissenschaften. Beginning in Niedersachsen, the revived organization gradually won favor among university and Länder officials and was officially refounded in January, 1949.\footnote{Stamm, Zwischen Staat, 109-114.} Three months later the Königstein Agreement committed the Länder to the financial support of the Notgemeinschaft.

At the same time, a number of researchers sought an even more centralized research structure. Called the Deutsche Forschungsrat (DFR) and officially founded in March, 1949, the organization quickly received the endorsement of the Max-Planck-Gesellschaft. Werner Heisenberg became the first president of the DFR, and its fifteen members included Adolf Butenandt.\footnote{For an insightful discussion of the brief history of the Deutsche Forschungsrat and the leadership of Heisenberg, see Cathryn Leigh Carson, Particle Physics and Cultural Politics: Werner Heisenberg and the Shaping of a Role for the Physicist in Postwar West Germany (Ph.D. Diss., Harvard University, 1995), 141-156; also see Stamm, Zwischen Staat, 126-141, and H. Eickemeyer, ed., Abschlussbericht des Deutschen Forschungsrates (München: R. Oldenbourg, 1953).} The DFR was remarkable for the ambitions of its members. They wanted their organization to finance research and to represent the
scientific community, but also to serve as a scientific advising body to state and federal governments, to coordinate projects across Länder boundaries, and to represent German research to foreign governments. The DFR was to undertake all of these tasks while remaining completely independent from the state. The expanded scope of activities that DFR leaders hoped to oversee, combined with their desire for independence, represented a rather significant departure in science/state relations in Germany.\textsuperscript{26}

When the DFR was founded, the Notgemeinschaft already existed as a rival organization and enjoyed considerable support at the local and state levels and among university administrators. The leaders of the Deutsche Forschungsrat therefore oriented themselves toward the Federal government. They believed that the national government should be the sponsor of scientific research in the postwar world because many of them—particularly physicists like Heisenberg—recognized that the needs of science had grown beyond the ability of the individual West German Länder to fulfill them.\textsuperscript{27} A trend toward costly big-science projects, especially in physics, had begun before the Second World War, and the members of the DFR knew that physics, rocketry, and aerospace research would be enormously expensive in the postwar world.\textsuperscript{28} The Deutsche

\textsuperscript{26} Carson, Particle Physics, 142-3, 149.

\textsuperscript{27} Carson, Particle Physics, 152; David Cassidy, Uncertainty: The Life and Science of Werner Heisenberg (New York: W. H. Freeman and Company, 1992), 533-534. Since the Max-Planck-Gesellschaft was such a strong advocate of the policies of the DFR, some scholars have suggested that the DFR was in essence a tool that would enable the MPG to bypass the Länder in order to receive the necessary funding from the Federal government. Andreas Stucke, Institutionalisierung der Forschungspolitik: Entstehung, Entwicklung und Steuerungsprobleme des Bundesforschungsministeriums (Frankfurt: Campus Verlag, 1993), 38-40; Hans-Willy Hohn and Uwe Schimank, Konflikte und Gleichgewichte im Forschungssystem: Akteurkonstellationen und Entwicklungspfade in der staatlich finanzierten außeruniversitären Forschung. Schriften des Max-Planck-Institutes für Gesellschaftsforschung, vol. 7 (Frankfurt: Campus Verlag, 1990), 103.

\textsuperscript{28} "Big Science" research had begun in earnest in the 1930s with the growth of particle physics labs, primarily those on the West Coast of the United States. Since then, large scale and high costs have been two of the defining characteristics of science. See Derek J. de Solla Price, Little Science, Big Science (New York: Oxford University Press, 1963); a historiographic overview can be found in James H. Capshew and Karen A. Rader, "Big Science Price to the Present," Science After '40, ed. Arnold Thackray. Osiris 7 (1992): 3-25. For specific studies, the essays by Robert Seidel, "The Origins of the Lawrence Berkeley Laboratory," and Peter Galison, Bruce Hevly, and Rebecca Lowen, "Controlling the Monster: Stanford and
Forschungsrat, modeled after American science organizations, was an effort to reform the German research infrastructure to cope with this reality.²⁹

However, the Notgemeinschaft and the Deutsche Forschungsrat had a difficult time demarcating responsibilities between themselves, which led to considerable frustration on the part of outside groups and donor organizations who could not understand with which organization they were required to deal. In 1951, representatives of both organizations met and decided to merge the two. The resulting organization, renamed the Deutsche Forschungsgemeinschaft, resembled the Notgemeinschaft much more closely than it did the DFR. The new DFG was a politically independent organization funded by public money, but it did not serve to direct research policy or advise governmental bodies. Instead, the DFG continued the policy of distributing grants to individual researchers.³⁰

Despite the failure of the leaders of the Deutsche Forschungsrat to fulfill their ambitions, in subsequent years, members of the DFG recognized the necessity of providing direction for West German research and began to implement policies reminiscent of the DFR's program. In 1952, the DFG initiated a number of Schwerpunktprogramme (Special Emphasis Programs) to recognize and encourage specific fields of research. The procedure was for the DFG leadership to identify the areas of research and then to announce to researchers that funds were available. The individual researchers would then submit their own grant applications for

²⁹ Carson, *Particle Physics*, 143.

³⁰ Ibid., 153-4. Also see Cassidy, *Uncertainty*, 532-537.
Schwerpunktprogramm funds. Disbursement of funds was based on the merit of the applications, and in this way the DFG encouraged specific fields of inquiry without fixing research priorities for the entire country.\textsuperscript{31} Nevertheless, such indirect focusing of research attention was a departure from the historical role played by the DFG. Furthermore, funding the Schwerpunktprogramme while simultaneously providing adequate funds for undirected research required more money than the Länder could provide.\textsuperscript{32} As a result of this foray into focused research, the DFG became financially dependent upon the federal government, which contributed DM 2.4 million in the 1952-1953 fiscal year.\textsuperscript{33}

One of the first areas targeted by the Schwerpunktprogramme was virology, announced in an open letter from Kurt Zierold, General Secretary of the DFG, dated May 13, 1953. Zierold wrote that the federal government planned to make a significant contribution to the budget of the DFG, to be allocated as the DFG saw fit. The senate of the DFG decided that a portion of this money should be used to support research on viral illnesses of plants and animals. Zierold invited interested researchers to send research proposals conforming to these general themes to the DFG, and virus research quickly became one of the most generously funded Schwerpunktprogramme.\textsuperscript{34} By 1956, 89 total proposals had received a total of DM 2,581,000, making virology third in terms of DFG

\textsuperscript{31} Deutsche Forschungsgemeinschaft, \textit{Bericht der Deutschen Forschungsgemeinschaft über ihre Tätigkeit vom 1. April 1952 bis zum 31. März 1953} (Bad Godesberg: Wiesbadener Graphische Betriebe GmbH, 1953), 9-11. The yearly reports of the DFG will henceforth be cited as \textit{Bericht der DFG}.


\textsuperscript{33} \textit{Bericht der DFG vom 1. April 1952 bis zum 31 März 1953}, 85. The Bund could contribute to the DFG without technically violating the constitution by arguing that it was supporting areas of research that the individual Länder could not. By 1953, Bund was contributing more than half of DFG money.

\textsuperscript{34} Ibid., 12; a copy of the letter from Zierold is in the Melchers Papers, Ordner 72 (Deutsche Forschungsgemeinschaft).
financial support, behind only aerospace research and atomic physics. Virology received such strong support for several reasons. In the postwar era, viruses such as the sugarbeet yellows virus caused significant crop losses in West Germany. At the same time, vaccines were becoming a powerful new weapon in the struggle against viral illnesses such as polio, and so in general, there was optimism in the struggle against viral pathogens. Furthermore, Butenandt was a vice-president of the DFG from 1952-1956. Although the position had no duties or responsibilities that were defined in the constitution of the DFG, it is likely that he used his influence to lobby on behalf of the research of his Tübingen colleagues, who benefited from the generosity of the DFG. In late summer, 1953, Zierold promised Melchers DM 39,500 as special assistance for TMV and phage research, and by 1956 Melchers, Freksa, Schramm, and Schäfer were all receiving funds from the Schwerpunktprogramm Virusforschung.

Other Schwerpunktprogramme supported the Tübingen researchers as well. For example, throughout the 1950s Melchers received thousands of marks from the Schwerpunkt program for genetics. In 1963, DFG President Gerhard Hess planned a "Schwerpunktprogramm molekulare Biologie," and solicited advice from Delbrück, Melchers, Weidel, and several others on the kinds of research to be supported. When the announcement for this program was made in 1964, it included six possible areas of investigation—tertiary and quaternary structures of proteins, molecular genetics, cell membranes and walls, molecular basis of muscle contractions, molecular basis of nerve


36 Ibid., 207-208; Zierold to Melchers, August 28, 1953. Melchers Papers, Ordner 72 (Deutsche Forschungsgemeinschaft). Interestingly, the funding for virus research fell within the DFG’s general category of agriculture and forestry research, the same category under which it had been financed during the war years.

conduction, and transduction problems in sensory physiology. In the first three years of its existence, the Schwerpunktprogramm molekulare Biologie disbursed an average of more than DM 2 million annually. In retrospect, the specificity of this proposal appears to be another step toward a more top-down, directed research program issuing from the DFG.

The autonomy of the Deutsche Forschungsgemeinschaft declined throughout the 1960s, as the organization became a partner in a national system of science policy involving scientists and politicians alike. President Gerhard Hess realized that a central advisory board uniting the research community with the federal and state governments was needed to address the problems facing the universities and West German research more generally. Thanks largely to his efforts and a general willingness to cooperate on all sides, the statutes for such an organization, to be named the Wissenschaftsrat (Scientific Council) were drawn up in September, 1957. The determination of members began the following November. Consisting of two commissions, one administrative and one scientific, the Wissenschaftsrat consisted of members delegated by the state and federal governments and members appointed by the West German president based on recommendations from the leading scientific societies. Because so many different groups were represented in the Wissenschaftsrat, it had an advisory rather than a policy function.


Nevertheless, the Wissenschaftsrat’s recommendations carried considerable weight and politicians consistently paid very close attention to them.\textsuperscript{40}

Members of the Wissenschaftsrat strongly suggested that the DFG partner with the federal government and take an active role in reforming the university research infrastructure. Specifically, they recommended the creation of Sonderforschungsbereiche (Special Research Areas) to be funded by the DFG. The idea of the Sonderforschungsbereiche was that individual universities would specialize in areas of research in which they were already strong, build links to non-university research institutes located nearby, and ultimately create a cooperative research network. Sonderforschungsbereiche differed from the DFG’s Schwerpunktprogramme in that they were much more expensive and of much longer duration. They were meant to be large-scale, long-term projects, the success of which would benefit both Bund and Länder.\textsuperscript{41}

Therefore, the Wissenschaftsrat suggested that the Bund and Länder split the special costs associated with the Sonderforschungsbereiche, which was eventually achieved via constitutional amendments in 1969 and 1975.\textsuperscript{42}

Unlike other large, expensive postwar research programs, the Sonderforschungsbereich program focused very heavily on biology throughout its existence. Since the program began in 1968, the biological sciences received the highest share of funds out of all four general categories of research. This money has been used to promote molecular biology, biochemistry, and virology at a number of West German

\textsuperscript{40} For a critical view of the Wissenschaftsrat’s lack of authority, see Ulrich Lohmar, Wissenschaftsförderung und Politik-Beratung: Kooperationsfelder von Politik und Wissenschaft in der Bundesrepublik Deutschland (Bielefeld: Bertelsmann Universitätsverlag, 1968), 63.

\textsuperscript{41} Ibid., 126-140.

\textsuperscript{42} For a documentary history of the Sonderforschungsbereich program, including the constitutional amendments, see Deutsche Forschungsgemeinschaft, Sonderforschungsbereiche: Grundlagen des Förderungsprogramms und Verfahrensregeln (1982), esp. 60-68.
universities. The creation of the Sonderforschungsbereiche was therefore a significant step in the institutionalization of molecular biology research in the Federal Republic of Germany. It was also a significant step in the evolution of West German science policy away from regional autonomy and toward federal direction. Finally, creation of the Sonderforschungsbereiche resulted in a loss in the independence of the DFG. Federalism and scientific autonomy had been diminished in order to meet the needs of a modern, centralized state.

6.2 Molecular Biology in the Max-Planck-Gesellschaft

6.2.1 The Presidency of Adolf Butenandt

In late autumn, 1959, Adolf Butenandt defeated the chemist Richard Kuhn by a decisive margin to become the second President of the Max-Planck-Gesellschaft. According to his colleagues, Butenandt was not Otto Hahn’s first choice as successor (Hahn preferred Kuhn), nor was he particularly enthusiastic about the position himself. He ran for the presidency in response to the urgings of his colleagues in the MPG rather than as a result of his own initiative. Having only recently settled in Munich, Butenandt

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44 The gradual loss of autonomy by the DFG, discussed as a negative trend, is the theme of Treue, "Die Notgemeinschaft."

45 Peter Karlson, Adolf Butenandt: Biochemiker, Hormonforscher, Wissenschaftspolitiker, (Stuttgart: Wissenschaftliche Verlagsgesellschaft mbH, 1990, 221-222; Reimar Lust, "Der Wissenschaftspolitiker Adolf Butenandt," in Adolf Butenandt 1903-1995; MPG Berichte und Mitteilungen (1995 #4): 43. Richard Kuhn, like Butenandt, won a Nobel Prize in the late 1930s but was forced by the Nazi government to reject it. Nevertheless, he supported the Nazi regime rather enthusiastically during the war, without actually becoming a member. After the war there was little evidence that he regretted having sided so clearly with the Nazis. See Ute Deichmann, "Kriegsbezogene biologische, biochemische und chemische Forschung an den Kaiser-Wilhelm-Institut für Züchtungsforschung, für Physikalische Chemie und Elektrochemie und für Medizinische Forschung," in Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus: Bestandsaufnahme und Perspektiven der Forschung, ed. Doris Kaufmann (Berlin: Wallstein Verlag, 2000), 245-257. Emil Witschi was a botanist from the University of Iowa who spent an extended stay in Tübingen in 1949, sponsored by the Rockefeller Foundation. Witschi believed that the wartime conduct of the Tübingen TMV researchers had been acceptable or at least understandable, but in 1949 continued to have reservations about Richard Kuhn’s wartime behavior. Witschi to Gerald R. Pomerat. "Diary of Gerald R. Pomerat, 1949," RAC, Record Group 12.2, Diaries, Box 36, p. 238.
did not wish to leave the city or his institute for Göttingen, and so the office of the
President was transferred to Munich when he began his duties in 1960. Over the next
eight years the general administration of the society followed as well and was eventually
housed in the buildings of the Residenz, the former household of the Wittelsbach family,

As the example of the DFG illustrated, the relationship between state and science
was changing in West Germany in the 1960s, with the Federal government taking an
increasingly assertive role in science policy.\footnote{See Stucke, \textit{Institutionalisierung der Forschungspolitik}, Chapter 2 pp. 35-96 for a theoretical discussion of the growth of the federal government’s influence on science policy.} The most significant change regarding the
Max-Planck-Gesellschaft was the agreement struck between the Bund and Länder on
June 4, 1964, that established the joint financing of research organizations such as the
DFG and the MPG. Actually, the Bund had begun financing the MPG through the
ministry of the interior in 1956, and by the early 1960s promised to provide more than
DM 13 million per year.\footnote{“Anlage zur Niederschrift über die Sitzung des Wissenschaftlichen Rates am 17.5.1960 in Bremen,” 3. Melchers Papers, Ordner 83.} However, rules for obtaining funding through the Bund were
complex, and turf battles between Bund and Länder sometimes threatened to delay the
entire process. The agreement of 1964 regularized matters and allowed for significant
yearly increases in the budget of the MPG during the 1960s.\footnote{Adolf Butenandt, “Förderung der Forschung in Deutschland,” \textit{Bild der Wissenschaft} 3 (1964): 194. The agreement is discussed in Stamm, \textit{Zwischen Staat}, 256-271. For example, in 1965, Butenandt met with Chancellor Erhard and was able to secure a promise for a DM 13 million increase in the budget for the following year. “Erhard sagt Butenandt mehr Geld zu,” \textit{Die Welt} 16 September, 1965.} The Bund also became
involved in the promotion of large-scale research.\footnote{To this end, the Federal Government created a new class of organizations, Grossforschungseinrichtungen (Big Research Establishments, GFEs), with their own unique legal and administrative status. The GFEs were designed to provide the kind of large scale link between academic science and industry that had been largely absent in the miracle economy of the 1950s. Paying for these new institutes required modification}
In general, the Max-Planck-Gesellschaft benefited from these changes in the scientific landscape, and in 1960 Butenandt assumed leadership of a thriving organization. Since the accession of the Tübinger Herren, the shortages of the postwar years had been replaced by a decade of steadily increasing funding and institutional expansion. In 1951, the operating budget for the society had been DM 11.5 million, with DM 2.5 million set aside for new expenditures. In 1961, the respective numbers were DM 48 million and DM 14 million.\(^{51}\) As President, Butenandt endeavored to use West Germany’s scientific resources efficiently, which meant coordinating research within a loose national framework. He did not see political intervention as the means to this end, however; he continued to believe very strongly in the autonomy of research organizations. Instead, he believed that the best way to give West German research policy a greater coherence was to coordinate Max-Planck research activities more closely with other independent research organizations. He also believed that the scientific policy of the MPG itself should be more directed, but only if scientists were the ones making the decisions.\(^{52}\)

\(^{51}\) Of the Grundgesetz on two occasions, resulting in the GFEs being jointly financed, with the Bund providing 90 per cent and the Land in which the institute was located providing 10 per cent. Groβforschung in der Bundesrepublik Deutschland, ed. Arbeitsgemeinschaft der Groβforschungseinrichtungen (Bonn: AGF, 1981). For scholarly discussions, see Gerhard A. Ritter, Groβforschung und Staat in Deutschland: ein historische Überblick (Munich: C.H. Beck, 1992), and the collection of essays edited by Margit Szöllösi-Janze and Helmuth Trischler, Groβforschung in Deutschland (Frankfurt/New York: Campus Verlag, 1990). Of the twelve GFEs created by the late 1970s, two were directly relevant to biomedical research, the Gesellschaft für molekulare biologische Forschung, in Braunschweig, and the Deutsche Krebsforschungszentrum, in Heidelberg. For more information, see GMBF & GBF: Entwicklung eines Forschungsinstitut 1965-1975 (Hannover: Stiftung Volkswagenwerk, 1975), and Gustav Wagner and Andrea Mauerberger, Krebsforschung in Deutschland: Vorgeschichte und Geschichte des Deutschen Krebsforschungszentrums (Berlin: Springer Verlag, 1989).

Butenandt achieved the first of these aims through a network of personal friendships and alliances. While in office, he established very close ties with the leaders of the other important West German research organizations, including Gerhard Hess, President of the Deutsche Forschungsgemeinschaft, and Hans Leussink, president of the Westdeutschen Rektorenkonferenz (West German Conference of University Rectors). Sometimes referred to as the “Holy Alliance” of West German science policy, the affiliation between the men was actually quite informal, consisting of regular discussions on science policy. Furthermore, upon beginning his duties, Butenandt immediately assembled a circle of advisers to guide him on questions of science policy. Such a body had not previously existed in the MPG, and it was all the more innovative in that several of its twelve members were drawn from the Federal government and from industry. Butenandt hoped to encourage industry leaders to enter a more cooperative relationship with the public sector in the joint financing of research in the MPG.

53 Obviously there was a need for common standards and practices among the various universities, and to achieve this without losing sovereignty required the creation of a larger self-governing body. Begun in 1945, this took its final form in 1949 as the Westdeutsche Rektorenkonferenz (Conference of West German University Rectors). This group was composed of members from the universities, Technical universities, and scholarly academies. Its members met to decide upon common policies and also to negotiate collectively with the cultural ministers of the Länder. The Rektorenkonferenz was created to provide a framework within which its members could cooperate on problems of common interest, and its decisions took the form of recommendations rather than mandates. Its president soon became an important representative of the West German research community. *Scientific and Academic Life in Western Germany: A Handbook* ed. Stiftverband für die Deutsche Wissenschaft (Essen, 1957), 49; Geimer, *Research Organization*, 16-18; Stamm, *Zwischen Staat*, 67-70.

54 Butenandt discussed the importance of the ongoing dialogue between the leaders of the major scientific organizations in West Germany before the scientific council of the MPG in the late 1960s. “Niederschrift über die ordentliche Sitzung des Wissenschaftlichen Rates der Max-Planck-Gesellschaft zur Förderung der Wissenschaften e.V. am Donnerstag, dem 27. Juni 1968, um 14.30 Uhr im Neuen Saal des Kurfürstlichen Schlosses in Mainz.” Melchers papers, Ordner 84. Also see Karlson, *Adolf Butenandt*, 226; Lust, “Der Wissenschaftspolitiker Adolf Butenandt,” 49-50. Members of these three organizations also interacted in a more formal setting as members of the Wissenschaftsrat.

55 Karlson, *Adolf Butenandt*, 243; Lust, “Der Wissenschaftspolitiker Adolf Butenandt,” 45. Among the non-MPG members were Carl Wurster, an executive from the BASF chemical company, and Siegfried Balse, Federal Minister for Atomic power.

Centralizing research policy within the Max-Planck-Gesellschaft required a more formal set of measures. In 1964, after several years of efforts, the MPG Senate passed several amendments to the society's constitution. One granted more organizational freedom to individual Max-Planck-Institutes, allowing them to be administered collectively by the directors of their divisions with overall leadership rotating from director to director. Such a structure had already been in place in institutes like the Max-Planck-Institut für Meeresbiologie (Max-Planck-Institute for Marine Biology), so the amendment of 1964 was in essence the sanctioning of a de facto policy. In contrast to this change, which gave more formal autonomy to division leaders within individual Max-Planck-Institutes, the reforms of 1964 also contributed to the centralization of research policy within the society. The reforms strengthened the presidency by giving the president the right to draft the MPG's research priorities. Butenandt believed that such a change was necessary in order to establish criteria for distributing funds within the society. The reforms of 1964 did limit the power of the president as well, most notably by restricting the number of terms by any one person to two.

Although the changes in the Max-Planck-Gesellschaft's constitution were representative of more general trends in West German science policy, they met with some internal resistance. Not surprisingly, Georg Melchers was openly critical of the changes. In general, Melchers was opposed to strengthening the authority of the president if this entailed infringing on the autonomy of the individual institutes themselves. Specifically,

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58 Adolf Butenandt, "Adolf Butenandt im Gespräch." Interview by Peter Friess, Ralf Hahn, Peter Steiner, and Carsten Reinhardt, Munich, 30 September 1993. In Forschung und Technik in Deutschland nach 1945, ed. Peter Friess and Peter M. Steiner (Munich: Deutscher Kunstverlag, 1995) 192-3; also Karlson, Adolf Butenandt, 235-236, and Lust, "Der Wissenschaftspolitiker Adolf Butenandt," 43-44. The exact phrase regarding the President's power to establish research priorities reads "Der Präsident entwirft die Grundzüge der Wissenschaftspolitik der Gesellschaft."
Melchers had long been critical of what he perceived to be Butenandt's authoritarianism. Melchers resisted the proposed changes to the role of the presidency because he thought it was an effort to require obedience from institute directors in place of creativity, and he thought Butenandt was attempting to stamp the institutes of the MPG with his own personality. Melchers even declared that the proposed changes were an effort to implement the Führerprinzip in the Max-Planck-Gesellschaft.\footnote{In his 1948 report on Butenandt's character to the University of Basel, Melchers said that in 1943 Butenandt declared to him that there would never be an institute founded in the Kaiser-Wilhelm-Gesellschaft unless it was done according to the "Führerprinzip," in this case with the leader of the institute having absolute authority over his subordinates. For him, Butenandt's behavior in the 1960s was consistent with his wartime behavior and with an authoritarian streak that Melchers could never tolerate. Melchers to Dr. Miville, June 3, 1948, Melchers Papers, Ordner 1. Also see Melchers to Anton Lang, May 16, 1962, Melchers Papers, Ordner 17.}

There was some truth in this characterization of Butenandt, as he himself readily admitted. Within his lab he was clearly the big boss, although he would delegate tasks to his subordinates.\footnote{Butenandt, "Adolf Butenandt im Gespräch," 190.} One of his former students characterized Butenandt as a scientific patriarch, whose style could clash with people, but wrote that in general Butenandt treated his colleagues very well and created a productive working environment. He took responsibility for the overall direction of work in his lab, but he also took responsibility for providing his students with the resources necessary to develop their own careers, and he remained loyal and supportive to them throughout his lifetime.\footnote{Peter-Hans Hofschneider, "Nachruf auf Prof. Dr. h.c. Adolf Butenandt," \textit{Adolf Butenandt 1903-1995}, 54-59. Also interview with Peter-Hans Hofschneider, Munich, August 2, 2000.} Butenandt believed that the Max-Planck-Gesellschaft as a whole should be run in a somewhat similar style, but there is no evidence that his organizational preferences were any less productive than those of Melchers, who preferred a looser association more along the lines of an ideal scholarly community. The success enjoyed by many of Butenandt's students and their
continuing affection for him years after his death testify that his style was not detrimental to the education and socialization of first-rate scientists.  

Butenandt’s scientific paternalism was perhaps partially responsible for the coherent growth of the MPG in the 1960s, but over the years his authoritarianism clashed more and more strongly with currents of reformist thought in West German society. When the student revolts broke out in 1968, Butenandt was not sympathetic. In particular, he spoke out against the demands of the students to have a voice in the administration of the universities. Many decried him as an elitist, and he became a target for elements of the protest movement. Student demonstrators attempted to disrupt the yearly assembly of the Max-Planck-Gesellschaft in Göttingen in July, 1969, and shortly afterward, a group of students and junior researchers calling itself the “Verband der Wissenschaftler an Forschungsinstituten, e.V.” produced a declaration demanding Butenandt’s resignation. Their primary reason was that they believed that Butenandt was trying to block the process of democratic reform in higher education and research by refusing to allow students a voice in governance. Apparently, some of the younger scientists from the Max-Planck-Gesellschaft were involved with this group, for the declaration made reference to a movement within the MPG to allow younger scientists responsibility in institutional decision making.  

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62 Thus the ongoing clash with Melchers, which has been described as a scientific “Thirty Years War,” was certainly as much a conflict of personalities as it was a conflict between administrative styles. Butenandt and Melchers finally reconciled on Butenandt’s 90th birthday, officially ending the “Thirty Years War,” which in truth was more nearly a fifty years war dating back to 1949. Hofschneider, “Nachruf,” 58; also see photo, p. 41, same volume, Karlson, Adolf Butenandt, 235.  

Reflecting on these issues shortly after he left office, Butenandt stated his reasons for resisting the changes demanded by the 1968ers. "The system which entrusts the running of a research institution to a scholar is decried as being hierarchical," he said. "In contrast to this the aim of democratization is described as being to get rid of 'systems of power' ('Herrschafts systeme'). In saying this people ignore the fact that getting rid of systems of power does not lead to democracy but rather to anarchy." He defended the elitism of science by saying, "In the realm of science, however, it is and always will be the achievements of the individual which in the final analysis will find the way to new knowledge."\(^6^4\) Nevertheless, in his final days in office Butenandt demonstrated considerable flexibility. There was a strong desire among many in the Max-Planck-Gesellschaft to share governance with the younger scientists, and the issue had divided the society. Butenandt knew that his successor, Reimar Lust (who was twenty years younger than he) supported this change. Desiring to leave his house in order behind him, Butenandt supported Lust's position, and as one of his last acts as president of the MPG, he presided over another change to the society's constitution. After 1972, junior scientists were given the right to choose representatives to share decision making with institute directors.\(^6^5\) Thus the authoritarian Butenandt left both a stronger presidency and a more democratic Max-Planck-Gesellschaft behind him in 1972.


6.2.2 The Institutionalization of Molecular Biology Research

With Butenandt as President, modern biology research had a patron at the highest level in the Max-Planck-Gesellschaft. In the postwar years, physics research tended to catch the imagination of the public and of politicians, but Butenandt argued that physics research was often so costly that a country like the Federal Republic could not realistically expect to compete with the resources of the United States. He suggested that the West Germans turn their attention towards fields that were new and exciting, yet not as costly. He singled out molecular biology as one such field. Tübingen provided his justification. He repeatedly presented the work of the virus research group as an example of how the Federal Republic could compete with countries such as the United States in scientific research.\(^{66}\) Owing to strong encouragement from Butenandt and others within the MPG, biomedical research on model organisms (microorganisms, plants, and invertebrates) received the second largest share of the society’s budget, fifteen per cent, second only to physics.\(^{67}\) Biological research benefited not only relative to other branches of research in the MPG, but on an absolute level as well, thanks to a continually increasing budget. When Butenandt left office in 1972, the Max-Planck-Gesellschaft had

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\(^{67}\) In 1960, biomedical research on model organisms (defined as invertebrates, plants, and microorganisms) received a smaller percentage of the society’s budget than biomedical research on mammals and humans and agricultural/breeding research. In 1972, its share of the budget (ca. 15 per cent) was nearly as high as the other two areas combined (ca. 18 per cent). Reimar Lust, “Anatomie der MPG,” Bild der Wissenschaft 10 (1973): 496.
more than fifty institutes, approximately 8,000 employees (of whom 2,000 were scientists), and a budget of more than DM 500 million.\textsuperscript{68}

As a consequence of this prosperity, a second wave of institute building commenced in Tübingen in the mid 1950s. In August, 1956, construction began on a new building for Wolfhard Weidel, Melchers' assistant who had recently been promoted to full scientific member of the Max-Planck-Gesellschaft.\textsuperscript{69} By this time, the Institut für Virusforschung had become too large for the building on Melanthonstrasse, and in 1958 ground was broken for a new set of buildings that was to serve as a more permanent home. Separate buildings were planned for Schramm, Freksa, and Schäfer's divisions; in the early 1960s, a new building was added for Gierer's division for Molecular Biology, and a library building was completed in 1962.\textsuperscript{70} In the late 1960s, Tübingen received another Max-Planck-Institut when Werner Reichardt became leader of the newly created Max-Planck-Institut für biologische Kybernetik (Max-Planck-Institute for Biological Cybernetics). Reichardt had been housed in Melchers' Institute since 1958, but Melchers, Kühn, and Butenandt all recognized the value of his work and joined forces to create an attractive research center for him to persuade him not to leave for the United

\textsuperscript{68} Statistics from the Max-Planck-Gesellschaft website. Not all was perfect in the 1960s, to be sure—the budget of the society actually decreased after the recession of 1966-1967, and did not surpass its peak budget of 1967 until 1969.

\textsuperscript{69} "Auch Max-Planck-Gesellschaft baut: Neues Haus für das Institut für Biologie, Abteilung Dr. Weidel," Schwäbischs Tageblatt, 8 December 1956.

States. With the addition of Reichardt's Institute, a veritable campus for biological research had been built in the hills overlooking the city of Tübingen.

Despite the enthusiastic building in Tübingen, in the late 1950s molecular biology research using microorganisms, especially bacteria, as models was decidedly underdeveloped in the Federal Republic of Germany. TMV had proven to be a wonderful experimental model for studies of proteins and nucleic acids, but since it was a single-stranded RNA virus, its nucleic acid did not recombine during replication. TMV was therefore unsuitable for genetic mapping experiments like those done with bacteriophages. Furthermore, TMV was of no use in exploring genetic regulatory systems along the lines of the Nobel Prize-winning work by the French researchers Jacques Monod and François Jacob. So despite the excellent TMV research, in the late 1950s West Germany was not participating in many of the exciting fields of molecular biology to the same extent that Britain, France, and the United States were. There were pockets of good work, to be sure: Carsten Bresch was training doctoral students in phage genetics in Göttingen; in Tübingen, Weidel did outstanding research on the structure of the cell walls of bacteria, Freksa and Fritz Kaudewitz also used one-celled microorganisms for interesting work on protein synthesis, and Schuster and Walter Vielmetter applied the nitrous acid technique to phages. But for the most part Ute Deichmann's conclusion holds true—"On the whole we can note that research in molecular genetics was slow to develop in the Federal Republic and was carried out on a larger scale at university institutes and institutes of the Max-Planck-Gesellschaft only from the end of the 1950s on."^72

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^71 The creation of Reichardt's institute was an example of the Max-Planck-Gesellschaft promoting truly innovative research. Alfred Gierer recalls that he worked very closely with Reichardt as his own interests began to shift from nucleic acids to developmental biology. Interview with Alfred Gierer, Tübingen, May 23, 2000; also see Chapter 5, Section 5.2.2 for more on the creation of Reichardt's institute.

Biological researchers in the Max-Planck-Gesellschaft noted the relative lack of molecular biology research in their country and worked to rectify the situation. Expanding the institutes in Tübingen was a first step. Another, more ambitious step was the decision to change the research emphasis in Hans Nachtsheim’s Max-Planck-Institut für vergleichende Erbbiologie und Erbpathologie (Max-Planck-Institute for Comparative Heredity and Hereditary Pathology) in Berlin.\(^{73}\) When Nachtsheim retired in 1960, the Max-Planck-Gesellschaft decided to replace him with someone whose interests were more in touch with international developments in molecular biology.

They chose Fritz Kaudewitz, one of Freksa’s former assistants. Kaudewitz had excellent international connections, having been a guest at the Cold Spring Harbor department of Genetics as early as 1954. He had spent more time in America in the late 1950s, and fear of losing him to the US combined with respect for his research motivated the MPG to give him the leadership of the Berlin institute. Unfortunately, he proved to be a short-term solution. He was an outstanding teacher, and the absence of teaching possibilities coupled with his isolation from colleagues in Berlin (the construction of the Berlin Wall in August, 1961, severed contact with the Humboldt University) led him to accept a position at Munich University in 1962.\(^{74}\) He was scheduled to leave the MPG in July, 1964.

\(^{73}\) Nachtsheim’s institute was the successor of the Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre, und Eugenik, where medical experiments on samples from concentration camp victims had been performed during the war. Nachtsheim himself was cleared of any wrongdoing and continued to study human genetics in the postwar era. For Nachtsheim’s institute, see Hans-Peter Kröner, \textit{Von der Rassenhygiene zur Humangenetik: Das Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre, und Eugenik nach dem Kriege} (Stuttgart: Gustav Fischer Verlag, 1998), esp. 174-235. For continuities in Nachtsheim’s eugenic thought, see Peter Weingart, Jürgen Kroll, and Kurt Bayertz \textit{Rasse, Blut, und Gene: Geschichte der Eugenik und Rassenhygiene in Deutschland} (Frankfurt: Suhrkamp Verlag, 1988), 593-602.

\(^{74}\) Lebenslauf preceeding Fritz Kaudewitz, “Genetische Grundlagenforschung-Heute,” \textit{MPG Jahrbuch} (1963) 33-63. Kaudewitz’s skills as a teacher were discussed by Joseph Straub of Cologne University, letter, Straub to Brauner, November 4, 1960; Melchers Papers, Ordner 15, as well as Freksa in a meeting to discuss the future of the institute after Kaudewitz’s departure. “Protokoll über die Sitzung der Kommission ‘Wiederbesetzung einer Direktorstelle am Max-Planck-Institut für vergleichende Erbbiologie und
This relatively rapid turnover suggested to the leaders of the Max-Planck-Gesellschaft that they needed to think very carefully about how to proceed in Berlin. In November, 1962, they appointed a commission to develop a plan for the future of the institute. The commission began meeting in April, 1963, and the members quickly determined that Berlin had unique problems that had contributed to Kaudewitz’s short tenure there. By this time, Berlin was not merely isolated—the western portions of the city were enclosed in a wall more than fifty kilometers inside East Germany. Attracting first-rate students and technical assistants to work there was exceedingly difficult, and there were not enough other research institutes in the area to create a scholarly community. Early in the discussions, one of the commission members asked, “Good people do not come to Berlin. Why should someone work there, where they will have no co-workers?”

And obviously, the Cold War and Wall created a threatening atmosphere that also discouraged anyone from settling in West Berlin.

The easiest solution would have been to move the institute; in fact, one of the members of the commission suggested moving it to Munich and allowing Kaudewitz to continue as director with a joint appointment at Munich University. The commission was charged, however, with maintaining the presence of the Max-Planck-Gesellschaft in Berlin. Scientific research in Berlin had already declined dramatically, and the leaders

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76 It was difficult to attract talented persons of all kinds to walled-in Berlin, not just scientists, which led the West German government to create a number of financial incentives, including pay supplements and tax reductions, to encourage people to live in West Berlin. According to David Clay Large’s insightful chronicle of the city, “West Berliners called this complex of incentives ‘our Zitterprämie’ (jitters premium)—the bribe they got for taking the risk of living on a tiny island in the big Red sea.” David Clay Large, _Berlin_ (New York: Basic Books, 2000), 464.

77 “Unsere Aufgabe ist nun, hier in Berlin für die Max-Planck-Gesellschaft etwas zu machen,...” “Kommission...am 18.4.1963,” 3, quote from 5.
of the Max-Planck-Gesellschaft did not want to abandon the city altogether. Therefore the commission had a significant non-scientific factor with which to grapple. The potential solution was suggested by Freksa and Melchers, who made up part of a disproportionately large Tübingen contingent involved with the future of the institute. They believed that the institute had to be a small, self-sustaining scientific community in itself. They therefore patterned their suggestion after the two Max-Planck-Institutes in Tübingen. Their goal was to create three separate but mutually complementary divisions, each with a single director. All three directors would share overall administrative responsibilities on a rotating basis. Melchers and Freksa suggested staffing the new institute with young scientists who already had excellent professional connections that they would be able to maintain in the relative isolation of Berlin.78

Their model was generally agreeable, but the specifics took longer to ascertain. The most immediate task was to define the scientific goals of the institute prior to determining potential candidates to lead it. On this point, the commission was split. At their April 18 meeting, several suggested that the institute focus on mammalian or human genetics, areas of research similar to those of Nachtsheim. Melchers and Freksa, however, were of the opinion that the Max-Planck-Gesellschaft needed to promote scientific research that did not exist elsewhere in Germany. One of the MPG's justifications for remaining independent of the universities was that it could move more quickly to develop new fields of research than the universities could. In the case of molecular biology research, the rest of the world seemed to be passing by West Germany. Therefore, the Tübingen researchers were committed to founding an institute that would pursue the newest techniques in microbial genetics. Freksa suggested the term molecular genetics would be broad enough to allow diversity among the three divisions while still

78 Ibid., 9-10.
providing them with enough common purpose to work constructively with one another.\textsuperscript{79} Several weeks later, a second group (including Schramm and Kaudewitz) reviewed the commission's suggestions and agreed with them, with the exception of Boris Rajewsky, who officially protested that mammalian genetics was to be excluded from the new institute.\textsuperscript{80} Despite this resistance, the plan for an institute for molecular genetics went forward. The next step was to find candidates to lead the new institute's three divisions.

The commission discussed several possible candidates and produced a list that was made up almost entirely of young scientists from the Max-Planck-Gesellschaft itself. Disturbed, one of the members described it as an "incest list," an assessment with which the others agreed; however, they also agreed that there were very few good candidates in the universities.\textsuperscript{81} After several more meetings, in early 1964 the commission recommended three people to the Senate of the Max-Planck-Gesellschaft—Heinz-Günter Wittmann, Heinz Schuster, and Gunther Stent.\textsuperscript{82} The choices of Wittmann and Schuster were not surprising. Wittmann had received an offer from Pasadena, which he had declined, but nevertheless offering him a promotion seemed the best way to reserve his talents for the Max-Planck-Gesellschaft and the Federal Republic of Germany. Melchers said, "If I let Wittmann go, I will be letting go of the best that I have, but on the other hand Berlin is so important that I would do this."\textsuperscript{83} Schuster was also a natural choice—

\textsuperscript{79} Ibid., 11.

\textsuperscript{80} "Protokoll Sektionssitzung vom 14.5.63," MPG-Archiv, II. Abt., Rep. 1A, MPI für molekulare Genetik.

\textsuperscript{81} Ibid., 28. Freksa said that if there were any potential candidates from the universities who were qualified, he would have been happy to call them, Melchers supported him, saying that outside of Cologne university there was no one. The reasons for Cologne's uniqueness in this respect will be discussed later in this chapter.


\textsuperscript{83} Ibid., 13.
he had been one of the individuals responsible for determining the exact mechanism of the nitrous acid/RNA reaction, which explained Gierer and Mundry’s results in precise chemical terms.

The decision to pursue Gunther Stent was significant for several reasons. For the sake of avoiding excessive intellectual inbreeding, it made sense to offer one of the three positions to someone outside the Max-Planck-Gesellschaft (and someone who had not trained in Tübingen). In addition, Stent was a Jewish émigré who had fled his native city of Berlin in the late 1930s. Since that time he had worked closely with Max Delbrück before becoming a professor at Berkeley. He was one of the most widely respected molecular biologists in the United States, his scientific credentials were impeccable, and so he would have instantly brought international credibility to the new institute. Some believed that Stent’s connection to the city of Berlin might lure him away from Berkeley, and the thought of attracting a former émigré back to West Germany must have carried a certain satisfaction with it as well. Stent was honored by the offer, but for personal and professional reasons he declined it, and instead he became a foreign scientific member of the institute.84

Stent’s decision to decline the directorship prompted another round of searching, which quickly narrowed to two candidates—Thomas Trautner and Peter Starlinger. Both were known as superb researchers and teachers, and both had excellent international reputations. Rather than choose between them, the MPG elected to offer both men directorships, understanding that if both accepted, a fourth division would be added to the new institute in Berlin. At the time Starlinger was involved in helping create and

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84 Stent’s early scientific career is discussed in his memoirs, Nazis, Women, and Molecular Biology: Memoirs of a Lucky Self-Hater (Kensington CA: Briones Books, 1998). Melchers thought very highly of Stent and encouraged him to accept the offer; Stent explained to him his reasons for declining. See the correspondence between the two, Melchers Papers, Ordner 20. Also see Stent to Adolf Butenandt, April 2, 1964, Melchers Papers, Ordner 21.
maintain the first modern molecular genetics department at a West German university—
Cologne University's Institute for Genetics, and therefore declined the offer. Trautner
accepted, providing the society with a third researcher with outstanding research skills
and excellent international connections. Beginning in 1951, Trautner had studied phage
genetics in Göttingen with Carsten Bresch.\textsuperscript{85} In 1953 he, like Alfred Gierer, became one
of the first German Fulbright exchange students to the United States and spent 1953-1954
working with Salvador Luria (who with Delbrück and Alfred Hershey had founded the
American phage group) at the University of Illinois.\textsuperscript{86} After completing his doctoral
work in 1957, Trautner worked in the microbiology division of the Botanical Institute of
Cologne University. Thanks to a stipend from the Deutsche Forschungsgemeinschaft, he
spent more than a year from 1960-1962 working at Arthur Kornberg's lab in Stanford.\textsuperscript{87}
After a brief return to Cologne, Trautner accepted an assistant professorship at the
University of California, Berkeley. In 1965, he chose to leave Berkeley in favor of
Berlin.\textsuperscript{88}

\textsuperscript{85} The example of Carsten Bresch illustrates that molecular biology work could be done in the Federal
Republic, even though it was not fully institutionalized. According to Bresch, it was somewhat of an
accident. He and a friend heard Delbrück talk in Berlin in 1947 and became interested in phage genetics.
Delbrück encouraged them, and thanks to his intervention his brother in law, Karl-Friedrich Bonhoeffer,
gave Bresch space at the Max-Planck-Institute für Physik in Göttingen, which is where Trautner learned the
subject (the institute has since moved to Munich). Interview with Carsten Bresch, Freiburg, June 9, 2000.

\textsuperscript{86} Luria was also an émigré, driven from Italy to France, and then to the United States. He shared the 1969
Nobel Prize for medicine or physiology with Delbrück and Hershey for their work on the genetic structure
and replication of bacteriophages. See Salvador E. Luria, \textit{A Slot Machine, a Broken Test-Tube, an

\textsuperscript{87} Kornberg shared the 1959 Nobel Prize for medicine or physiology with Severo Ochoa for their work on
the enzymatic synthesis of nucleic acids. Kornberg is best known for his isolation of the enzyme DNA-
polymerase I. For his career, see Arthur Kornberg, \textit{For Love of the Enzymes: The Odyssey of a Biochemist}

\textsuperscript{88} Biographical material from an interview with Thomas Trautner, Berlin, April 5, 2000; Trautner's
\textit{Lebenslauf}, and a \textit{Gutachten} (letter of recommendation) from Carsten Bresch, dated October 6, 1964. Both
documents MPG-Archiv, II. Abt., Rep. 1A, MPI für molekulare Genetik.
With the scientific goals and the leadership established, construction of the new set of buildings finally began in Dahlem in 1968. It was completed in 1970 at a cost of DM 13 million. Schuster’s division focused on DNA replication and its associated enzymes in bacteria and viruses, Trautner’s on the regulation of DNA synthesis, bacterial conjugation, and the biological activity of isolated nucleic acids, and Wittmann’s group investigated the structure and function of ribosomes and the molecular mechanisms of protein synthesis more generally. The institute attracted students and junior researchers from around the world and quickly achieved an international reputation for excellence.

While the Institut für molekulare Genetik was completed in Berlin, an even more ambitious project took shape around Butenandt’s Institut für Biochemie in Munich. In the early 1960s, there were actually three Max-Planck-Institutes dedicated to biological topics in Munich—Butenandt’s, the Max-Planck-Institute für Zellchemie, led by Feodor Lynen, winner of the 1964 Nobel Prize in chemistry, and the Max-Planck-Institute für Eiweiß und Lederforschung, led by Wolfgang Graßmann. In an effort to recreate the research climate of Dahlem, the leaders of the institutes hoped to combine them as part of an integrated scientific campus which would allow which individual researchers and their institutes to work together while still retaining their autonomy. The new collective retained the name of the Institute for Biochemistry. The MPG leadership was able to procure a sufficiently large piece of land (roughly 37 hectares) in the town of Martinsried, southwest of the Munich city center. This location had the added attraction of being very close to the clinic of Munich’s Ludwig-Maximilians-Universität, opening up the possibility of collaborative biomedical research in a hospital environment. Work on the new complex of buildings began in the late 1960s.


In 1973, the massive new Max-Planck-Institut für Biochemie was completed. In keeping with the changes in institutional organization implemented in the 1964 reforms, the institute did not have a single director, but was run cooperatively by the leaders of its divisions, with one serving as acting director at any given time. The institute contained twelve relatively autonomous divisions, many of which pursued research in molecular biology topics including gene function, peptide and protein biochemistry, and virus research. Two of its division leaders, Wolfram Zillig and Gerhard Braunitzer, had been integral parts of the Tübingen research group nearly two decades earlier. The reconfigured Institut für Biochemie, in cooperation with the university clinic and the natural sciences faculty of the university (which were moved to Martinsried in the 1990s to create a high-tech subcampus of the LMU) have since become the Federal Republic of Germany’s leading biotechnology research center.

The Max-Planck-Institut für Biochemie exemplified of a number of trends in West German molecular biology in the early 1970s. It demonstrated the centrality of biochemistry to molecular biology, an ongoing theme of this study. Furthermore, the collegial structure of the institute marked a shift in the organization of Max-Planck-Institutes. The leadership of the Kaiser-Wilhelm-Gesellschaft had chosen outstanding researchers and built institutes around them. Too strong an emphasis on this “personality principle” was incompatible with the interdisciplinary nature of molecular biology research and with modern team-based research in general. The MPG still pursued the

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91 “Max-Planck-Institut für Biochemie: Forschungsgebiete und Struktur,” Martinsried, April, 1976.

92 “Martinsried als Markenzeichen: Streiflichter zum 25-jährigen Bestehen des Max-Planck-Institutes für Biochemie in Martinsried.” Manuscript edited and prepared by Peter-Hans Hofschneider and R. Tatzel for the twenty-fifth anniversary of the Institute for Biochemistry in Martinsried. I would like to thank Professor Hofschneider for sharing this source with me.

93 When Butenandt had first taken office in 1960 he initially declared that he would adhere to this principle, often called the “Harnack principle” after Adolf von Harnack, first president of the Kaiser-Wilhelm-Gesellschaft. This idea of targeting a great researcher and building an institute around them had become a
best researchers but now created an environment in which they could combine their skills if they so chose. Thus the new institutes in the Max-Planck-Gesellschaft, both in Berlin and Martinsried, represent a modernization of West German research. Finally, the freedom given to individual divisions in this kind of organization allowed a variety of projects to be pursued in a single institute, which meant that molecular biology research was no longer confined to specialized institutes. By the early 1970s, then, it was impossible to speak of one center of molecular biology research in West Germany, but instead of various areas, such as Munich, Heidelberg, Göttingen, Cologne, and Berlin, where the concentration and excellence of research were unusually high.

6.3 Molecular Biology and the West German Universities

6.3.1 The “Fatal Division”

As my narrative has shown, molecular biology research was not well institutionalized in the Federal Republic of Germany until the 1960s, despite the fact that an innovative program had existed there since before World War II. The international character of molecular biology and its conspicuous absence in West Germany has led Ute Deichmann to conclude that the damage done to relations with foreign scientists during the Nazi era was the greatest single factor inhibiting the development of molecular genetics in the Federal Republic. In her pioneering study on the impact of Nazism on biological research, she concluded,

After 1945 it became clear that biological research in Germany was lagging behind research in Western countries. The present study has shown that this lag did not—as has frequently been assumed—primarily result from National

\[\text{myth in the MPG by the 1960s, and as the organization of the Institutes for Biochemistry and for Molecular Biology show, it was not compatible with modern, interdisciplinary research. See Rudolf Vierhaus, “Bemerkungen zum sogenannten Harnack-Prinzip. Mythos und Realität,” in Das Harnack-Prinzip, 129-138.}\]
Socialist science policy following the expulsion of the Jews. Instead, it is attributable above all to the moral failure of German scientists after 1933 and the later international isolation that ensued.\textsuperscript{94}

Deichmann's conclusions, particularly regarding the impact of National Socialist science policy, are of course valid. Yet my reconstruction of the history of the German TMV community demonstrates that other factors helped to determine the course of events in West Germany as well. More fundamentally, the lag in molecular biology research was the consequence of the inability of an aging, conservative research structure to support a dynamic and fundamentally new style of scientific research.\textsuperscript{95} International isolation and the devastation of the war certainly exacerbated this phenomenon, but neither is sufficient to explain it fully.

My research indicates that in general, West German researchers were often ignored because prior to the mid-1950s they had very little to say that was of interest to the international community. The isolation of German scientists during and after the Second World War did have an extremely negative impact on their work, but they were able to produce a minimal level of quality research anyway. Immediately after the war, contact with foreign colleagues was sporadic, but by the late 1940s/early 1950s, regular correspondence and exchange had been re-established in a number of specific cases. Overall, West German contributions to international molecular biology meetings and journals continued to be minimal, but when the researchers there did have something interesting to say, scientists in other countries were willing to listen. For example, the international community quickly accepted the work of Gierer and Schramm because it

\textsuperscript{94} Deichmann, \textit{Biologists Under Hitler}, 319.

\textsuperscript{95} A similar situation existed in physics research in Germany even before the Nazi takeover. After the stunning successes of German physicists in the first part of the century, by the 1930s many investigators began to believe that they no longer had anything to contribute to their colleagues in America, who had surpassed them. See Alan D. Beyerchen, \textit{Scientists Under Hitler: Politics and the Physics Community in the Third Reich} (New Haven: Yale University Press, 1977), 202.
was scientifically significant. Therefore, the lack of more extensive correspondence and exchange was at least partially the result of, rather than the cause of, the underdeveloped state of biology research in the Federal Republic of Germany.

All of the molecular biology research groups discussed in this study—Weidel and the TMV group in Tübingen, Carsten Bresch in Göttingen—were established by the late 1940s/early 1950s, a full decade before molecular biology began to be institutionalized in West German universities. The question now becomes, why was so little noteworthy work done outside of these groups throughout the 1950s? The TMV group is evidence that National Socialist science policy was not the problem. Nor in fact was the legacy of the abuse of biology under National Socialism. Outside of a failed effort to purge the teaching of biology from the gymnasium curriculum because of its association with Nazism, biology, and especially molecular biology, were unburdened by the past. 96 Research on organisms such as TMV, phage, and E-coli was morally unproblematic and seemed far removed from the medical experiments of Auschwitz. Of the many scientists interviewed for this study, none believed that the biological crimes of the Nazis had had a detrimental effect on biological research in West Germany in the 1950s and 1960s. 97

While the devastation of the war should not be dismissed, it should not be overestimated either. Most of the West German universities were functional by the early

96 “Die Biologie: Nicht mehr teil Moderner Bildung?” Denkschrift des Verbandes Deutscher Biologen zur Lage des Unterrichts in Biologie an den Gymnasien der deutschen Bundesrepublik.” This paper was sent to Melchers, which he acknowledged in a letter to Dr. R. Kaplan, on August 29, 1960, and was prepared as a response to a recent conference of the Cultural Ministers of the West German Länder. The ministers suggested removing biology as a required subject in “Unter- und Oberprima” in ALL Gymnasiums in West Germany. They believed that biology could lead one to dangerous world-views, requiring the biologists to use full page of their response to defend biology from the crimes of the Nazis. They wrote “Der naziistische Rassenwahn ist keine Erkenntnis der Biologie oder eine notwendige Folge der Darwinschen Theorie,” p. 3. Melchers Papers, Ordner 15.

97 This situation changed in the past two decades. The growing influence of environmental groups such as the Greens, the recognition of the tainted past of the German scientific and medical communities, and the development of therapeutic uses for molecular biology research via recombinant DNA technology, all have made the pursuit of molecular biology and biotechnology more politically charged in the Federal Republic of Germany than elsewhere.
1950s, though repairing their infrastructure was an ongoing task. Of much greater importance was their organizational structure, which for the most part made them uniquely inhospitable to molecular biology research as it had developed internationally. As discussed above, molecular biology was characterized by a lack of respect for boundaries, whether national or disciplinary. The restored West German universities were an unlikely place for such a style to flourish. Since, as noted above, university professors and officials re-established their organizations with their thinking fixed firmly on the nineteenth, rather than the twentieth century, they inadvertently kept the new biology out of the universities until the 1960s.

West German biology departments maintained what Max Delbrück called “the fatal division” in the biological sciences, the separation of botany from zoology, to which I have already alluded repeatedly.\textsuperscript{98} Prior to the war, this structure had caused important new areas of study, such as physiology and microbiology, to become established in medical schools rather than university departments, and Delbrück saw the same thing happening with molecular biology. In a talk given in West Germany in 1956, he argued that it was because of this structure that the best molecular biology research had been done outside of the university environment—for example at the Pasteur Institute, the Rockefeller Institute, the Cavendish Laboratory for Physics in Cambridge, and the Max-Planck-Gesellschaft.\textsuperscript{99} He noted that this was an international problem, not unique to

\textsuperscript{98} Letter from Delbrück to the Cultural Minister of the state of Northrhine Westphalia, Dr. Freiherr von Medem, dated 5/25/59. Delbrück Papers, Carton 26, Folder 22.

\textsuperscript{99} Max Delbrück, “Neue Bahnen biologischer Erkenntnis,” Delbrück Papers, Carton 21, Folder 5. This same inflexibility had slowed the acceptance of morphology into German universities in the nineteenth century. Since no new positions for morphology were created, investigators interested in this area had to “colonize” other fields including zoology, anatomy, and physiology. See Lynn K. Nyhart, \textit{Biology Takes Form: Animal Morphology and the German Universities 1800-1900} (Chicago: University of Chicago Press, 1995).
Germany, but clearly the expulsion of Jewish scientists and the isolation and destruction of the war made the problem most severe in the Federal Republic.

The leaders of the Deutsche Forschungsgemeinschaft agreed with Delbrück. In 1957, they distributed a questionnaire to a number of leading biological researchers asking for an assessment of the state of biology research in the Federal Republic. The resulting study, published in 1958, painted a bleak picture. The report indicated that in certain areas, such as microbiology, West Germany was quickly approaching the status of an undeveloped country. One of the explanations for this situation (and an obstacle in the way of remediying it) was the structure of university biology departments. The report noted that most universities had one professor for botany and one for zoology, each of whom was responsible for covering all the new developments in their disciplines (see Table 6.1). Seven years later, the situation had scarcely improved. In 1963, Gerhard Hess sent out another round of surveys on the state of West German research. He presented some of the results in a 1964 talk entitled "Strukturprobleme unserer wissenschaftlichen Hochschulen" (Structural Problems of our Universities). The input of leading West German researchers had convinced Hess that the fragmented disciplinary structure of the university faculties was largely responsible for their lack of research productivity. He spent much of his time discussing large-scale research and physics, but addressed biology as well, and he told his audience that one simply could not do the new biology in West German universities.101


101 Gerhard Hess, "Strukturprobleme unserer wissenschaftlichen Hochschulen," speech given at the University of Münster, 1/23/64, on the occasion of the meeting of the Friedrich Nauman Stiftung. Hess had sent the initial questionnaire out on March 8, 1963; Georg Melchers received one and responded to Hess on April 5. Hess sent a thank you to Melchers on April 25, then on May 28 sent an invitation to all of his respondents to meet in June to discuss the results. All correspondence Melchers Papers, Ordner 72 (Deutsche Forschungsgemeinschaft).
Table 6.1: Biology Professorships in Natural Science Faculties\textsuperscript{a} of West German Universities, 1960

<table>
<thead>
<tr>
<th>University</th>
<th>Chair\textsuperscript{b}</th>
<th>Botany</th>
<th>Zoology</th>
<th>Genetics</th>
<th>Biochemistry</th>
<th>Phys. Chem.\textsuperscript{c}</th>
<th>Microbiology</th>
<th>Biophysics</th>
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<tr>
<td>FU Berlin</td>
<td>Full Associate</td>
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<tr>
<td>Erlangen</td>
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<tr>
<td>Frankfurt</td>
<td>Full Associate</td>
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<tr>
<td>Freiburg</td>
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<tr>
<td>Göttingen</td>
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<td>Hamburg</td>
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<td>Kiel</td>
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<td>Cologne</td>
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<td>Mainz</td>
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<tr>
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<td>Münster</td>
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<tr>
<td>Saarland</td>
<td>Full Associate</td>
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<tr>
<td>Tübingen</td>
<td>Full Associate</td>
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<tr>
<td>Würzburg</td>
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<td>1</td>
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</tbody>
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\textsuperscript{a}Excludes Medical School/Veterinary School Faculties
\textsuperscript{b}For type of position, I am using the (very) approximate translation Ordinariate = Full Professor, Extraordinariate = Associate Professor
\textsuperscript{c}Physiological Chemistry

NOTE: Within faculty departments and institutes many people other than the full professor, including adjunct faculty, doctoral students, and scientific and technical assistants, worked and taught. Nevertheless, since the full professor had relatively complete control over research priorities, the above table gives a rough approximation of the kinds of biological research taking place in the West German universities.
In addition to its having no clearly defined place in the traditional university departmental structure, molecular biologists were also voraciously annexationist, gobbling up intellectual territory long held to be the province of other disciplines. By appropriating techniques, objects, and concepts from previously existing disciplines and making them their own, molecular biologists posed a threat to the prestige of older disciplines. Historian Pnina Abir-Am has explored this phenomenon at length, arguing:

The tenor of the criticism suggests that molecular biology posed a threat to a wide range of disciplines, because it was redefining, and hence appropriating, many concepts, both central and peripheral, around which the "classical" disciplinary monopolies were constituted. The concepts of "virus" and "gene," for example, are central to the disciplines of virology and genetics. These concepts were redefined when the concept of the structure of nucleic acids (especially DNA and the variety of RNAs) was elevated from a peripheral position within biochemistry... to the center of molecular biology, as carriers of biological information. These changes forced biochemistry, virology, and genetics to redefine the meaning of their classical concepts and hence their scientific authority.102

We have seen that biochemists responded to the molecular biology challenge by learning the language of molecular biology and recasting their basic concepts in terms of this new information science discourse.103 Klaus Munk, a virologist who worked with Werner Schäfer in the Max-Planck-Institut für Virusforschung in the 1950s, noted an analogous change in virology. Munk studied medicine before turning to virology, and initially he approached viruses as pathogens to be handled within the framework of


103 See Chapter 5, Section 5.2.1. Also, biochemistry itself was largely absent from West German university chemistry departments in the 1950s—by the mid 1950s there were only two chairs for biochemistry in all the entire West German system. Dieter Beherens, ed., *Denkschriften zur Lage der Deutschen Wissenschaft: Herausgegeben von der Deutschen Forschungsgemeinschaft. Band I: Denkschrift über die Lage auf dem Fachgebiet Chemie unter besonderer Berücksichtigung der Universitäten und Hochschulen* (Wiesbaden: Fritz Steiner Verlag, 1957), 21, 32-33.
Koch’s postulates. Wendell Stanley’s work, and that of Munk’s colleagues in Tübingen, encouraged a shift toward approaching viruses from a biochemical (later molecular biology) perspective. As a result, in the Federal Republic at the end of the twentieth century, few medical students (who had clinical training and an understanding of disease) seemed to be interested in virology. Instead, biochemists and molecular biologists dominated virology. The entire focus of the discipline had shifted away from medicine as a result of the molecular revolution in biology.\textsuperscript{104}

6.3.2 Structural Change

In addition to the problems discussed above, biological researchers in the Federal Republic had an additional hurdle to clear in the form of the authority of university professors, who were typically also the directors of university research institutes.\textsuperscript{105} Within their own institutes, their authority was difficult for younger scientists to challenge. This strongly hierarchical system could stifle cooperative research among the junior scientists in university research institutes and made them dependent upon the institute director for patronage and support. Looking at the US, West Germans came to believe that improving the situation required creating an environment in which researchers with different specializations could work together in relative equality, as a team. As I have noted, biological researchers and their supporters in organizations such as the DFG recognized this situation as early as the mid-1950s, and began to advocate a significant reform of the West German universities well before the West German students did.

\textsuperscript{104} Interview with Klaus Munk, Heidelberg, June 20/22, 2000. Dr. Munk believes that medical virology has almost disappeared as a discipline. He told me that the difference between medical and biochemical virology was illustrated by the styles of research in the Institut für Virusforschung. Schramm, Freksa, and their colleagues used viruses as experimental systems, while Werner Schäfer, the third director (who trained as a veterinarian) used a medical approach.

\textsuperscript{105} Several universities had created “parallel chairs” allowing them to have two full professors in either botany, zoology, or both. Refer to Table 6.1.
The DFG's 1957 Denkschrift zur Lage der Biologie recommended expanding the number of university chairs in each biology department, but also abolishing the "One Man Institute" in favor of a more collegial research environment.\textsuperscript{106} The author suggested increasing the number of both full and associate professor positions and making them independent leaders of their own divisions. When these divisions were housed within a single building, one of the directors should be named to the administrative post of acting director of the entire institute. This arrangement would give individual leaders control of their own budgets and research priorities while still allowing coherent management of the entire enterprise. The DFG based these recommendations on comments solicited from leading biologists, who in turn based their own suggestions on the structure of biology departments in the United States and the MPG.\textsuperscript{107}

The kinds of reforms suggested by the DFG were echoed in an influential report published by the Wissenschaftsrat in 1960, which touched off a major wave of university construction.\textsuperscript{108} A later report, published in July, 1967, called for improving the research of West German universities by linking them more closely with industry and with research organizations such as the Max-Planck-Gesellschaft.\textsuperscript{109} The authors of the report noted that biology was a subject in transition, changing from the two traditionally separate disciplines of botany and zoology to a much more collaborative, integrated discipline comprising biochemistry, genetics, microbiology, botany, zoology, and

\textsuperscript{106} Meyl, Denkschrift zur Lage der Biologie, 42-46.

\textsuperscript{107} Ibid., 43, 45. Also see Gerd Klasmeyer, Biomedizinische Technik. Mit einer Studie aus den USA, erstattet für den Direktor der National Institutes of Health (Göttingen: Vandenhoeck und Ruprecht, 1969) 17-33.


\textsuperscript{109} Wissenschaftsrat, Empfehlungen des Wissenschaftsrates zum Ausbau der Wissenschaftlichen Hochschulen bis 1970 (Bonn: Bundesdruckerei, 1967).
anthropology. Interdisciplinary research of this nature was incompatible with the structure of the West German biology departments, necessitating a change in both university structures as well as in the ingrained attitudes of many West German academics.\textsuperscript{110}

As new universities were built, their departmental structures were designed to incorporate many of the suggestions made by the DFG and the Wissenschaftsrat. In a 1968 progress report, biology was used to provide an illustration of the new type of faculty structure. In the ideal department, investigators from biology and theoretical medicine were to work together on a regular basis. Such a department would contain approximately twelve positions in general biology (including physiology, genetics, molecular biology, biophysics, microbiology, and cybernetics), seven positions in theoretical medicine, and roughly four or five more positions in special/developmental biology.\textsuperscript{111}

Of course, this ideal model did not become reality. Faculties in existing universities resisted such change, protesting against the fragmentation of the discipline entailed in the creation of so many different positions within biology. Even in new universities, the model outlined by the Wissenschaftsrat could be implemented only in a very conservative manner. For example, the newly created University of Bochum proposed to create a biology department with nine equal positions. However, two positions, general zoology and general botany, were to be filled first, and these two professors were to be chosen based on their previous university experience. The botanist and zoologist were then to be responsible for basic education in botany and zoology and

\textsuperscript{110} Ibid., 87, 125-126.

were also to determine the character of the other seven, more specialized, positions.\textsuperscript{112} Although the two senior professors were directed to build bridges between various disciplines under the rubric of general biology, the setup of the new department continued to privilege the older disciplinary structure. Melchers, who had been asked to recommend candidates for the first two positions, was outraged. He attacked the ongoing conservatism in West German biology as follows:

In the twenty-two universities in Germany there are old-fashioned botany and zoology professors. Why don’t we create something really new at one of the new universities? Why don’t we attempt to begin with genetics and biochemistry and then see if we really need botany and zoology after all? These are the specialized fields that one only needs if they are going to teach medical students and pharmacists! Why can’t we try something new just once at one place—Bochum? That has nothing to do with money. It is simply the lack of will to create something new.\textsuperscript{113}

Melchers attributed this lack of will to German pride in the Humboldtian ideal of universal education. For him, the German confidence in the uniqueness of their own cultural ideals prevented them from reforming their universities along more rational lines.\textsuperscript{114}

Of course, behind the ideal of Humboldt lurked the reality of lost prestige if university reform were carried out along these lines. While existing faculty protested that the creation of so many new positions represented the end of German universal education in favor of a fragmented, materialistic style foreign to German Kultur. Left unsaid was the fact that current institute leaders and professors stood to lose a great deal of their power and privilege within the university if they were forced to work with a number of

\textsuperscript{112} H. Autrum to Georg Melchers, June 6, 1962. Melchers Papers, Ordner 17.

\textsuperscript{113} Georg Melchers, “Diskussion,” in Max Delbrück, \textit{Über Vererbungskunde} (Köln: Westdeutscher Verlag, 1963) 38. Melchers elaborated on his suggestions for reforming and improving the West German universities in a lengthy report to the DFG, entitled “Abschrift an die Deutsche Forschungsgemeinschaft,” April 8, 1963. RAC, Record Group 1.2, Series 717, Box 6, Folder 68.

\textsuperscript{114} Ibid.
younger colleagues on an equal basis. For this reason young molecular biologists were often viewed skeptically by their older colleagues.\footnote{Interview with Carsten Bresch, Freiburg, June 9, 2000. Erwin Büning, “Alles was kreucht und blüht” Schwäbisches Tageblatt 24 September 1977. This article is a brief history of Tübingen University’s botany department by one of its emeritus members. Büning wrote that biologists were aware of the problems of the German universities long before the students were, but that their efforts for change encountered strong resistance from faculty members. Büning, who himself was a botany faculty member in the 1960s, said that the public rhetoric of preserving the unity of the sciences masked a hidden agenda of maintaining monopoly power on the part of the chairs of botany and zoology. I would like to thank Karl-Wolfgang Mundry for sending me this very important article.} This conservatism slowed the implementation of the changes suggested in the late 1950s.\footnote{Clausen, \textit{Stand und Rückstand}, 21-42; Wissenschaftsrat, \textit{Empfehlungen des Wissenschaftsrates zum Ausbau der Wissenschaftlichen Hochschulen bis 1970}, 125-126; Zarnitz, \textit{Molekulare und physikalische Biologie}, 19, 31.}

There were also many difficulties facing university research institutes that had nothing to do with the structural problems discussed above, difficulties that also hindered innovative biology research. In the 1950s, very few West German students studied biology, as there were relatively few employment opportunities in the biological sciences. The Max-Planck-Gesellschaft contributed to this shortage by attracting the best doctoral students to its own research institutes. Since the MPG could provide students with stipends, which the cash-poor universities often could not, in certain cases the MPG could literally buy the best students away. The students would do their research in a Max-Planck-Institut, and their work would count toward their degree at the nearest university.\footnote{In 1956, fewer than 2 per cent of West German university students chose biology as their major field. Meyl, \textit{Denkschrift zur Lage der Biologie}, 15, 19-25. On the paying of students by the MPG, see Büning, “Alles was kreucht und blüht.” Melchers actually reached an agreement with Büning whereby he refused to pay any of his own students so that he would not have an unfair advantage in attracting students. Interview with Karl-Wolfgang Mundry, Stuttgart, May 25, 2000.} General teaching requirements and administrative duties continued to consume a great deal of university professors’ time. These extra burdens made university positions less attractive, particularly to scientists who had trained in the MPG.\footnote{Butenandt, “Zur Leistungsstandes der deutschen Forschung,” 34-35. The Wissenschaftsrat recognized this as well, and in addition to its other suggestions, recommended that university professors have periodic free semesters, when they would have no teaching and minimal administrative obligations, allowing them}
6.3.3 The Great Experiment: Cologne

The desire for reform among leading intellectuals and the problems in implementing that reform are both illustrated by the first sustained effort to create a collegial, interdisciplinary biology department in a West German university setting: Cologne (Köln) University’s Institut für Genetik, completed in the early 1960s.\textsuperscript{119} Cologne already had one of the strongest botany programs in the Federal Republic; Melchers had very nearly left the Max-Planck-Gesellschaft to accept a position in Cologne after the war. Cologne also possessed an extraordinary figure in Joseph Straub, a botanist and deacon of the university’s natural sciences faculty in the late 1950s. Straub was a close colleague of Melchers and the two corresponded extensively, making Straub keenly aware of the promise of the new biology and the problems it faced in the universities.\textsuperscript{120} His first attempt to improve the situation came in 1955, when he persuaded the university to create a new chair for microbiology and then attempted to lure a talented molecular biologist to fill the post. His first choice, quite naturally, was Delbrück, who was offered the position in 1955. Delbrück refused the offer because of his very satisfactory personal and professional situation at Caltech.\textsuperscript{121} The other two to concentrate on their research. Empfehlungen des Wissenschaftsrates zum Ausbau der Wissenschaftlichen Hochschulen bis 1970, 124.

\textsuperscript{119} What follows is not meant to be a complete history of the Cologne institute, but instead an examination of the importance of scientific cooperation within it. For a brief history by one of the key participants, see Peter Starlinger, “Genetik in Köln,” in Forschung in der Bundesrepublik Deutschland: Beispiele, Kritik, Vorschläge, ed. Christoph Schneider (Weinheim: Verlag Chemie, 1983), 301-306. For secondary accounts of the institute, see Ernst Peter Fischer and Carol Lipson, Thinking About Science: Max Delbrück and the Origins of Molecular Biology (New York: W.W. Norton and Company, 1987), 264-271, and the article by Bruno Strasser in the upcoming special volume of Studies in the History and Philosophy of Biological and Biomedical Science, edited by Soraya de Chadarevian and Bruno Strasser.

\textsuperscript{120} Straub had worked in von Wettstein’s division of the KWI für Biologie for nine years, where he and Melchers began their very cordial professional relationship. Lebenslauf, Josef Straub, Melchers Papers, Ordner 14.

\textsuperscript{121} Kultusministerium des Landes Nordrhein-Westfalen to Max-Delbrück, June 28, 1955; Delbrück to Kultusministerium, July 6, 1955; also see Delbrück to Peter Rassow, Cologne University, July 23, 1955. All letters Delbrück Papers, Carton 26, Folder 22.
candidates, Wolfhard Weidel and Gunther Stent, also refused the position. Given the prestige of university chairs in the West German system and the extraordinary nature of this position (one of very few for microbiology in the Federal Republic), the refusal of all three candidates was somewhat of an embarrassment. Straub, however, was undeterred, and he now had an ally in Delbrück, who, despite his refusal, also desired to see modern biological research succeed in a West German university.

In the summer of 1956, Delbrück visited Cologne for several weeks. He led a phage genetics course much like those taught in the summertime at Cold Spring Harbor, New York, and he also lectured publicly on the need to bring team-based research to European universities. His talk made an impression; from that point, Straub found that officials from the university and the cultural ministry of the Land of Northrhein-Westphalia were willing to take his ideas seriously and provide him with the necessary support. Straub’s plans became much more ambitious than the creation of a single new teaching position. By late summer of 1956, he was proposing to create a research group in Cologne designed along the lines suggested by Delbrück. Straub wrote to Delbrück: “The execution of our plans would create an institute that deviates in many ways from the current style of university institutes of botany and zoology. It is quite possible that there will be resistance based on these grounds.”

Straub wanted Delbrück to lead this new institute, but again, Delbrück declined. He professed his great enthusiasm for the project, though, and offered to assist by serving as a guest director for two years. With Delbrück on board, Straub was able to secure a grant from the DFG for the construction of the new institute in 1957. The

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122 Joseph Straub to Max Delbrück, August 1, 1956. Delbrück Papers, Carton 21, Folder 5.

123 Delbrück to Straub, September 27, 1956.

124 Straub announced the new institute and outlined its potential to the rest of the natural sciences faculty in an open letter dated January 31, 1957. Delbrück Papers, Carton 21, Folder 5.
next step was to begin attracting scientific talent to staff the new institute, which proved easier to do once Delbrück was associated with it. In the late 1950s, Delbrück persuaded Carsten Bresch to join as an assistant professor under Straub. Soon Peter Starlinger was recruited from Tübingen as an assistant to Bresch. Later Thomas Trautner and Walter Harm joined them.125 Despite the contempt for biochemistry that he sometimes expressed publicly, Delbrück realized that a biochemist would be an important part of any successful molecular biology research team, and he set about recruiting two of the most talented biochemists in Butenandt’s Institute for Biochemistry, Hans Georg Zachau and Wolfram Zillig. With Butenandt’s consent, Zachau joined the institute, the goals and purpose of which Delbrück very eloquently expressed in one of his letters to Zillig:

What appeals to me is the possibility of forcing a substantial piece of Modern Biology into a legitimate University set-up. As you know, Academic Biology all over the world suffers from the dichotomy into Zoology and Botany, an unfortunate institutional relic of the nineteenth century. As a result, everything that is interesting in biology since [sic] about 60 years, like genetics, biochemistry, microbiology, etc., has to live by make-shift arrangements: in Medical Schools, in Agricultural Schools, in the Institut Pasteur, in Medical Research Council Units, and last but not least, in the Max Planck Institutes. I think it is necessary to reform the situation at the University. I don’t think that this can be done at once by a general reform until it has been demonstrated in one case, and the situation in Köln seems to me to have certain advantages for creating such a model situation.126

The great experiment began with the commencement of Delbrück’s two-year appointment as guest director in April, 1961. Official work at the institute, which had cost roughly DM 6 million, began in the fall of 1961, and the building was dedicated on June 22, 1962. Niels Bohr gave the opening lecture.127

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125 Interview with Carsten Bresch, Freiburg, June 9, 2000.


Under Delbrück’s leadership the institute flourished. It attracted scholars from the United States and Britain to West Germany, which was still unusual, and it held a yearly phage course modeled on the one offered in America at Cold Spring Harbor. When Delbrück left in 1963, though, the institute entered a period of personnel difficulties. Over the next several years, Harm, Bresch, and Zachau accepted positions elsewhere, leaving only Starlinger to carry on the work of the institute. After a very promising beginning, the success of the experiment was seriously in question.

The problem was that Delbrück had not been able to achieve everything he had hoped when the institute was founded. His overriding concern was that the institute should be collegial; he did not want one overall director, but instead five co-directors with a rotating manager’s position. The acting manager was to be the head administrator but was to have no authority over the other directors as far as research was concerned.128 The key to making such an egalitarian system work was to provide all of the directors with positions of roughly equal standing, and this Delbrück had been unable to achieve. In addition to his own guest position, he asked that four new assistant professorships (Extraordinariate) be created for the institute’s researchers, but the university’s faculty created only two. Delbrück then had to work out a compromise. The other two positions were set up as guest professorships with a minimum three-year duration. The cultural ministry of Northrhein-Westphalia agreed to consider the prospect of making these permanent at some point in the future. Since these two positions were not pensioned and did not confer civil servant status, Delbrück had to solicit outside funds to make them attractive to potential candidates.129


Naturally all four of the researchers recruited to the institute aspired to full professorship, and once Delbrück left, they feared they had lost their most influential advocate. Delbrück did not help matters, for he had not indicated whether he preferred that Bresch or Harm should be the first to be so promoted. Concerned about his own future, Harm accepted a position in the United States in 1964. Bresch accepted a position at Freiburg, where he was to head a modern department of genetics based on a smaller version of the Cologne model. He was able to secure a promise that three full professorships would eventually be created; during the years in which the institute was being built, he took a temporary position in the United States. In 1966, Zachau accepted a position at Munich's Ludwig-Maximilians-Universität, returning to the center of West German biochemistry research. Confronted with this exodus, Delbrück feared that there was more at stake than just the Cologne Institute. When he asked Starlinger to stay on in 1964, Delbrück wrote that if the Cologne Institute were to fail, it would be not just a local failure, but a German and European failure as well, for he saw the institute as the symbol of team based research in the European university environment. Starlinger was a bit more pragmatic. He chose to remain despite receiving the offer to head a division at the Max-Planck-Institut für molekulare Genetik in Berlin. He justified his decision to stay not along the grandiose lines of saving modern biology research, but

\footnote{Fischer and Lipson, Thinking About Science, 270.}

\footnote{Bresch to Delbrück, March 16, 1964. Delbrück Papers, Carton 4, Folder 20. Interview with Carsten Bresch, Freiburg, June 9, 2000. Once the faculty of the university recognized that they might lose several of their best researchers, they offered Bresch a full professorship in order to persuade him to stay, but he had already made his choice. In his words: "I got offers, I got an offer from Freiburg so here I am. I got several from America, and in fact after I had all these offers Cologne came to me and said 'Now you can be a full professor' and I said 'Damn it, now I don't want it anymore!'"}

\footnote{The threat of losing the last faculty member other than Starlinger led Delbrück to describe the situation as the "Zachau Crisis." The search for his replacement commenced in early 1967. Delbrück to Starlinger, April 13, 1966; Starlinger to Delbrück, February 24, 1967. Both letters Delbrück Papers, Carton 20, folder 17.}
because he enjoyed teaching and had spent the last several years of his life building up one institute in Cologne and had no desire to spend the next several years building another in Berlin.133 His decision was fortuitous, and he provided a vital element of continuity that allowed the Cologne’s Institut für Genetik to weather the crisis.

Delbrück also did his part to limit the damage. In 1964, he wrote to the deacon of the university to explain why Bresch and Harm were leaving. He placed much of the blame on the University faculty, who had been unwilling to guarantee the promotion of Bresch and Harm, and he wrote that the researchers had interpreted this as a vote of no confidence. Delbrück expressed great exasperation since his efforts to introduce a collegial, multidisciplinary institute were still being frustrated. Again, he placed the equality among directors as the most important goal he was trying to achieve. “This principle,” he wrote, “and not money, is the actual secret weapon of the American universities.”134 Given that Cologne was having trouble retaining faculty in its DM 6 million investment, Delbrück’s assessment seemed very accurate. He promised to return to Cologne in the summer to re-negotiate positions for the faculty members of the Institut für Genetik. At a meeting with the faculty on July 29, the positions were renamed and re-organized.135 After that point, the most difficult task was attracting researchers to a collegial institute in which there were currently no colleagues. Starlinger persevered, and over the years hired first-rate young scientists such as Walter Doerfler, Walter Vielmetter, and Benno Müller-Hill. The DFG continued to support the institute, first through Schwerpunktprogramme money, and later through the Sonderforschungsbereich


134 Delbrück to Herr Lauterjung, Dekan der naturwissenschaftlichen Fakultät, Universität zu Köln, May 19, 1964. Delbrück Papers, Carton 26, Folder 22.

135 Fischer and Lipson, Thinking About Science, 271.
program. After the late 1960s, assisted by a positive relationship with the nearby Max-Planck-Institut für Züchtungsforschung, Cologne University’s Institut für Genetik became the leading institute in molecular biology research in the Federal Republic of Germany.

6.3.4 Berufungspolitik

The Kaiser-Wilhelm-Gesellschaft had been founded in 1911 in order to complement the research of the universities. When the Kaiser-Wilhelm-Gesellschaft was refounded as the Max-Planck-Gesellschaft, its task, to serve as the vanguard of West German scientific research, was unchanged even though the society was now funded almost entirely by the public. The role of the postwar MPG, articulated by Butenandt in a 1961 speech, was to identify new, promising areas of research, to develop them, and eventually make them ready for introduction into the university research setting.\(^{136}\) In the area of molecular biology research, the MPG succeeded in the first two areas mentioned above—it did identify and develop promising new research before any other organizations in West Germany. The record of the MPG in transferring research to the universities was more mixed, however. The relative lack of talented German biologists encouraged the leaders of the MPG to behave very protectively toward their own researchers. MPG officials acted aggressively to create new opportunities for their best young scientists to prevent them from leaving for jobs in America. As new positions opened up in the West German universities in the 1950s and 1960s, the Max-Planck-Gesellschaft exercised this defensive behavior against them as well and attempted to prevent the best scientists from moving on to the universities.

Since few university researchers were experienced in the new biology, very little teaching of molecular biology took place in the West German universities before the

reforms of the 1960s. The best place to learn about advances in biology was in the Tübingen institutes of the Max-Planck-Gesellschaft, but relatively little teaching was done there. Of course there was instruction within the institutes, often in the form of seminars in which senior and junior researchers from different divisions all participated in discussions of current research and literature. Doctoral students often worked in MPG Institutes and had their work accepted by the nearest university, forging links between the two. In the case of Tübingen, the researchers in the MPG even presented lectures at the university on their work, providing Tübingen students with a window into advanced biology research available to very few young people in the Federal Republic. Still, the teaching load in the MPG was much lighter than in the universities.

While training the next generation of scientists was a major concern in the 1950s, the relative lack of students trained in the new biology presented few practical difficulties since there were even fewer jobs available for biologists in West Germany. In a memorandum on the subject written in 1957, Leo Szilard noted the paucity of career opportunities for biologists in the Federal Republic and suggested that students interested in biology should begin their careers in fields such as physics or chemistry. They could then spend a couple of years exploring biology by drawing upon their physical science training. When they were finished with their studies, most could then go on to industry

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137 Interview with Alfred Gierer, Tübingen, May 23, 2000.

138 In the early 1960s, Melchers wrote that no one could dispute the fact that the universities produced far more doctoral students than the MPG; he said that his friend Erwin Bünning, a professor in the botany department of Tübingen University, probably taught 10-20 times the number of doctoral students, but that the quality of the training the students received in the MPG was far superior. Melchers urged that critics of the MPG take this qualitative superiority in account in order to balance the quantitative inferiority of the MPG in preparing the next generation of West German scientists. Melchers to Dr. Schneider and Straub, both Cologne University, November 17, 1964. Melchers Papers, Kasten 2. During the search for a successor to Fritz Kaudewitz, Freksa discussed the relative unimportance of teaching within the Max-Planck-Gesellschaft as follows, “It is really so: I knew that Kaudewitz would be best at a university; didactically he is so good he really belongs there, because his interests also lie in this direction. In this sense he is really not a true Max-Planck-Man. He is interested not only in scientific problems, but also in becoming a professor and so forth. There are problems that interest him that should not interest a Max-Planck-Mann.” “Protokoll…am 18.4.1963,” p. 8.
thanks to their original training, while only a rare few who showed exceptional aptitude should pursue biology as a career. Even these talented individuals could not count on a career in the West German universities.\textsuperscript{139} With so few jobs in available in both industry and academia, there was nothing to attract young people to careers in molecular biology, and consequently the lack of production of young molecular biologists was not a practical problem in the 1950s.

Despite this practical consideration, national ideology cut in, and brain drain in the direction of America was perceived as a threat by the West German scientific community. To them, this intellectual migration indicated that something was wrong with the Federal Republic, and added to the strength of America's already overwhelming research enterprise. The Max-Planck-Gesellschaft combated this phenomenon, as I have suggested, on a case by case basis by making generous counter-offers to promising young scientists who had received job offers from abroad. For example, this was done to keep Werner Reichardt in the 1960s, and later to keep F. Alfred Anderer.\textsuperscript{140} Butenandt called this policy "Berufungspolitik," literally the "politics of appointment." He was speaking in terms of the Bavarian government's deliberate policy of offering attractive positions to selected candidates in order to attract first rate scientific talent (including himself) to its universities.\textsuperscript{141} In the case of Bavaria, Berufungspolitik was a positive measure intended to create a strong research community, but it could also be used defensively, as it was against the United States, to prevent the loss of talent from a previously existing research group. In the late 1950s, when the universities first began to create new positions for

\textsuperscript{139} Leo Szilard, "Welche Methoden eignen sich zu Förderung der Biologischen Forschung in Deutschland?" December 16, 1957. Copy in Melchers Papers, Ordaer 89.

\textsuperscript{140} See section 5.2.3; Interview with F. Alfred Anderer, Tubingen, July 28, 2000.

\textsuperscript{141} Adolf Butenandt, "Bayern und die Max-Planck-Gesellschaft," 516.
biologists, members of the Tübingen Max-Planck-Institutes also engaged in an ad hoc Berufungspolitik aimed at keeping their best young scientists out of the universities.

Berufungspolitik was not an official policy of the Max-Planck-Gesellschaft, but rather a strategy used by directors to negotiate from a position of strength on behalf of their junior colleagues.\textsuperscript{142} Such behavior could be problematic and sometimes led to strained relationships between the MPG and the universities. The most dramatic example was the conflict surrounding Wolfhard Weidel’s offer to take the new chair for microbiology at Cologne University in 1955. Delbrück had been the first choice for the position, but Weidel was the second. Delbrück enthusiastically supported Weidel’s candidacy, and Joseph Straub had requested letters of recommendation regarding Weidel in November, 1954. Naturally, he wrote to Melchers, who at the time was frustrated by his own inability to secure a position for Weidel commensurate with his extraordinary talents within the MPG.\textsuperscript{143}

In 1955, an opportunity opened in the Max-Planck-Gesellschaft when Max Hartmann, one of the other directors of the Max-Planck-Institut für Biologie, decided to retire. Melchers hoped that Weidel would become Hartmann’s replacement and began to gather support for Weidel’s promotion. In the meantime, Delbrück had rejected the offer from Cologne, causing the university to fall back to its second choice. Weidel received a formal offer from the university in the fall of 1955, which greatly complicated Melchers’ goal of keeping him in the MPG. Melchers confided to his friend Hans Gaffron that the

\textsuperscript{142} Since appointments in the MPG were so prestigious, its leadership screened potential candidates rigorously. The policy was that for scientists to be considered, they had to demonstrate exceptional scientific qualifications. One of the ways of determining this was if a prospective member had a claim to a university chair, which seems to indicate that the tendency to make counter offers to encourage candidates not to leave the MPG for the university was not unusual. Hans Dölle, “Satzungsprobleme,” no date given, p. 5. Melchers Papers, Ordner 7.

\textsuperscript{143} Melchers to Straub, November 11, 1954; Melchers to Straub, November 24, 1954. Both letters Melchers Papers, Ordner 7.
offer was politically dangerous. The Cultural Minister of the Land of Northrhine-Westphalia had been involved in the creation of the chair for microbiology and was interested in seeing it filled. A rejection by Weidel, a young West German coming from a junior position, would have been taken as an insult, particularly in the wake of Delbrück’s rejection. The situation was further complicated by the fact that the Cultural Minister also had an influential position in the Ländergemeinschaft, which helped determine MPG funding. Melchers feared economic retribution if he encouraged Weidel to reject the offer—but that is exactly what he did.\textsuperscript{144} He immediately began gathering support to keep Weidel in the MPG, and in early October wrote to President Otto Hahn to nominate Weidel for an MPG directorship.\textsuperscript{145} He added that the offer from Cologne required the MPG to act with haste in the matter.

In the late autumn of 1955, it was unclear whether or not Melchers would succeed, as influential persons in the Max-Planck-Gesellschaft took different positions. On the one hand, Feodor Lynen commended Melchers for trying to keep such an excellent young scientist in the MPG. He wrote that filling scientific positions (he was referring specifically to positions in the field of physiological chemistry) in West Germany with good people was very challenging, and cited several examples to prove his point. It was difficult, he wrote, to create an environment in the Federal Republic that would attract the best researchers, most of whom (like Weidel) had spent extended periods in the United States.\textsuperscript{146} Butenandt, on the other hand, believed that it would be good for West German microbiology if Weidel went to Cologne, and he cautioned Melchers that it would be a blow to the prestige of the universities if Weidel were to

\textsuperscript{144} Melchers to Hans Gaffron, September 27, 1955. Melchers Papers, Ordner 8.

\textsuperscript{145} Melchers to F. Gummert, September 27, 1955; Melchers to Otto Hahn, October 5, 1955. Both letters Melchers Papers, Ordner 8.

\textsuperscript{146} Feodor Lynen to Melchers, October 25, 1955. Melchers Papers, Ordner 8.
reject the offer. Melchers did not take Butenandt’s objections seriously, arguing that because of personal antipathy between Butenandt and Weidel that Butenandt simply wanted the MPG to be rid of his former pupil.147 When the leadership of the MPG met in early March of the following year to decide the issue, resistance focused around Butenandt. Nevertheless, after a struggle, Weidel received the position.148

Straub, Delbrück, and the leaders of Cologne University were angry with Melchers and the Max-Planck-Gesellschaft, although Straub had already suspected that Melchers would try to keep Weidel in the MPG.149 Friedrich Oehlkers, then at Freiburg University, wrote to Melchers, Hartmann, and Kühn on behalf of Straub. Oehlkers argued that the universities were in a poor position relative to the Max-Planck-Gesellschaft, and that it was difficult to find people with the courage to create new positions like microbiology in the generally conservative university environment. Now that such a position had actually been created, Melchers was trying to ensure that it went unfilled. Citing the excellence of the other researchers in the Tübingen Max-Planck-Institutes, he concluded that it was foolish to keep a person with Weidel’s skills out of the university system.150 The rector of Cologne University contacted President Hahn to ask him not to keep Weidel from the university; Straub and several others from the university even offered to drive to Tübingen in January, 1956, in order to discuss the matter with Melchers and Weidel in person. Their offer was refused, and the new position for

147 Melchers to Feodor Lynen, October 10, 1955. Melchers Papers, Ordner 8. Melchers detailed the reasons for the dislike between Butenandt and Weidel in his letter of November 11, 1954 to Joseph Straub, cited above. Weidel’s friend Gunther Stent has also discussed the antipathy between the two men. See discussion of Weidel in Chapter 4, section 4.2.4.


149 Regarding the appointment, Straub wrote “Ich glaube aber, das Melchers will mir die Suppe salzen.” Straub to Delbrück, November 11, 1955. Delbrück Papers, Carton 21, Folder 5.

150 Oehlkers to Melchers, Kühn, and Hartmann, November 15, 1955. Melchers Papers, Ordner 8.
microbiology at Cologne went unfilled until the creation of the Institut für Genetik five years later.\footnote{Melchers to Straub, December 16, 1955; Straub to Melchers and Weidel, January 3, 1956; Melchers to Straub, January 5, 1956. All letters Melchers Papers, Ordner 9.} Delbrück's behavior throughout the entire episode is noteworthy. Although he badly wanted Weidel to take the Cologne position, when he received a request from the MPG to recommend Weidel for their position, he praised Weidel without reservation, saying that Weidel had few peers in the entire scientific world.\footnote{Delbrück to Klaus Rajewsky, November 4, 1955. Delbrück Papers, Carton 25, Folder 14.}

The Max-Planck-Gesellschaft's desire to retain the best young scientists led to further frustration for Delbrück in his capacity as guest leader of Cologne’s Institut für Genetik. Delbrück had the highest respect for Alfred Gierer, especially after he had completed a research stay with Delbrück in Pasadena. When Delbrück began to fill positions for the new institute, he quickly contacted Gierer. Gierer expressed interest, but was uncertain as to whether he should remain in Tübingen or join the Cologne institute. In either case, he asked Delbrück for a concrete offer, noting that such an offer would aid Freksa in negotiating with the MPG’s leadership on Gierer’s behalf. Shortly after receiving the offer from Cologne, Gierer informed Delbrück that the MPG had decided to evaluate him as a candidate for a directorship in the Institut für Virusforschung. Were he to receive an offer, he wrote, his choice was to remain in Tübingen.\footnote{Gierer to Delbrück, December 9, 1959; Gierer to Delbrück, February 4, 1960. Both letters Delbrück Papers, Carton 9, Folder 6.}

Delbrück was disappointed and angry. His disappointment was undoubtedly magnified by his personal dislike for the Max-Planck-Gesellschaft—he believed that the society sucked in the best scientific talent and because of its light teaching load did little to contribute to West German science on the whole.\footnote{Interview with Alfred Gierer, Tübingen, May 23, 2000. Delbrück also expressed his dislike for the MPG to other of his colleagues. For example, he wrote to John Kendrew that he had decided to work with the Cologne Institute because the Max-Planck-Institutes "seemed to make no substantial contribution at all}
persuade Weidel and Gierer to leave the MPG undoubtedly fed this personal prejudice, but his response to Gierer’s rejection provides a clear illustration of the tensions between the MPG and the universities caused by competition for scarce scientific talent:

The ugly alternatives I mentioned above are the following: either the MPG offers you a satisfactory arrangement, and you accept it, then I will feel cheated, because obviously they would be doing so only in the face of the threat of losing you to a University, and that in contrast to everybody’s professed happiness over the fact that I am exerting myself to force an entry for modern biology into a university set-up.

Or, if I point this out to the MPG, either at the Freksa level or at the higher level, they, as a result, will not make you a competitive offer, and then you will be mad at me.

Or, Freksa and Co. propose you for membership, but at the higher level, even without me saying anything, they become bashful about their public relations with the Universities, and do not accede to the request of Freksa and Co. Then everybody will be mad at everybody.\(^\text{155}\)

While the tensions discussed by Delbrück were real, Gierer rightly pointed out that the alternatives for him were not as simple as Delbrück believed. Gierer had personal as well as professional reasons for remaining in Tübingen, and he was concerned about the direction the Cologne Institute would take once Delbrück left (a very valid and prescient concern, as it would turn out).\(^\text{156}\)

Of all the senior Max-Planck directors involved in molecular biology research, it was Butenandt who was most helpful in Delbrück’s Cologne endeavor. Delbrück recognized that he would need an excellent biochemist for the institute. Butenandt’s Institut für Biochemie was clearly the leader in West Germany. There, Delbrück pursued both Hans Georg Zachau and Wolfram Zillig. Of the two, Zachau demonstrated the

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greater interest. Zachau visited Cologne in 1960 and was intrigued. He had a number of doubts, however, particularly regarding his employment status, since he would not have an ordinary professorship. Delbrück was able to answer these points to Zachau’s satisfaction.157 Delbrück, in turn, was concerned about Butenandt’s opinion, but after a visit in late summer, 1960, Butenandt was very positive about the prospect of his student’s going to Cologne. Of Butenandt, Zachau wrote: “...he is allowing me, exactly as he is Zillig, a fully free hand; he only feels that it is important to gain a further clarification of the specifics of the Köln position.”158 In fact, Butenandt helped arrange matters such that Zachau would have time to finish his Habilitationschrift by arranging to keep his position in Munich open even after his formal appointment in Cologne began.159

The previous examples demonstrate that in the competition for talent the universities did not always end up losing. Starlinger rejected an offer from the MPG in favor of staying in Cologne, and Kaudewitz even left the MPG for a university post. When Bresch left Cologne, he did so for another university position, not an MPG directorship. While in the late 1950s and early 1960s, the possibility of excessive intellectual inbreeding and defensive Berufungspolitik threatened to limit the influence of scientists who had trained as part of the TMV research group, by the late 1960s these investigators had spread out from the Max-Planck-Gesellschaft and enriched the entire West German scientific community with their talent, experience, and international connections.

Ultimately, the most important factor in distributing scientific talent throughout biology institutes in the Federal Republic was the decision making of individual


scientists. None of the people mentioned above was forced against his will to accept a position. Instead, each chose the position that most closely matched his desired career goals. The Berufungspolitik of Max-Planck-Gesellschaft directors did not create a wall around the society, but it did create attractive positions for the relatively small number of West German molecular biologists, encouraging them to remain in the society and slowing the spread of researchers (and research) from the MPG to the universities. In this sense, Berufungspolitik contradicted the society’s mission to be the vanguard of West German scientific research.

Conclusion

In the 1950s and 1960s, scientific research in the Federal Republic developed along lines much different from those initially explored during the occupation. The autonomy of organizations such as the Deutsche Forschungsgemeinschaft was compromised as they participated more closely with the federal and state governments in an increasingly centralized and directed scientific policy. The leaders of the Max-Planck-Gesellschaft were able to safeguard their own autonomy, although they, too, moved their organization toward a more directed scientific policy. The budget of the MPG grew dramatically during the 1960s, especially after the 1964 agreement that established joint funding for the MPG through both the federal and state governments. As the society’s budget increased, so did the number of institutes, and molecular biology was one of the fields of research that profited from this prosperity. By the early 1970s, molecular biology had spread throughout many different institutes.

Despite a steady growth in funding, molecular biology research took many years to become firmly rooted in West German universities, owing largely to structural rigidities that hampered the establishment of new departments within existing faculties. The tendency of the Max-Planck-Gesellschaft to retain its best scientists rather than encouraging them to take up university careers contributed to this delay. Nevertheless, it
would be incorrect to characterize the relationship between the MPG and the universities as negative or antagonistic regarding molecular biology. Tensions certainly existed, but members of both organizations profited from one another as well. Due to their funding and resources, Max-Planck-Institutes were often able to attract excellent students from the universities, and by the early 1960s, an academic career in the Federal Republic became a viable alternative for young scientists trained in the MPG. On the other hand, the MPG produced nearly all of the quality molecular biology research in the Federal Republic as well as most of the young German molecular biologists. In spite of Berufungspolitik, people like Peter Starlinger and Fritz Kaudewitz left the MPG to enrich the universities. The best way to characterize the relationship between the two organizations is to say that it was usually tense, often positive, and sometimes negative.
Epilogue

In recent decades, the significance of Tübingen as a center of molecular biology research has declined relative to cities like Cologne, Heidelberg, Munich, and Berlin. To a large extent, this decline has resulted from steady outflow of talent from the small university city, for as the Max-Planck-Gesellschaft and the universities both began to emphasize molecular biology research more strongly, they drew upon the personnel of the Tübingen research community to staff their new institutes. Like a family tree with its roots in prewar Dahlem, the TMV investigators branched out across the West German research landscape. The dispersal had already begun with Butenandt’s move to Munich. Wolfram Zillig and Gerhard Braunitzer, both of whom had made important contributions to the TMV research, accompanied him. In the mid 1960s, Schuster and the Wittmanns all left for Berlin’s Max-Planck-Institut für molekulare Genetik. Peter Starlinger went to Cologne’s new institute, where Walter Vielmetter later joined him. Other university departments also benefited at Tübingen’s expense. For example, in the late 1960s, the Technical University of Stuttgart created a modern biology department. The first three professors of the department, including Karl-Wolfgang Mundry, were all from the various Tübingen Max-Planck-Institutes.¹

On July 27, 1964, Wolfhard Weidel died at the relatively young age of 48.² In February, 1969, Gerhard Schramm died suddenly and unexpectedly after a short illness.³ He was 59. Schramm, as I have suggested, had been one of the most important researchers in the Tübingen group from the late thirties until his death and had made a number of significant contributions to the early history of molecular biology in Germany. However, outside of his circle of colleagues, he has not been remembered with the level of respect that his achievements seem to have merited, that of a Nobel Prize caliber researcher.⁴

Without access to the Nobel records, the issue of the prize cannot be decided. As far as Schramm’s overall reputation is concerned, however, in 1969 Butenandt hinted that Schramm had not received the recognition he deserved because of prejudice against Germans by the rest of the scientific community. Butenandt cited a passage from James Watson’s recently published *The Double Helix* as evidence.⁵ Regarding Schramm’s experiments on the re-aggregation of TMV, Watson had written:

Virtually no one outside of Germany, however, believed that Schramm’s story was right. It was inconceivable to most people that the German beasts would have permitted the extensive experiments underlying his claims to be routinely

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⁴ The impression that one takes away from Ute Deichmann’s *Biologists under Hitler*, trans. Thomas Dunlap (Cambridge: Harvard University Press, 1996), is that Schramm was not an exceptional scientist. See 313-314, 317, as well as Benno Müller-Hill’s Foreword, xvi.
⁵ At the time of his death, Schramm had been involved in arranging a scientific conference in Tübingen to commemorate the 100th anniversary of Friedrich Miescher’s isolation of DNA there in 1869. When the conference convened after Schramm’s death, Butenandt spoke about Schramm’s achievements, then spoke very negatively about Watson’s characterization of Schramm in *The Double Helix*. Adolf Butenandt, “In Memorium Gerhard Schramm: Tübingen, Miescher Symposium, 14. September 1969.” Butenandt’s talk was published in edited form as the Foreward to Gerhard Schramm, *Bauplane des Lebens* (Munich: R. Piper & Co., Verlag, 1971), 7-14. This book is a posthumous collection of Schramm’s philosophical essays, collected and edited by his former student F. Alfred Anderer.
carried out during the last years of a war they were so badly losing... Wasting time to disprove Schramm was not to most biochemists’ liking.\textsuperscript{6} Apparently Butenandt was so appalled by the reference to the “German beasts” that he did not read further, for Watson continued: “As I read Bernal’s paper, however, I suddenly became very enthusiastic about Schramm, for, if he had misinterpreted his data, by accident he had hit upon the right answer.”\textsuperscript{7} \textit{The Double Helix}, therefore, does not depict a postwar world of blind hatred toward the Germans, but rather one in which justifiable skepticism toward German work could be overcome depending upon the scientific merit of the research itself. After all, Watson was one of the first Anglo-American scientists to take the Tübingen virus researchers seriously, and, as we have seen, both he and Francis Crick had a very positive working relationship with Schramm and his colleagues in the 1950s and 1960s. Furthermore, in my research, I have encountered only one clear case in which Schramm’s association with National Socialism was held against him as a scientist.\textsuperscript{8} In contrast, I found numerous cases in which he was accorded the highest respect for his intelligence and charisma. Butenandt’s interpretation of events does not seem plausible to me.

It is more likely that Schramm’s reputation suffered for scientific rather than historical reasons. In the late 1950s, Schramm became interested in the origins of nucleic acids, and he spent much of the 1960s trying to understand how they might have arisen spontaneously from simpler compounds as part of the early evolution of life on earth. The pre-biotic synthesis of nucleic acids became the focus of his many fruitful scientific and philosophical inquiries in the 1960s, but it was also the subject of what was undoubtedly


\textsuperscript{7} Ibid.

\textsuperscript{8} Max Perutz told me that he refused to allow Schramm to become a member of the European Molecular Biology Organization (EMBO) because of his SS membership. Telephone Interview with Max Perutz, August 8, 2000.
the most unfortunate episode in Schramm’s scientific career. In 1962, Schramm’s name appeared as senior author on a paper based largely on the experimental work of one of his students, Wolfgang Pollmann.\textsuperscript{9} The paper suggested that short sequences of nucleic acids could be synthesized without a template through a polymerization reaction utilizing single nucleotide bases. This conclusion, however, did not stand. Pollmann had decided to present Schramm with only the data that seemed to confirm their speculations, and he had omitted evidence to the contrary. In essence, Pollmann noted the disappearance of single nucleotide bases in the reaction and inferred that it resulted automatically in the formation of polymers—an assumption others found to be unwarranted. The longer chains did not form because Pollmann had not done the time consuming work necessary to protect reactive side-groups on the single nucleotides to allow them to polymerize in the proper manner.\textsuperscript{10}

Pollmann had chosen a uniquely inappropriate time to present Schramm with these one-sided results. In 1961, the work of Nirenberg and Matthaei (see section 5.1.4) had interested many labs in pursuing the Genetic Code, and Schramm and Pollmann’s work offered a promising technique. According to their paper, it should have been possible to synthesize short DNA molecules of determined sequence easily and then use them as probes to determine correlations with individual amino acids.\textsuperscript{11} Investigators


\textsuperscript{10} Interview with Heinz Schaller, Heidelberg, July 21, 2000; Interview with Thomas Hohn, Basel, July 19, 2000.

\textsuperscript{11} In the early 1960s, the synthesis of segments of nucleic acids with a determined sequence was extremely time-consuming work; for example, it took Heinz Schaller a year to synthesize the sixteen possible di-nucleotide sequences. Schaller was at that time working in Har Gobind Khorana’s lab at the University of Wisconsin. Khorana shared the 1968 Nobel Prize for his exacting work along these lines. For an overview, see Khorana’s Nobel lecture, “Nucleic Acid Synthesis in the Study of the Genetic Code” in the Nobel Foundation, ed., \textit{Nobel Lectures in Molecular Biology 1933-1975} (New York: Elsevier Press, 1977), 303-331.
who attempted this procedure were, of course, unsuccessful. Schramm was able to clarify the matter with his colleagues and distance himself from the claims made in the paper, and those who had known him for years continued to respect him. However, for many investigators, this paper had been their only exposure to Schramm's scientific research, and they were less likely to be forgiving. Since Schramm was the senior scientist listed on the paper, responsibility for its content ultimately fell on his shoulders, even though he had not knowingly taken part in any deception. Coming relatively late in his career, this incident tarnished the otherwise excellent reputation he had built up through his numerous achievements. Thus, as Francis Crick recalls, Schramm was a brilliant scientist, but you had to double check his research before basing your own work on it, just to be certain.

Schramm's death was the beginning of the end of non-medical virus research in the Tübingen institutes. Freksa's death in 1973 was another turning point marking a shift in research emphases. After the losses of both Schramm and Freksa, the scientists who remained in Tübingen became interested in very different lines of research. After working on the amino acid sequence of TMV, F. Alfred Anderer had become interested in immunology and immunochemistry and pursued these lines of research after becoming a director in the Institut für Virusforschung in the late 1960s. Melchers had long since

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12 Schramm's papers at the Archive of the MPG contain a draft of a talk given at the 1963 Gordon Conference on Nucleic Acids which shows that he was trying to retreat from the claims in the paper without actually admitting that his co-worker had reported the data incorrectly. Regarding the Pollmann paper, Schramm said: "Since several of these compounds cannot easily be synthesized by chemical means, this new method attracted some attention, but I also heard that other laboratories had some difficulties to reproduce the results. Many people asked me for further details. In our opinion a 'cooking recipe' is not sufficient to avoid all sources of error, but it is necessary to find out the exact kinetics of this reaction." Gerhard Schramm Papers, MPG Archiv, III Abt., Rep. 62, Nr. 62/0062. Also, Interview with Heinz Schaller, Heidelberg, July 21, 2000.

13 Telephone Interview with Francis Crick, November 9, 2000.

ceased his own research on TMV in favor of studies of tissue cultures, regeneration of plants from protoplasts, and hybridization of plants by fusion of protoplasts.\textsuperscript{15} He retired in 1976, but continued active research until the mid-1980s, after which he served as a director of the Agrocentric Corporation, based in Tokyo, until 1991. He died in November, 1997, at the age of ninety one. In 1996, the leadership of the Max-Planck-Gesellschaft decided to close the Max-Planck-Institut für Biologie after more than eighty years of existence. The last two directors were scheduled to become emeritus in 2004.\textsuperscript{16} 

Alfred Gierer also remained in Tübingen. In the 1960s, as many of his colleagues left and others shifted the focus of their work, he found himself increasingly interested in problems of pattern formation in early biological development.\textsuperscript{17} He began to work very closely with Werner Reichardt from the neighboring MPI für biologische Kybernetik, and under Gierer’s leadership, research in the Institut für Viruskorschung became oriented toward developmental biology.\textsuperscript{18} In 1984, the name of the institute was changed to the Max-Planck-Institut für Entwicklungsbiologie (Max-Planck-Institute for Developmental Biology), and the institute made the transition to this new area of research successfully. In its short lifetime, the Institut für Entwicklungsbiologie has the distinction of having already produced a Nobel Laureate, Christiane Nüsslein-Volhard, whose research on developmental genetics in \textit{Drosophila} embryos was done there in the 1980s.\textsuperscript{19}


\textsuperscript{16} "Schlag für Max-Plancker: Präsident will traditionsreiches MPI für Biologie schließen," \textit{Schwäbisches Tagblatt} 18 October 1996; "Der Letzte macht das Licht aus," \textit{Schwäbisches Tagblatt} 23 October, 1996.

\textsuperscript{17} Gierer’s personal recollections of his growing interest in pattern formation in biological systems are discussed in Alfred Gierer, "Hydra the model—model for what?" Evening Lecture, 8\textsuperscript{th} International Workshop on Hydroid Development, September 1999—Tutzing/Germany. I would like to thank Dr. Gierer for providing me with a copy of this talk.

\textsuperscript{18} Interview with Alfred Gierer, Tübingen, May 23, 2000.

\textsuperscript{19} The importance of Tübingen and the influence of scientists such as Alfred Gierer on Nüsslein-Volhard’s research are discussed in her autobiography in the Nobel E-Museum website at www.nobel.se/medicine/laurerates/1995.
Alfred Gierer's own career followed an interesting path from the simple to the complex. His original training was in physics, specifically in the movement of protons across hydrogen bonds. From this foundation, he moved into the study of large biomolecules such as nucleic acids and proteins. Later, his interests shifted to a higher level of complexity, the way that cells, each containing vast numbers of complex molecules, themselves reproduce and form the coherent patterns necessary to allow multicellular life to function. In recent decades, he added another layer of complexity to his field of study, and has turned his attention to the history and philosophy of science itself. He has written several well-reviewed books on these subjects.  

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Conclusion

This study has shown that excellent molecular biology research using tobacco mosaic virus as a model system began in Berlin during the Third Reich and continued in Tübingen until the 1960s, although the overall German contribution to the field did not match those made by the US, Great Britain, or France. The devastation of the war, the dismissal of many of the best scientists due to Nazi policies, and the temporary isolation of the German scientific community after the war, were all responsible for this more limited achievement. Furthermore, West German universities were often insufficiently funded and possessed of disciplinary rigidities, both of which slowed the diffusion of the new biology throughout the Federal Republic. Competition for scarce resources between the universities and the Max-Planck-Gesellschaft, particularly competition for talented scientists, lent an added complication to the picture. The relationship between the two organizations was sometimes very positive, but it could be contested as well, resulting in the poor utilization of what human and material resources were available. In spite of these complicating factors, West German researchers were able to make significant contributions to the solution of fundamental problems in biology. This is testimony to the scientific excellence of the research community established in 1937 as well as to the willingness of many West German institutions to support innovative research when possible.
1. *Science in Twentieth Century Germany*

One of the empirical findings of this study is that excellent biological research did flourish during the National Socialist period. This is not to say that Nazi science policy was responsible for the TMV program—it was not. The TMV program was driven by the research ambitions of three scientists, Adolf Butenandt, Alfred Kühn, and Fritz von Wettstein, who had in turn been motivated by the work of the American Wendell Stanley. TMV research was unrelated to questions of human health and heredity and was never used to justify the racial hierarchy through which convinced National Socialists viewed the world. With that said, Nazi officials did not hinder the TMV investigators and provided more than adequate financial support. True, the researchers had to portray their research program as one of applied agriculture to a government desirous of useful results, but this has long been a normal part of scientific funding. The point is that Nazi science policy did not discourage quality biological research. On the contrary, if the scientists were flexible enough to present their research astutely, they could expect generous support. The history of the TMV community therefore complements Robert Proctor’s recent analysis of cancer research in Nazi Germany.¹ Proctor showed that Nazi policy was, in some cases, directly responsible for the promotion of excellent research, while my own research indicates that excellent research took place independent of all but the most general oversight by the regime. These findings confirm the conclusions of many other historians of Nazi science, who have already shown that a complete range of research activities, from excellent science to pseudoscience, found support among the many patrons available in Nazi Germany’s polycratic structure.²

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² This is the theme of Mitchell G. Ash’s review essay, “Science, Technology, and Higher Education under Nazism,” *Isis* 86 (1995): 458-462. The fact that support for basic biological research increased during the
1.1 Saints or Sinners

This study has also contributed to our understanding of the three issues for further research suggested by scholars of German science in Chapter 1. The first of these was to provide an understanding of the behavior of scientists that did not depend upon a simplification of behavior into the dichotomy of evil Nazis and heroic resisters. The behavior of the scientists examined in this study, particularly Butenandt and Melchers, defies classification into either "saints" or "sinners." Of the two, Butenandt has been subject to greater criticism because of his disingenuousness regarding his relationship with the Nazi government. Nevertheless, the decisions that the two men made regarding their individual relationships with Nazism were similar because both sets of decisions were based upon the individual needs of the scientists, not universal moral concerns.

Recent scholarship on National Socialist Germany has revealed that even given the limitations on personal freedom imposed by the regime, individuals had more alternatives available to them than they later pretended. Their choices were not limited to a stark, "Nazism or resistance" dichotomy, but instead they had a range of options leading to various levels of accommodation with the regime. Individuals made choices, even unpleasant ones, not simply because they were good or evil, but because they were attempting to satisfy their own needs and desires. As the cases of Butenandt and Melchers demonstrate, the ways in which people went about fulfilling their needs was based on their own particular political and cultural socialization.3

Butenandt's decision to join the party in 1936 was just that—a decision, not an obligation, and was made by weighing advantages and disadvantages. The fact that Alfred Kühn, who was not a party member, was appointed as Director of a Kaiser-

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Wilhelm-Institut the following year makes it clear that Nazi Party membership was not an explicit prerequisite for the position. Butenandt must have believed that the advantages of the Dahlem position outweighed the disadvantages of joining an organization with which he did not personally agree. Professionally, he was not as well established as Kühn, which gave him a greater motivation for reconciling himself with the regime. Politically, he was not sympathetic to the Nazis, but he was relatively conservative and nationalistic, giving his political views overlap with the moderate elements of the Nazi Party. He had no overriding reason to oppose the Party in early 1936. His activities were those of an individual who was conciliatory, not defiant, someone who, when confronted, preferred to say what was expected of him in place of "no."\(^4\)

As we have seen, Butenandt was also very much an elitist. He held the profession of science in high esteem and believed that scientists such as he should occupy positions of privilege in society. Like many of his colleagues, he believed that German society was obliged to support science, but he did not believe that scientists had specific reciprocal obligations. He was therefore accustomed, by both temperament and socialization, to focus on his own situation to the exclusion of all others. His attention naturally extended to the members of his immediate scientific group, whom he rigorously supported. To those within the group, he was therefore an outstanding and supportive mentor; to those outside the group, he appeared arrogant.\(^5\) Focusing on the needs of his immediate circle

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\(^4\) The education department of the canton of Basel solicited several references regarding Butenandt’s character before offering him a position in 1948. Dr. Hans Weber, of Tübingen University, replied favorably, but said that Butenandt was by nature conciliatory, always wanted to put forth a friendly face, and had a very difficult time saying no. Letter, Weber to Educational Department, Canton Basel, July 8, 1948. A copy is in the Melchers Papers, Ordner 1.

\(^5\) The insider/outsider perspective is clearly on display in the two primary scholarly works on Butenandt: Peter Karlson, Butenandt’s student and friend, wrote a largely celebratory biography, while historian Robert Proctor, who examined the consequences of Butenandt’s actions from a broader perspective, has been much more critical. Peter Karlson, *Adolf Butenandt: Biochemiker, Hormonforscher, Wissenschaftspolitiker* (Stuttgart: Wissenschaftliche Verlagsgesellschaft mbH, 1990); Robert N. Proctor,
of colleagues allowed him to pursue his own scientific interests in the midst of a dictatorship, while ignoring the moral consequences of Party membership.

Melchers, in contrast, by his own admission was shielded by his adviser Fritz von Wettstein. Melchers was therefore not faced with the same choices as was Butenandt. Nevertheless, Melchers was less conservative and less arrogant, and his association with the Sozialdemokratische Partei Deutschlands (Social Democratic Party of Germany—SPD) suggests that he had a greater sense of social responsibility. It is therefore difficult to imagine his ever joining the party despite any potential advantages that membership might have incurred. He did, however, rely on the authority of Albert Speer to convince officials in Tübingen that his own research was relevant to the armaments industry in an attempt to persuade them to build him an experimental greenhouse.

Of course, his work was not essential to the war effort, nor does this letter compromise his integrity. It does illustrate that even he saw the need to play along with the National Socialist system in order to preserve his own scientific freedom. Unlike Butenandt, however, Melchers never attempted to portray his scientific work as resistance to Nazism. He was the first to admit that the behavior of the TMV researchers

"Adolf Butenandt (1903-1995): Nobelpreisträger, Nationalsozialist, und MPG-Präsident. Ein erster Blick in den Nachlass," Forschungsprogramm Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus Ergebnisse 2 (2000). Witnesses noted both the supportive and arrogant aspects of Butenandt’s personality. In his 1949 diary, Rockefeller Foundation representative Gerald R. Pomera wrote: “RF [Rockefeller Foundation] officers have no direct evidence that B [utenandt] engaged in reprehensible practices although they have heard of instances where he displayed typical Prussian arrogance. B certainly made a good impression on GRP [Gerald R. Pomera] this year and was about the only German scientist who asked that an NS [Natural Sciences] officer talk primarily with his youngsters since these were the men who ought to be helped.” Diary of Gerald R. Pomera, September 15, 1949. RAC, Record Group 12.2, Diaries, Box 36.


7 Letter, Georg Melchers an Seine Magnifizenz den Herrn Rektor der Universität Tübingen, September 10, 1943. Tübingen Universitätsarchiv, Sig. 117/13634.

was less than admirable. “We were no heroes of the resistance,” he told Benno Müller-Hill in a candid interview, “but we muddled through.”

Melchers, stubborn as a mule, was never reluctant to play the part of critic, to “salt the soup,” as the German phrase goes. This was especially true in the postwar era, when he was one of the few who attempted to deal responsibly with the worst Nazi collaborators in the German scientific community. His efforts to exclude such persons from the institutes of the Max-Planck-Gesellschaft shows that critical self-examination of the kind the MPG is currently undergoing was possible in the postwar era, but that it would have required both courage and humility. Melchers’ behavior justifiably earned him a reputation for honesty and integrity. This reputation reinforced his critical nature and consequently he often associated his own position in a given conflict with the morally correct one. That is to say, he usually thought he was right and of course, he often (but not always) was.

While Melchers remained ever the critic, Butenandt remained accommodating and political. In the postwar years, he again chose the path of least resistance. He would not accept that his behavior was subject to question, and he chose to portray himself and his colleagues as resisters of Nazism in order to give his postwar leadership moral authority. Naturally, he denied his Party membership. His behavior in the postwar era has since led many to question the credibility of science and of scientists, and in this sense his unwillingness to confront his own past has been more problematic than his actual wartime behavior.

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Yet, in spite of these differences there was much in common in the behavior of both men after the war. Both lobbied tirelessly to improve the conditions of their own labs and organizations and were dedicated to the advancement of their junior colleagues. They justified their own autonomy, funding, and privilege in the name of science, and their pursuit of the greater good of science via their own self-interest led them both to self-contradictory behavior. Butenandt continually spoke of the need for complete freedom in scientific inquiry, while supporting a rigidly hierarchical, authoritarian structure within science itself. Melchers advocated the improvement of the West German universities, yet worked to persuade one of his best pupils, Weidel, not to join a university department. In both cases, their behavior, as different as it was in specifics, led to great success for themselves, their colleagues, and for West German biology research more generally.

This demonstrates that in science, as in other competitive fields of human activity, greed and self-interest could be necessary and even creative forces. Science was driven by the successes of individual researchers and laboratories. In this sense, whatever was good for individual scientists was good for science as a whole.\textsuperscript{12} For someone as successful as Butenandt, for example, there was no hypocrisy in conflating his own achievements with those of German science, even if this was done to excuse his cooperation with National Socialism. However, it does not follow that what is good for science is then automatically good for society, as Butenandt assumed. The pattern of behavior demonstrated by Butenandt and countless other uncommitted National Socialists—negotiating between different job offers, seeking patronage, and ultimately pursuing personal gain—was exactly the same both before and after the war. In both eras

it contributed to their professional advancement and by extension to the vitality of their professions. In the Third Reich, however, their success in turn contributed to the vitality of the National Socialist state. The Dahlem TMV investigators did not cause Nazism, but they sometimes played the role of enablers.

Therefore, the problem with scientists in the Third Reich is not that they were corrupted by Nazism, but instead that they proceeded as normally as they could and worked with the National Socialist government. The decision making of Butenandt and his colleagues, even those who never joined any Nazi groups, was normal, characterized by logic and self interest. However, in Nazi Germany the proper decisions to make were not normal but courageous; not logical, fitting smoothly with the social order, but illogical and defiant; and they were not made for individual gain, but for the good of all humankind. In short, they were not easy or obvious decisions, but hard and complicated ones. They were challenging enough that only a rare few possessed of great moral strength actually made them. It seems as though Butenandt’s activities in the Third Reich have become so controversial precisely because they were the actions of a normal human being, and given prestige of science we often, correctly or incorrectly, tend to expect a higher standard of conduct from its practitioners.

1.2 Chronology

This study’s most significant contribution to modern German history is in its chronology. The story of TMV research in Germany demonstrates that it is possible to examine twentieth century German history outside the chronology imposed by the years 1933 and 1945. For the TMV-workers, the important years were 1937, when they first began to follow up Wendell Stanley’s research; 1953, when Watson and Crick published their model of the structure of DNA; 1956, when Gierer and Schramm’s work brought them international recognition; and 1966, when the last parts of the Genetic Code were completed. There was no Stunde Null for German virus research in 1945. 1945 was
important, but perhaps not so important as 1943, when the researchers began their evacuation to Tübingen, or even 1941, when they became an autonomous group within the Kaiser-Wilhelm-Gesellschaft. The continuities in behavior discussed above accompanied continuities in personnel, equipment, experimental object, and choice of questions. When major changes did take place, such as the shift from a belief in a nucleoprotein version of the gene to a nucleic acid version of the gene, they came about as shifts in scientific thought, supported by the academic community and verified empirically. Of course the end of the war brought with it such a profound change in German political, social, and cultural life that it cannot be ignored as a key date in the social factors constraining science, but its importance should not be inflated to the extent that it obscures important underlying continuities.

1.3 Institutions

In the European universities of the postwar era, it was very difficult to create the kind of institutional environment in which molecular biology could flourish. For many years, universities lacked either the resources or the motivation to reform their biology departments to the extent necessary to allow researchers from different backgrounds to work together as a team. In West Germany, this was especially true, and consequently molecular biology research in the early postwar years was largely confined to the Max-Planck-Gesellschaft. Tensions arose between the MPG and the universities as they competed for money and scientific talent, the latter of which tended to be concentrated in the MPG. As MPG President, Butenandt justified the inequality by claiming a pathbreaking role for the Max-Planck-Gesellschaft in regard to innovative research. The assumption was that the excellence of the MPG would automatically improve the situation of science in the entire country. In biology, such was not necessarily the case in the 1950s, when research and researchers moved slowly between the Max-Planck-Gesellschaft and the universities. The impact of this split on the growth of molecular
biology research in West Germany is difficult to gauge. The competition between the
two organizations may well have favored the MPG and deprived the universities of
money and researchers, but the MPG created an environment in which the money and
researchers that were available could work more productively in an interdisciplinary
setting than was common in a university department or institute. However one attempts
to resolve this issue, the importance of institutional structure to the success of molecular
biology research is inescapable.

A broader issue is the extent to which the institutes of the Max-Planck-
Gesellschaft served as progressive forces in the West German economy. In the case of
the biological sciences, the record was also mixed. Max-Planck-Institutes were
innovative in their structure relative to the West German universities, but members of
both organizations consistently distanced themselves from practical concerns. Public
funding and revulsion against Nazi centralization allowed the MPG and universities to
reject, at least rhetorically, applied research in favor of a mission of pure, unfettered
intellectual activity. Collaboration between molecular biology labs and the medical and
pharmaceutical industries was not as strong in West Germany as elsewhere, and apart
from a few scientists, the MPG steered clear of involvement with biotechnology
companies. Only in the past decade has this trend been reversed.\textsuperscript{13}

The shortcomings I have mentioned were summarized by plant virologist Karl
Silberschmidt in the late 1950s. Silberschmidt was a Jew who had been forced to leave
Germany in 1935; he lost several family members in the Holocaust. After the war, he
settled in Brazil, from where he corresponded regularly with Melchers. At Melchers’
urging, Silberschmidt was awarded the Richard Merton Guest Professorship, which was
funded by the Deutsche Forschungsgemeinschaft.\textsuperscript{14} In 1958, Silberschmidt used this

\textsuperscript{13} Interview with Heinz Schaller, Heidelberg, July 21, 2000; Interview with Axel Ullrich, April 17, 2000.

\textsuperscript{14} There is an extensive correspondence regarding Silberschmidt’s visit in the Melchers Papers, Ordner 11.
opportunity to spend several months as a guest professor in Melchers' division in the Max-Planck-Institut für Biologie. Upon the conclusion of Silberschmidt’s visit, the MPG published his observations.15

Bringing a fresh, non-American perspective to the institutes, Silberschmidt noted three characteristics of the Tübingen research community that he found troubling. First, he said the investigators he met focused too closely on one specific object (in Melchers’ institute that object was not necessarily TMV), and that this narrowed their vision. He believed that they were ignoring other potentially exciting experimental systems in favor of devotion to their “Lieblingsobjekt.”16 He also criticized the disdain of Max-Planck scientists for applied research. Silberschmidt wrote that the barrier between pure and applied research, which seemed to be very real for some individuals within the MPG, was fuzzy and indistinct. He argued that there was a definite feedback between the two research styles, and that the pursuit of one often led to advances in the other. Silberschmidt found that this intellectual elitism was true of the West German universities as well, where it also limited the scientific vision of researchers. Consequently, he left Tübingen convinced that West German students had very little opportunity to learn about the most interesting new problems in his own area of expertise, plant pathology, in comparison with their colleagues abroad.17 Finally, he suggested that West Germany needed a closer relationship between the Max-Planck-Gesellschaft and the universities, for it seemed to him that the lack of teaching in the MPG concentrated innovative research in isolated pockets. He also noted, as had many others, that having a


16 Ibid., 352.

17 Ibid., 353-354.
single senior professor running a research institute, as was often the case in the universities, impeded the incorporation of new ideas and approaches.

Overall, the desire for scientific freedom and political autonomy that characterized scientists and scientific institutions in West Germany tended to discourage the kind of integrated research networks that emerged in the United States. It is not so much that any one West German institution—be it the Max-Planck-Gesellschaft, the Deutsche Forschungsgemeinschaft, or the universities—was critically flawed. All could and did support excellent biological research. Instead, interactions between various elements of the research infrastructure were limited. In West German biology the whole equaled, at best, the sum of the parts, while elsewhere webs of professional relationships linked funding agencies, public and private universities, independent research labs, and industrial research labs in an extremely productive, self-reinforcing manner.\(^\text{18}\) West German researchers, in contrast, often defended their hard-won autonomy by strictly demarcating their own intellectual territory and policing the boundaries. Consequently, innovative biology research took place in enclaves linked bilaterally to the United States, instead of in a mutually reinforcing network of relationships.

2. *The History of Molecular Biology*

2.1 *The German Contribution*

A second empirical finding of this study is that German researchers made contributions to the history of molecular biology that have not been noted fully in the

historical literature. Their achievements were acknowledged by their colleagues in the 1950s and 1960s—Gierer, Schramm, Mundry, and the Wittmanns all received professional recognition for their research—but only recently, as subjects like TMV have become part of the history of molecular biology, have these people and their colleagues drawn the attention of historians. In this sense, my findings are in keeping with the trend in the history of molecular biology noted in Chapter 1—its increasing complexity.

My purpose for stressing the importance of the German researchers is not to overturn previous findings, but to add another element in order to help provide a clearer picture of the emergence of molecular biology in the 1940s, 1950s, and 1960s. The German experience adds depth and nuance to the existing literature rather than overturning it.

2.2 International Collaboration

From the inspiration of Wendell Stanley’s research to Max Delbrück’s efforts to create an American-style research group in a West German university setting, the story of molecular biology in West Germany has been one of international collaboration. International isolation, though never complete, certainly compromised the quality of the German TMV research during the war years and delayed its growth and development in the postwar era. Once international contact was resumed, foreign travel became one of the most essential facets of molecular biology research in the Federal Republic, more important, perhaps, than in most other countries since a research stay in the United States for purposes of professional networking and English language practice was necessary. In contrast, students from the United States and England could socialize themselves completely into the profession without ever speaking a word of German.

In fact, America's scientific excellence became a minor threat to the West German scientific enterprise. True, molecular biology was international, but its center of gravity was located in the United States. America became a magnet for German scientists and engineers of all backgrounds and was especially attractive to molecular biologists. Within the Federal Republic, the positions that afforded researchers the greatest opportunity to match efforts with their colleagues abroad, at least in the 1950s, were in the Max-Planck-Gesellschaft. Thus the United States drew some scientists out of West Germany, and within the country, the American influence encouraged the others to work in the MPG where interdisciplinarity was more common. The fact that lower administrative and teaching duties would not interfere with their research efforts strengthened the appeal of the MPG. Overall, the United States and the MPG both drew talent away from the universities where (despite institutional limitations) it would have been available to a much greater number of West German citizens. The science as an investigative enterprise benefited, but this was at the cost of institutionalizing the new biology as part of the curriculum of the West German universities. In an ideal scenario, both research and teaching would have benefited German society and repaid the investment of the federal and state governments. Walter Doerfler, currently of Cologne's Institut für Genetik, described the ongoing attraction of the United States as follows: "It really doesn't matter where the good science is done, but we would not mind some of the better Germans staying here."20

2.2 A Scientific Revolution?

In his classic work, The Path to the Double Helix, Robert Olby argued that the shift from a nucleoprotein version of the gene to a nucleic acid version could justifiably

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20 Interview with Walter Doerfler, Cologne, June 23, 2000.
be interpreted as a scientific revolution along the lines developed by Thomas Kuhn.21 Others have disagreed; in particular, Horace Freeland Judson has suggested that "The rise of molecular biology asks for a different model" than the overthrow of one monolithic worldview in favor of another. He continues, "Biology has proceeded not by great set-piece battles but by multiple small-scale encounters—guerilla actions—across the landscape. In biology, no large scale, closely interlocking, fully worked out, ruling set of ideas has ever been overthrown."22 The history of the German biology community tends to support Judson's interpretation. Certainly many of the German researchers were deeply committed to a nucleoprotein version of the gene, so its replacement required a significant re-orientation of their thinking, as Olby argues. However, the ideas and suggestions that caused them to change their thinking arose not as anomalies within their own particular paradigm, but came from other researchers using different approaches and different model systems. Avery used pneumococci bacteria; Hershey and Chase used phage; Watson and Crick combined the insights of phage genetics with the biochemistry of Chargaff and the physical approaches of x-ray crystallography. All of these approaches suggested to the Germans that they should look toward nucleic acids. When they did, they were able to provide concrete evidence that validated these earlier suggestions. Not all of these researchers were committed to a particular paradigm to the extent that the Germans were, so not all of them experienced it as a revolution.

The biology community was incredibly diverse, as Judson suggests. The importance of discipline crossing suggests that there was no one highly developed research paradigm into which a significant portion of the biology community fit. Kuhn's


model of scientific revolutions, based as it is on case studies from physics and astronomy, may not be appropriate for the biological sciences. Biologist Ernst Mayr believes that it would be more appropriate to borrow an image from biology itself in order to understand the way that the biological sciences advance—that of natural selection. At any given time, there are many competing theories in the scientific world. Those that endure and become generally accepted are the most robust, the ones that incorporate empirical evidence in a coherent framework and are persuasive in the context of the prevailing beliefs of the scientific community. Mayr therefore concludes that there are major and minor revolutions in biology, that different paradigms can co-exist, and that biological science is never unsettled—the constant change means that there are no periods of “normal science.”

Mayr’s interpretation of the history of the biological sciences is very promising, provided one prefaces it with an understanding of how the mechanism of natural selection works. Natural selection does not mean the “survival of the fittest.” Such an interpretation of the history of any branch of science would imply that the only criterion for success in a scientific theory is its closeness to the “truth.” This is a relatively simple view that leaves the human element out of the scientific process. Natural selection as understood by biologists does incorporate such an element of “competitive success,” but it is always in the context of a very specific environment, and always tempered with the importance of accident and contingency.

Applying this understanding to the history of science means that scientific theories stand or fall based upon how they are received by specific communities with their own biases and prejudices. Thus, the very real possibility exists that what a

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24 Ibid., 101.
community accepts as a valid theory at a given time may prove to be untenable to future investigators. However, since scientific communities change while the physical environment they study remains relatively constant, when a theory fits closely with the natural world, it tends to persist. While not perfect, scientific communities are adept at recognizing and correcting poor theories, making the accuracy with which theories map the world an important selection criterion, but of course not the only one. Scientific theories are subject to many selection criteria, some of which are empirical and logical, some of which are social and cultural. A given theory must be able to satisfy some of these criteria to survive. The most successful theories satisfy the greatest number of criteria over the longest time.

3. *The Nature of Science*

This case study suggests that the complexity of science is best understood as being embodied in diverse groups of investigators. Science, as practiced in Dahlem and Tübingen, was an untidy human activity that nevertheless produced accurate knowledge regarding the physical character of biological inheritance. The TMV community grew, prospered, and branched apart, creating an influential genealogy of biological researchers and institutes in West Germany. Within this genealogy, there was a strong, shared intellectual inheritance that can be traced back to Dahlem. Butenandt, Melchers, Freksa, and Schramm influenced one another in the early years of the TMV research, and later went on to be very significant in shaping the thought and professional development of their proteges after the war. As the younger researchers matured, "left the nest," so to speak, and became scientific leaders in the Federal Republic, they helped shaped a biological community that owed much to the interdisciplinary Arbeitsstätte für Virusforschung founded in 1937, yet at the same time differed from it in ways that the founding scientists could scarcely have envisioned.
Given the interplay of logic, continuity, and circumstance in twentieth century German science, an examination of the TMV investigators from either a strictly realist perspective or a strong social constructivist perspective would have been insufficient. A constructivist may rightly point out that the German scientists were always constrained by their particular culture, whether that was the isolated world of the Third Reich or the conservative world of the postwar universities. Clearly non-scientific factors affected the quality of the knowledge they produced, both for better and for worse. Nevertheless, adherents of the realist position can note that logic, rigor, and reproducible, empirical results were also important parts of the process. One reason the infectious RNA work was so persuasive was because it had been produced by two independent labs using different techniques. On the opposite extreme, Schramm’s reputation for scientific excellence suffered after his name was associated with work that was neither verifiable nor reproducible.

Despite this complexity, science does have defining characteristics. Because scientists represent an independently existing natural world in human terms, theirs is a high-resolution activity. There is an external standard to be met, or at least approximated. In addition, the daily, human process of science requires that its practitioners possess education, money, and cognitive and social skills, all of which limit access to the profession. The selectivity that characterizes socialization into the profession of science tends to instill a feeling of shared achievement among scientists, one that cannot be shared with non-scientists. Scientific communities thus become subcultures that in some ways are isolated from the larger society around them, yet also share the biases and assumptions of that society.

The fact that many of the scientists in this study identified themselves as members of such an elite subculture, a subculture with its own special rights and privileges, will undoubtedly trouble some readers, who may interpret this story as evidence of the
authoritarian, undemocratic nature of modern science. Such is not my intention. Science has been selective and undemocratic, but these characteristics have made it no different from the other scholarly disciplines, or even from the performing arts.\textsuperscript{25} If the TMV researchers were elitist, it was because in Imperial and Weimar Germany they were socialized to believe that their status carried special privileges. They were therefore no different from lawyers, teachers, and higher civil servants, for in general, the educated middle class of early twentieth century Germany was elitist and self-centered. In this regard, there was nothing about the behavior of biological researchers that was intrinsic to the profession of science.\textsuperscript{26} The history of TMV research in Germany suggests that the flaws noted by critics of science are not flaws of science itself, but rather flaws that science shares with the culture that has produced it.

Interestingly, the elitism of the German scientific community was always accompanied by a lack of political and economic authority. The pure/applied science distinction that I have repeatedly addressed was, after all, a negotiating tool used to redress the basic inequality between scientists and political and economic leaders, and was not uniquely German. The distinction between pure and applied science had emerged in the nineteenth century, and the idea of pure or objective research had been a legitimizing force in scientific research for much longer. According to historian Robert Proctor:

\textsuperscript{25} David Hull wrote, “The fact that only a tiny percentage of those who study a musical instrument or take ballet lessons ever get to perform in public is as damning of a society as the fact that only a very few scientists contribute measurably to science.” Hull, \textit{Science as a Process}, 360.

\textsuperscript{26} Konrad H. Jarausch, \textit{The Unfree Professions: German Lawyers, Teachers, and Engineers, 1900-1950} (New York: Oxford University Press, 1990). David White argues that the elitist, undemocratic attitudes of the educated upper-middle classes in Weimar Germany resonated with the authoritarian elements of National Socialism. Consequently, many of these people were willing to contribute to the both the governance and ideology of National Socialism. David White, “Upper-Middle-Class Complicity in the National Socialist Phenomenon in Germany (Ph. D. Diss, University of Edinburgh, 2001).
Value-freedom is an ideology of science under siege—a defensive reaction to threats to the autonomy of science from political tyrants, religious zealots, secular moralists, government bureaucrats, methodological imperialists, or industrial pragmatists asking that science be servile or righteous or politically correct or practical or profitable.²⁷

This points to another continuity between the two major political periods spanned by the existence of the TMV research community, which is the idea of science under threat. In both eras, researchers defended themselves by attempting to control the ways in which the knowledge they produced about the natural world would be interpreted by the broader human community. They shifted their emphasis from applied research in the Nazi era to pure research in the postwar era in order to appear useful to the respective governments, illustrating their lack of independence in both eras. This lack of independence is the most important characteristic of science to emerge from this study. German science was at the intersection of the human and natural worlds and drew upon both for the production of knowledge. The scientists themselves, however, were firmly embedded in the human world, and constantly had to barter the knowledge they produced for funding and prestige. This interaction has made the history of German science one of the most fascinating human stories of the twentieth century.

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I have used the following three abbreviations in the footnotes:

*Stanley Papers:* Wendell Stanley Papers, Bancroft Library, University of California, Berkeley, Collection 78/18.


*Melchers Papers:* Georg Melchers Nachlass, Archiv zur Geschichte der Max-Planck-Gesellschaft, Abt. III (ZA 28), Rep. 75. The Melchers correspondence is massive (more than 80 binders/Ordner) and is not catalogued. The binders are divided roughly chronologically, and within a given binder, correspondence is arranged alphabetically. In my citations, I have indicated the binder number and the last name of the person under whom a given document is filed. When a binder has a specific name (for example, number 72 consists of correspondence relating to the DFG) I have included that as well.
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