

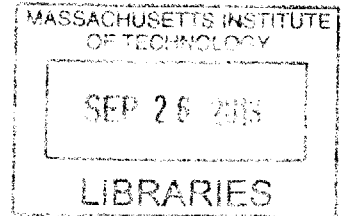
Conditional Inequalities: American Pure and Applied Mathematics, 1940-1975

by

Alma Steingart

Bachelor of Arts
Columbia University, 2006

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Submitted to the Program in Science, Technology, and Society
in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy in History, Anthropology, and Science, Technology and Society
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Abstract

This study investigates the status of mathematical knowledge in mid-century America. It is motivated by questions such as: when did mathematical theories become applicable to a wide range of fields from medicine to the social science? How did this change occur? I ask after the implications of this transformation for the development of mathematics as an academic discipline and how it affected what it meant *to be* a mathematician. How did mathematicians understand the relation between abstractions and generalizations on the one hand and their manifestation in concrete problems on the other?

Mathematics in Cold War America was caught between the sciences and the humanities. This dissertation tracks the ways this tension between the two shaped the development of professional identities, pedagogical regimes, and the epistemological commitments of the American mathematical community in the postwar period. Focusing on the constructed division between pure and applied mathematics, it therefore investigates the relationship of scientific ideas to academic and governmental institutions, showing how the two are mutually inclusive. Examining the disciplinary formation of postwar mathematics, I show how ideas about what mathematics *is* and what it *should be* crystallized in institutional contexts, and how in turn these institutions reshaped those ideas. Tuning in to the ways different groups of mathematicians strove to make sense of the transformations in their fields and the way they struggled to implement their ideological convictions into specific research agendas and training programs sheds light on the co-construction of mathematics, the discipline, and mathematics as a body of knowledge.

The relation between pure and applied mathematics and between mathematics and the rest of the sciences were disciplinary concerns as much as they were philosophical musings. As the reconfiguration of the mathematical field during the second half of the twentieth century shows, the dynamic relation between the natural and the human sciences reveals as much about institutions, practices, and nations as it does about epistemological commitments.

Thesis Supervisor: David Kaiser

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*To Ruti,
The first Dr. Steingart in the family*

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been there from day one and every day had been enriched by her presence. My debt to her is too large to pay and I can only offer her my love.

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In days of doubt, I turned to him. יהי זכרו ברוך

ABBREVIATIONS

AAAP	Abraham Adrian Albert Papers. Special Collection Research Center, The University of Chicago, Chicago, IL.
ACM	Association for Computing Machinery
AMS	American Mathematical Society
AMSR	American Mathematical Society, <i>Records 1888</i> -. Call Numbers Ms. 75.2, 75.5., 75.6, 75.7, 75.8 John Hay Library, Brown University Archives, Providence RI.
COSRIMS	Committee of Support of Research to the Mathematical Sciences
JWTP	John W. Tukey Papers, 1937-2000. Call Number Mss. Ms. Coll 117. American Philosophical Society Library, Philadelphia, PA.
MAA	Mathematical Association of America
MMP	Papers of Harold Marston Morse, 1892-1977. Call Number HUGFP 106.10. Pusey Library, Harvard University Archives, Cambridge, MA.
ONR	Office of Naval Research
RAMP	Records of the Applied Mathematics Panel, 1942-1946, Records of Panels and Committees, Records of the Office of Scientific Research and Development (OSRD). Call Number 227.5.4. The National Archives at College Park, Maryland.
RCP	Richard Courant Papers, 1902-1972. Call Number MC 150, Elmer Holmes Bobst Library, New York University Archives, New York. While I was visiting the archive the collection was being rearranged. Box x (new), refer to material that had already been processed according to the new organization. Box Y (old) refers to the older system.
SMP	Saunders MacLane Paper, 1969-1979. Call Number 86-10. Dolph Briscoe Center for American History, The University of Texas at Austin, Austin, TX.
WPC	War Preparedness Committee

Introduction.

The Logic of Mathematics



At left: Professor R. Courant, Professor Emeritus of the Courant Institute at New York University, and special consultant to IBM. At right: Dr. H. H. Goldstine, Director of IBM's Mathematical Sciences Department.

What does this chat on mathematics have to do with biologists, your heartbeat, moon shots and a sizzling steak?

YOU ARE looking in on a work session at IBM's Thomas J. Watson Research Center. The two mathematicians you see here are discussing a system of non-linear differential equations.

It is a most abstruse subject, but out of their work may come dozens of practical applications. For instance, it may result in a new programming idea for a computer. This, in turn, may help some

biologist probe deeper into life processes, or enable a cardiac specialist to analyze the heart's electrical activity with new precision. Or speed an astronaut to the moon. Or, simply, show a rancher how to raise better beef.

Mathematical thought has a world of destinations at IBM. Its applications are almost as numerous as humanity's needs—and dreams.

IBM

A Chat on Mathematics

Richard Courant and Herman Goldstine are sitting in front of a table. Both men are dressed identically, white dress shirts with narrow black ties and large rimmed glasses. Goldstine is hunched over the desk. His hands are crossed under the table as he is listening intently to Courant. There are some loose papers spread over the table in front of them, and Courant is

resting his right arm, pen in hand, on a stack of them. Behind them are three shelves lined with books. Otherwise, the room they are sitting in is quite empty. A doorknob is discernable on the right side of the image. The photographer seems to have opened the door to the office, interrupting the two in the midst of conversation. The headline under the photograph draws the viewer's attention. In black bold letters, it poses a question, "what does this chat on mathematics have to do with biologists, your heartbeat, moon shots and a sizzling steak?" Indeed, the contrast between the image and the text is drastic. There are no lab coats, medical equipment, or laboratory apparatuses in the photograph nor, for that matter, a grill. What one sees is an office, and a pretty bare one at that. The text under the image begins to answer the question. It explains to the viewer, "the two mathematicians you see here are discussing a system of non-linear differential equations. It is a most abstruse subject, but out of their work may come dozens of practical applications." Here were the two faces of mathematics – the abstruse and the applied. Using non-linear differential equations to program computers, the text under the image further explains, could help a "biologist probe deeper into life processes," enable "a cardiac specialist to analyze the heart's electrical activity," shoot "an astronaut to the moon," or instruct "a rancher how to raise better beef."

This IBM advertisement, which appeared in a special issue of *Scientific American* on "Mathematics in the Modern World" in 1964, celebrated the growing applicability of mathematical theories and techniques. It represented one of the fundamental features of the development of mathematics in the postwar period: namely, its increased appropriation into ever-larger domains of knowledge. IBM copywriters put it somewhat more fancifully when they concluded, "mathematical thought has a world of destinations at IBM. Its applications are almost as numerous as humanity's needs – and dreams." Yet the ad also presents a more traditional

image of mathematics – a world in which all a mathematician requires is an office, some paper, a pen, and possibly a few books. A world populated by equations, linear or otherwise. A world defined by abstract ideas, not things. That this tension existed in the ad is no surprise. It was perhaps the defining characteristic of the growth of mathematics in the aftermath of World War II.

What was the status of mathematical knowledge in mid-century America? When did mathematical theories become applicable to such a wide range of fields, from medicine to raising cattle? And how did this change occur? What were the implications of this transformation on the development of mathematics as an academic discipline? How did it affect what it meant to be a mathematician? Or the nature of mathematical work? Did mathematicians really require, as the image above suggests, only some paper and a pen? Or did they increasingly rely on experimental apparatus? How did mathematicians understand the relation between abstractions and generalizations, on the one hand, and their manifestation in concrete problems on the other? And, how did mathematical ideas travel from university offices into labs and industries such as IBM?

In Cold War America mathematics was caught between the sciences and the humanities. In 1963, the National Science Foundation annual report declared mathematics to be “the basic language of science, a feature common to all the disciplines of the physical sciences, and increasingly to the biological and social sciences.”¹ Yet, at the same time, many mathematicians pronounced mathematics to be a form of art. “Creative work in this field,” mathematician Adrian Albert asserted in 1960, “has the same kind of reward for the creator as does the composition of

¹ National Science Foundation 13th Annual Report, 1963. (Washington, DC: U.S. Government Printing Office, 1963), 12.

a symphony, the writing of a fine novel, or the making of a great work of art.”² That mathematics was simultaneously celebrated as both science and art was not unique to the postwar period. It has been, after all, a subject of much philosophical and mathematical discussion. However, in the aftermath of World War II, this dual definition of the field was no longer just a matter of intellectual musing. Rather, it came to define the field’s institutional and intellectual remaking.

This dissertation tracks how this tension between the sciences and the humanities shaped the development of professional identities, pedagogical regimes, and the epistemological commitments of the American mathematical community in the postwar period. It therefore investigates the relationship of scientific ideas to academic and governmental institutions, showing how the two are mutually inclusive. Examining the disciplinary formation of postwar mathematics, I show how ideas about what mathematics *is* and what it *should be* crystallized in institutional contexts, and how in turn these institutions reshaped those ideas. The re-articulation of the boundaries of mathematics and the ontological shaping of the field in the period took place within a specific social and political context. Tuning into the ways different groups of mathematicians strove to make sense of the transformations in their fields and the way they struggled to implement their ideological convictions into specific research agendas and training programs sheds light on the co-construction of mathematics, the discipline, and mathematics, as a body of knowledge.

² Albert, who was the Dean of the Physical Sciences at the University of Chicago, made the following claim during a speech he gave at the University of South Florida, Tampa in November 1964. However, this sentiment was in no way unique to Albert and was voiced by many contemporary mathematicians. Marston Morse of the Institute of Advanced Studies similarly claimed that “mathematics is an art, and as an art chooses beauty and freedom” and Mina Rees noted that “like poetry,” mathematics requires “persons who are creative and have a sense of the beautiful for its surest progress.” The view of mathematics as art, and as the decades proceeded, especially as abstract art, was quite common and was made by mathematicians as well as various commentators. See: Adrian Albert, “The Curriculum in Mathematics,” 11 November 1964, AAAP, Box 2; Marston Morse, “Mathematics, The Arts and Freedom,” MMP (106.10), Box 2, Folder “Bul. Of Atomic Science;” Mina Rees, “The Nature of Mathematics,” *The Mathematics Teacher* 55, no. 6 (1962): 434–440.

I contend that mathematics provides an especially persuasive case from which to examine the situated nature of knowledge production. Mathematics is at once both historically stable and incredibly malleable. Unlike biology, which studies living organisms, or chemistry, which studies the substances and properties of matter, mathematics is not a discipline centered around a subject, but rather on a methodology.³ This unique property of mathematics implies that mathematical theories are not bound by the contexts in which they were developed. Differential equations, as the IBM ad reminds us, were applied in the postwar period to a wide range of fields from biology to economic endeavors. Moreover, their emergence can be traced all the way to Leibniz at the end of the seventeenth century.⁴ Yet the context of application matters. For example, the study of differential equations from the perspective of theoretical or “pure” mathematics revolved around proofs of the existence and uniqueness of solutions, whereas their study in relation to ballistics focused on developing rigorous methods of approximation. What counted as “legitimate” mathematical reasoning and justification was deeply bound by the fields to which mathematics was applied. Mathematics, being at once both fixed and adaptable, therefore, offers a compelling demonstration of how ideas, communities, and institutions are formed in relation to one another.

³ The method of logical deduction is often presented as situated at the core of the mathematical enterprise, its roots are traced all the way to ancient Greece. In the 1960s, as will be described in greater detail in Chapter Four, the act of symbolic reasoning came to define mathematical activity. See: Reviel Netz, *The Shaping of Deduction in Greek Mathematics: A Study in Cognitive History* (New York, NY: Cambridge University Press, 2003).

⁴ Tracing contemporary mathematical concepts and theories to their “origin” in previous centuries is a common practice in the history of mathematics. In their most historical form such claims will identify Leibniz as the inventor of the modern theory of differential equations disregarding the numerous developments that have taken place since Leibniz’s day and ignoring any reference to specific circumstances. Though even a more skeptical historian can acknowledge some historically contingent development that traces contemporary differential equations and Leibniz’s work as a successive iteration of specific events.

Conditions

The growth of mathematics between the sciences and the humanities was mapped onto both the disciplinary formation of the field and its intellectual making. Like the rest of the sciences, mathematical research garnered the increasing support of the federal government.⁵ From a small scholarly community centered around a few elite universities, mathematics grew in scale and scope. From computing to fluid dynamics and statistics, mathematical research proved useful to military operations.⁶ Starting in 1946 with the Office of Naval Research, academic research

⁵ The literature on the effect of federal funding on scientific research during the Cold War is vast and covers a diverse set of fields, from physics to biology, oceanography, and the social sciences. For classic works and some recent studies, see: Paul Forman, "Behind Quantum Electronics: National Security as Basis For Physical Research in The United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* 18, no. 1 (1987): 149–229; Dan J. Kevles, "Cold War and Hot Physics: Reflections on Science, Security and the American State" (1988); Peter Galison and Bruce William Hevly, *Big Science: The Growth of Large Scale Research* (Stanford, CA: Stanford University Press, 1992); Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York, NY: Columbia University Press, 1993); Naomi Oreskes, "A Context of Motivation US Navy Oceanographic Research and the Discovery of Sea-Floor Hydrothermal Vents," *Social Studies of Science* 33, no. 5 (2003): 697–742; David C. Engerman, *Know Your Enemy: The Rise and Fall of America's Soviet Experts* (Oxford University Press, 2009); Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold War America* (Rutgers University Press, 2013). So far there has not been any full-length study that investigated the effects of increasing federal funds on the mathematical profession as a whole. The only exception is Amy Dahan-Dalmedico's study of the growth of applied mathematics during the war and its aftermath. Dalmedico's study points to some areas of research such as statistics, probability, operations research, nuclear reaction, and game theory, which emerged out of military research. Yet it does not offer a comprehensive analysis of the ways in which federal support altered the American mathematical profession writ large. See: Amy Dahan-Dalmedico, "L'essor Des Mathématiques Appliquées Aux États-Unis: L'impact De La Seconde Guerre Mondiale," *Revue D'histoire Des Mathématiques* 2 (1996): 149–213.

⁶ There have been several studies that investigated the penetration of mathematical techniques and theories into specific fields or the emergence of new mathematical fields, such as computing or control theory. This study differs from these by approaching these developments from the perspective of the mathematical community. Namely, I am not interested in the development of specific fields such as operations research, or the application of mathematical techniques in economics. Rather, this study examines how contemporary mathematicians made sense of these changes and how they impacted the development of the field, both institutionally and intellectually. Moreover, studies that examine the growth of computing or game theory as independent disciplines miss the fact that, at least in the first decade after the war, these fields were often seen as a branch of applied mathematics. See for example: Peter Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision," *Critical Inquiry* 21, no. 1 (1994): 228–266; Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (MIT Press, 1997); Agatha C. Hughes and Thomas Parke Hughes, *Systems, Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After* (MIT Press, 2000); Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (Cambridge University Press, 2002); Atsushi Akera, *Calculating a Natural World: Scientists, Engineers, and Computers During the Rise of U.S. Cold War Research* (MIT Press, 2008); Robert Leonard, *Von Neumann, Morgenstern, and the Creation of Game Theory: From Chess to Social Science, 1900-1960* (Cambridge University Press, 2010); Paul Erickson, "Mathematical Models, Rational Choice, and the Search for Cold War Culture," *Isis* 101, no. 2 (2010): 386–392; George Dyson, *Turing's Cathedral: The Origins of the Digital Universe* (Random House Digital, Inc., 2012).

mathematicians began to be supported by direct contract grants. The National Science Foundation played an especially significant role in the development of the field, sponsoring conferences, workshops, publications, and a host of translation projects, as well as fellowships for doctoral and postdoctoral students. In the aftermath of the war mathematicians might not have been deemed as valuable a commodity as were physicists, but they were included in the nation's scientific manpower demands.⁷ Especially after Sputnik, this was reflected in the exponential growth in the number of mathematics Ph.D.s (see figure two). Yet unlike the physical or biological sciences, mathematics remained by and large a scholarly pursuit.⁸ Besides computing, the field did not require expensive experimental apparatus. Mathematics was "Little Science," and the majority of research was still conducted on an individual basis rather than in large teams.⁹ The discipline was altered by the war, but it also remained remarkably the same.¹⁰

⁷ On scientific manpower see: David Kaiser, "Cold War Requisitions, Scientific Manpower, and The Production of American Physicists After World War II," *Historical Studies in the Physical and Biological Sciences* 33, no. 1 (2002): 131-159; David Kaiser, *American Physics and The Cold War Bubble* (University Of Chicago Press, forthcoming).

⁸ Substantial mathematical work had been conducted, of course, in industry, government, and think tanks in the postwar period. Yet, for the purpose of this study, I let "mathematician" denote a person who obtained a Ph.D. in mathematics. The majority of mathematicians trained in the postwar period resumed teaching and research positions in universities.

⁹ The literature on Big Science has for the most part revolved around the use of expensive apparatus and large collaborations. See: Derek John de Solla Price, *Little Science, Big Science* (Columbia University Press, 1963); James H. Capshew and Karen A. Rader, "Big Science: Price to the Present," *Osiris* 7, 2nd Series (1992): 3-25; Galison and Hely, *Big Science: The Growth of Large Scale Research* (Stanford University Press, 1992).

¹⁰ An additional feature of departments of mathematics that aligned them closely with the humanities was that within the university, instruction in the field were understood in terms of service. Departments across the humanities were integral to universities' adherence to the goals of general education. Faculty research in departments of English or Philosophy was concomitant with their goal of providing undergraduates with broad-based liberal education. Despite some mathematicians' dissatisfaction, instruction in mathematics was also conceived of in terms of academic service. Most undergraduates enrolled in mathematics courses, especially at an elementary level, were not mathematics majors. The growth in departments of mathematics was spurred by undergraduate teaching as much as by academic research.

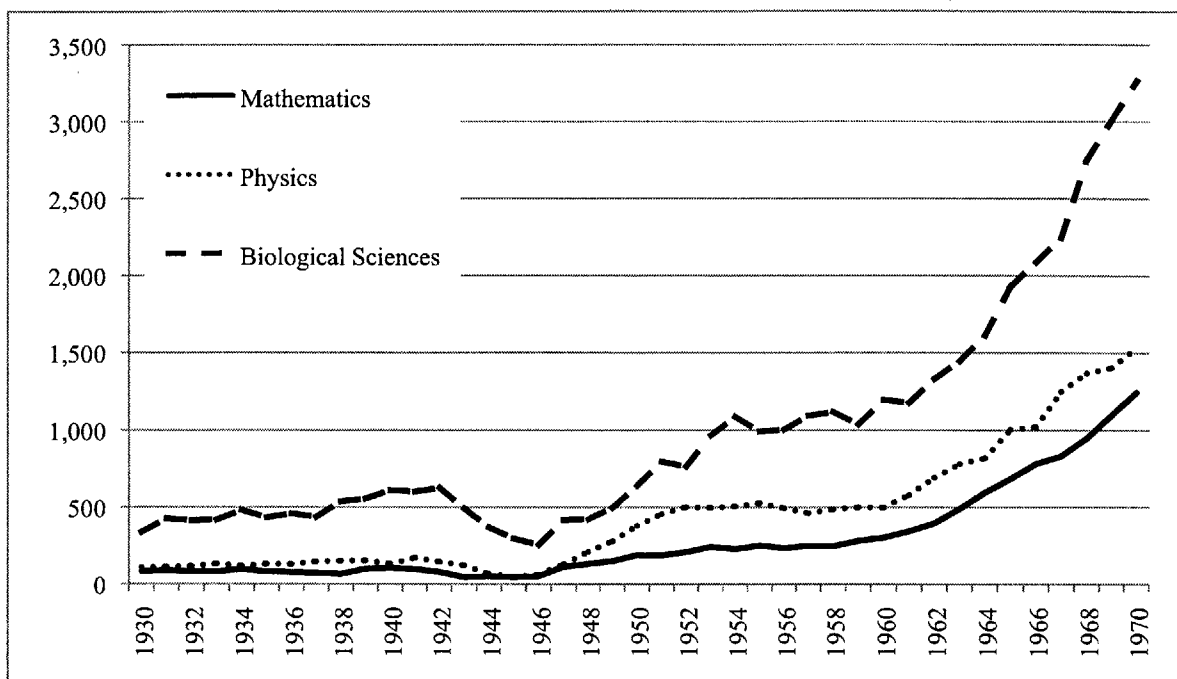


Figure One: The growth in the production of PhDs in mathematics, physics and the biological sciences. *Source:* Adkins, Douglas L. *The Great American Degree Machine: An Economic Analysis of the Human Resource Output of Higher Education : a Technical Report Sponsored by the Carnegie Commission on Higher Education.* The Commission, 1975.

Intellectually this divergence was delineated across the distinction between pure and applied mathematics. The closer applied mathematics moved towards the sciences, the freer pure mathematics was to develop as a humanistic field. At the same time that IBM hailed mathematics as providing the key to all humanity’s needs and dreams, most scientists criticized mathematicians for pulling away from the rest of the sciences and for promoting the development of mathematical theories as independent of the physical world. The emphasis on abstractness and generalization that was prevalent among American research mathematicians in the prewar period did not diminish in the aftermath of World War II. On the contrary, it only increased.¹¹ Mathematical tools and techniques began to be increasingly used not only in the

¹¹ On the turn toward abstractness in mathematics see: Leo Corry, “The Origins of Eternal Truth in Modern Mathematics: Hilbert to Bourbaki and Beyond,” *Science in Context* 10, no. 02 (1997): 253–296; Leo Corry, *Modern*

natural and social sciences, but also in government and industry. Yet, throughout the postwar period, mathematicians maintained that the field developed according to its own internal logic. Mathematics in Cold War America was characterized by two opposing tendencies, whereby mathematics was at once deeply embedded in the world and manifestly independent of it.

A relentless occupation with the question of what is mathematics pervaded mathematical discourse in the period as a way of coming to terms with this inherent ambiguity. New areas of research such as game theory, control theory and computing called into question the intellectual boundaries of the field. By 1968, when the National Research Council commissioned a report on the state of mathematical research in the US, the authors of the report felt it was necessary to publish an accompanying edited volume of essays outlining the contours and boundaries of the field.¹² In the introduction they explained that the book was necessary because not only are most policymakers and other scientists unaware of the constitution of the field, but so were most mathematicians. The “proper” domain of mathematical knowledge became a constant worry for mathematicians.

A related but even more troubling question was the relation between abstract mathematical theories and mathematical applications. If the question “what is mathematics?” was difficult to answer, the question “what is applied mathematics?” turned out to be impossible. Applied mathematics simply resisted definition. In part this was due the fact that the field of applications continued to grow, but more fundamentally it was because mathematicians could

Algebra and the Rise of Mathematical Structures (Springer, 2004), Chapter 8–9; Leo Corry, “Axiomatics Between Hilbert and the New Math: Diverging Views on Mathematical Research and Their Consequences on Education,” *International Journal for the History of Mathematics Education* 2, no. 2 (2007): 21–37; Nathan Reingold, “Mathematics: The Devotees of the Abstract,” in *Science in America: A Documentary History 1900-1939*, ed. Nathan Reingold and Ida H. Reingold (Chicago, IL: The University of Chicago Press, 1981), 110–125; Jeremy Gray, *Plato's Ghost: The Modernist Transformation of Mathematics* (Princeton, NJ: Princeton University Press, 2008).

¹² National Research Council, Committee on Support of Research in the Mathematical, *The Mathematical Sciences: A Collection of Essays* (Cambridge, MA: M.I.T. Press, 1969).

not agree on how mathematical knowledge should and does progress. Applied mathematics, most mathematicians concurred, was not a coherent collection of subjects or mathematical techniques. Fluid dynamics or mathematical statistics could be identified as applied mathematics, but what united these various areas of research? Instead, applied mathematics was often presented as a matter of attitude, a willingness to apply mathematical analysis outside of the strict confines of the field. Moreover, despite persistently using the two labels most mathematicians acknowledged that it was impossible to make any clear-cut distinction between pure and applied mathematics. A research paper in aerodynamics could lead to new advances in the theory of partial differential equations and vice versa.¹³

In popular accounts, national surveys, and committee reports, what is mathematics and what is the nature of mathematical knowledge became the prominent trope of the period. This uncertainty was reflected in the language mathematicians used to describe their field. In the first decade following World War II, “mathematics” was used by some practitioners to denote the totality of all mathematical research regardless of its methodology or aim, and by others to mean “pure mathematics,” as distinguished from “applied mathematics.” These designations reflected deep intellectual fissures. Was applied mathematics a byproduct of abstract research? A subfield akin to algebra and geometry? Or was it an independent field that developed according to its own standards and logic? Theoretical, basic, abstract, and fundamental were all labels mathematicians

¹³ In 1968, when a committee of mathematicians was in charge of studying the current editorial policies of the *Mathematical Review*, it reflected on the current classificatory system, which made a division between pure and applied mathematics. “The division at Section 68 (Computing machines) is somewhat arbitrary. For example, Section 62 (Statistics) and 65 (Numerical methods) have a comparatively high proportion of ‘applied’ articles. Section 93 (Control Theory) had always contained quite a few papers in ‘pure’ mathematics; in the new Subject Classifications, an attempt is being made to redistribute some of these articles in the sections on differential equations and calculus of variations. A considerable number of papers formerly put in Section 81 (Quantum Mechanics) now appear in the section on Lie Groups.” Despite these difficulties, policies were, nonetheless, constructed across the line dividing pure from applied. “Editorial Committee for Mathematical Review,” AMSR, Box 16, Folder 128.

used at some point or another to draw distinctions between various research fronts. The 1960s saw the emergence of a new term of art – “the mathematical sciences” – a phrase that reflected the field’s expansion and the growth of aligned research fields such as computing, operations research, and statistics. They simultaneously began referring to “core mathematics” and “classical applied mathematics” as a way of demarcating the boundaries of the field. Changes in terminology were often a source of much debate and mathematicians on both sides were prone to appeal to history and philosophy in order to make their respective cases.

In the postwar period mathematical rhetoric ran high because it mattered. The question what is mathematics was always as much about the disciplinary formation of the field as it was about its ontology. The nature of mathematical knowledge, as has been clearly demonstrated by historians, has been a source of concern for mathematicians for centuries. Yet, never before World War II were these questions imbued so deeply and on such a large scale within the politics and health of the State as they were in Cold War America. At stake were concerns about what departments should be established, which research should be funded, how students should be trained, and for what purpose. As some mathematicians veered toward the sciences and other toward the humanities, questions about the boundaries of mathematics triggered heated debates in lecture halls and the offices of national funding agencies. Mathematicians might not have been able to agree on where to draw the line between pure and applied mathematics, or even if one exists, but they nonetheless wrote it into the institutional formation of the field.

Inequalities

At least from an institutional perspective, the division between pure and applied mathematics emerged at the end of the nineteenth century around the German research universities. The

elevation of pure mathematics into an independent area of research following its own standards of rigor was, in fact, part of the professionalization of the field during the period.¹⁴ The elevation of polytechnic schools to full academic status and the growing disagreements between engineers and mathematicians gave rise to the conception of applied mathematics as an independent field of study. As Herbert Mehrtens noted, “ironically, the separation of applied mathematics with separate positions helped to make room for pure mathematics to develop the purist modernist version that would dominate much of the twentieth century.”¹⁵ The American mathematical research community that came into its own at the end of the nineteenth century was modeled on the German model, and espoused to the ideals of mathematical modernism characterized by the separation of mathematics from the physical world and the belief that mathematics deals with man-made concepts.¹⁶

¹⁴ Studies on German mathematics distinguish for the most part between two mathematical traditions, one centered at Göttingen and the other at Berlin. The Berlin mathematical center followed a neo-humanist orientation promoting pure mathematics to the almost complete neglect of applied mathematics. The Göttingen center, under the leadership of Felix Klein and later David Hilbert, did study mathematics with connections to the natural sciences, though it also saw the separation of applied and pure mathematics on an institutional level. See: Gert Schubring, “The Conception of Pure Mathematics as An Instrument in the Professionalization of Mathematics,” in *Social History of Nineteenth Century Mathematics*, ed. Herbert Mehrtens, H. J. M. Bos, and Ivo Schneider (Boston, MA: Birkhäuser, 1981):111-134; Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (American Philosophical Society, 1983); Gert Schubring, “Pure and Applied Mathematics in Divergent Institutional Settings in Germany: The Role and Impact of Felix Klein,” in *The History of Modern Mathematics: Institutions and Applications*, ed. David E. Rowe and John McCleary (San Diego, CA: Academic Press, 1989), 109–128; Eberhard Knobloch, “Mathematics at the Berlin Technische Hochschule/Technische Universität: Social, Institutional, and Scientific Aspects,” in *The History of Modern Mathematics: Institutions and Applications*, ed. David E. Rowe and John McCleary (San Diego, CA: Academic Press, 1989), 109–128; Herbert Mehrtens, *Moderne Sprache Mathematik* (Suhrkamp, 1990).

¹⁵ Herbert Mehrtens, “Mathematical Models,” in *Models: The Third Dimension of Science*, ed. Nick Hopwood and Soraya de Chadarevian (Stanford, CA: Stanford University Press, 2004), 297.

¹⁶ The 19th century is often characterized by the break of mathematics from physical reality and the realization that mathematics deals with man-made concepts. According to this view as expressed by Morris Kline, “the introduction of quaternions, non-Euclidean geometry, complex elements in geometry, n -dimensional geometry, bizarre function, and transfinite numbers forced the recognition of the artificiality of mathematics.” Kline, further notes, that “The introduction and gradual acceptance of concepts that have no immediate counterparts in the real world certainly forced the recognition that mathematics is a human, somewhat arbitrary creation, rather than an idealization of the realities in nature, derived solely from nature.” Morris Kline, *Mathematical Thought from Ancient to Modern Times: Volume 3* (Oxford University Press, 1990), 1032.

This emphasis on research in pure mathematics to the neglect of applied areas of research continued throughout the first three decades of the twentieth century, despite intermediate calls by concerned mathematicians (especially during World War I).¹⁷ The main change in the growth of the community came during the 1930s with the large migration of European mathematicians into the United States. Among the various emigrés were German applied mathematicians such as Richard Courant and Theodore von Karman, who even before the start of the Second World War began promoting research in the field.¹⁸ Nonetheless, when World War II broke out, unlike in Germany, there did not exist a strong tradition of research in applied mathematics in the United States.

The war called into question this historical trajectory. Overnight, applied mathematics garnered the attention of policymakers and scientists. Yet, despite the tremendous changes in the relation between science and the State, the prewar development of the field was not halted or even slowed down. On the contrary, it was only reinscribed by the emergence of applied mathematics and its formation in direct opposition to pure mathematics. In the first decade and a half after the war, the majority of research in applied mathematics and mathematical applications was conducted outside of traditional academic departments at semiautonomous research institutes, such as the Applied Mathematics and Statistics Laboratory at Stanford and the Institute for Numerical Analysis at the University of California, Los Angeles, and in national laboratories such as the National Bureau of Standards. These activities were almost completely dependent on

¹⁷ Karen Hunger Parshall, "Perspectives on American Mathematics," *Bulletin of the American Mathematical Society* 37, no. 4 (2000): 381–406.

¹⁸ On the migration of mathematics to the United States during the 1930s, see: Nathan Reingold, "Refugee Mathematicians in the United States of America, 1933–1941: Reception and Reaction," *Annals of Science* 38, no. 3 (1981): 313–338; Reinhard Siegmund-Schultze, *Mathematicians Fleeing From Nazi Germany: Individual Fates and Global Impact* (Princeton, NJ: Princeton University Press, 2009). On the limited research in applied mathematics in the decades leading up to the war, see: Reinhard Siegmund-Schultze, "The Late Arrival of Academic Applied Mathematics in the United States: A Paradox, Theses, and Literature," *NTM International Journal of History & Ethics of Natural Sciences, Technology & Medicine* 11, no. 2 (2003): 116–127.

federal funding, specifically from the Department of Defense. In the 1960s, as computing and statistics began to steadily divorce themselves from the proper domain of mathematical research and grew into in autonomous independent fields, this trend was further crystallized in institutional contexts with the establishment of separate departments of computer science and statistics around the country. Consequently, mathematics, or rather pure mathematics, remained the sole province of academic departments.

Advanced training programs in the field only perpetuated this institutional separation between pure and applied mathematics. The number of mathematics Ph.D.s began to rise exponentially only in the post-Sputnik period. Yet the distribution of growth among the various subfields of research was not equal. Throughout the 1960s, approximately 60% of all doctorates wrote their dissertation in areas of pure mathematics. Ph.D.s in applied mathematics accounted for a mere 16% and those in statistics and probability for about 20%.¹⁹ New doctoral granting programs in mathematics were founded at a staggering rate during the decade, yet they for the most part followed the traditional departmental structure. Topology, algebra, number theory, and analysis were the main areas of research, not fluid dynamics or electromagnetic theory. Moreover, while all students in applied mathematics were expected to be well-versed in the theories of abstract algebra and complex analysis, this was not the case when it came to pure mathematicians. Most programs did not have any official requirement for their students to take a course in applied mathematics or, for that matter, in any scientific field. Graduate work in

¹⁹ The distribution of doctorates in mathematics by field from 1957 until 1966 is available from the National Academy of Science reports. See: National Research Council (U.S.) Office of Scientific Personnel, *Doctorate Production in United States Universities, 1920-1962: With Baccalaureate Origins of Doctorates in Sciences, Arts, and Professions* (National Academies, 1963); National Research Council (U.S.) Office of Scientific Personnel Research Division, *Doctorate Recipients from United States Universities, 1958-1966: Sciences, Humanities, Professions, Arts; A Statistical Report* (National Academy of Sciences, 1967). For data on the years 1967 until 1973, see "Doctorate in Mathematics and Related Areas, July 1967 – June 1973," *Notices of the American Mathematical Society* 21 (1974), 254.

mathematics was focused on independent specialized research. During a conference in 1966, Stanislaw Ulam remarked on the narrow and isolated tendency of mathematics Ph.D.s by proclaiming that if you stop students at random most would not be able to explain the difference between an electron and a neutron.²⁰ Increased federal support for graduate education and the inclusion of mathematics, regardless of specialty, among the sciences enabled departments of mathematics to expand without making major changes in graduate training in the field.

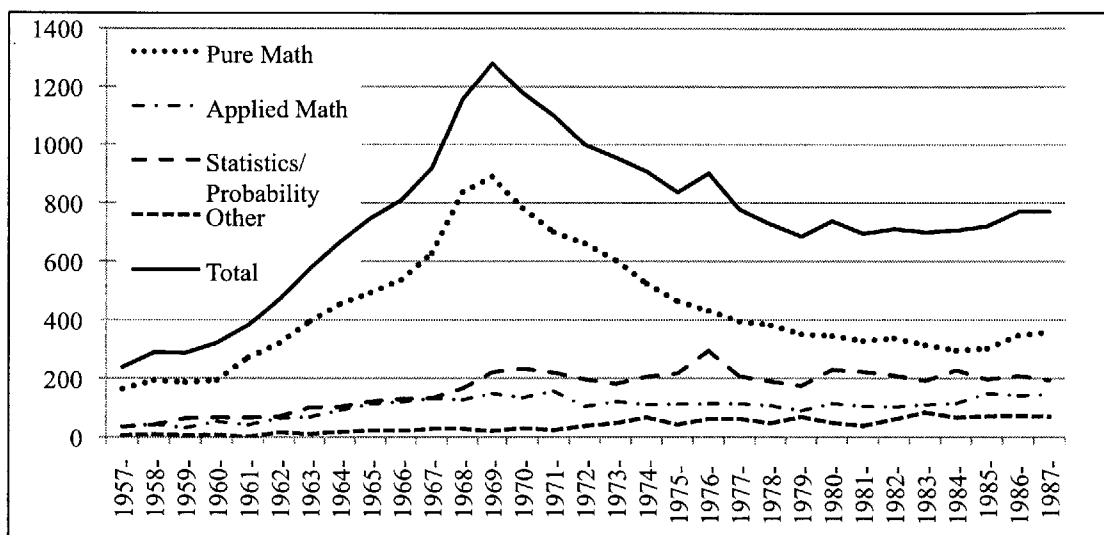


Figure Two: Distributions of Ph.D.s in mathematics by area of research. The information for this table was collected through an annual survey of the American Mathematical Society and is based on self-reporting. Of course, it is impossible to clearly draw the distinction between pure and applied, but as long as the inaccuracies are consistent, the trend is obvious.

Possibly the starkest distinction in the postwar period was not between applied and pure mathematics, but rather between applied and pure mathematicians. The applied mathematician as a distinct professional identity did not exist in the United States prior to World War II. Emerging out of war related research, the applied mathematician was fashioned as a team player, a man of

²⁰ Ulam's statement is recounted in, H. J. Greenberg, ed., "Proceedings of a Conference on Education in Applied Mathematics," *SIAM Review* 9, no. 2 (April 1967): 323.

action interested in concrete problems rather than abstract formalisms.²¹ The applied mathematician was seen in direct opposition to the pure mathematician. He was a scientist, not a scholar. In the postwar period, these two professional identities were constructed less around their intellectual arsenal and more around their personality characteristics. This was clearly reflected in mathematicians' professional trajectories. The employment placement of most graduates in the field, whose research was in areas of pure mathematics, was closer to their peers in the Department of English rather than the Department of Physics. Industry began to hire an increasing number of mathematicians, but as a 1957 report discovered, most of these "mathematicians" were in fact engineers and physicists. Throughout the 1960s, the rate of mathematics Ph.D.s that pursued non-academic positions was approximately 20%.²² Mathematics might have been integral to the military-industrial-academic complex, but mathematicians were not. In the 1950s and 1960s, mathematics was still fashioned as a vocation rather than a job.

American universities played a crucial role in the transformation of scientific research in the postwar period, and were deeply impacted by it. Within the new alliances formed between military agencies, industries and the academic world, it was the role of the university to invest in basic research, produce new knowledge, and train the necessary scientific manpower. Within this new socio-political milieu, universities were no longer perceived as ivory towers. They became instead institutions dedicated to national service.²³ As Steven Shapin notes, "during the Cold

²¹ In referring to the applied mathematician as a man I follow the sources. This is not to say that there were not any mathematically trained women. However, the applied mathematician was almost always assumed to be a man.

²² Committee on the Survey, *A Survey of Training and Research Potential in the Mathematical Sciences: Final Report. Part II.* (Chicago, IL: University of Chicago, 1957).

²³ There is a growing literature investigating the rise of the Cold War university in the United States. These studies attend to the ways in which the Cold War transformed American higher education on both an institutional and intellectual level. See: Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities Since World War II* (Oxford University Press, 1993); Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford*; Rebecca S. Lowen, *Creating the Cold War University: The Transformation of Stanford* (Berkeley, CA: University of California Press, 1997); Noam Chomsky et al., *The Cold War & the University: Toward an Intellectual History of the Postwar Years* (New Press, 1998); David Engerman,

War, it was immediately understood that it was the natural scientists and engineers who had departed the Ivory Tower en masse, leaving the humanities and most of the human sciences behind.”²⁴ The dependence on federal contracts and grants, according to Shapin, implied that scientists and engineers “now lived in a world in which there were always alternatives to the university.”²⁵ This was not the case when it came to mathematics and mathematicians.

Mathematics was included among the sciences within the Cold War university structure and enjoyed many of the fiscal benefits, but mathematicians did not leave the ivory tower en masse. Rather, during the postwar period, the field went through a process of fragmentation and as various mathematical fields gained independence and moved out of the tower, mathematicians, on the whole, remained safely inside.

This was enabled in part by the fact that research in pure mathematics did not require large funds. The bare office and the loose papers in the IBM ad is not far off from the working conditions of the average mathematicians of the period.²⁶ Instead of expensive equipment, military contracts or NSF grants were used mostly to support summer salaries, research

“Rethinking Cold War Universities: Some Recent Histories,” *Journal of Cold War Studies* 5, no. 3 (2003): 80–95; Ron Theodore Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton, NJ: Princeton University Press, 2009); Louis Menand, *The Marketplace of Ideas: Reform and Resistance in the American University* (New York, NY: W. W. Norton & Company, 2010); Kaiser, *American Physics and The Cold War Bubble*.

²⁴ Steven Shapin, “The Ivory Tower: The History of a Figure of Speech and Its Cultural Uses,” *British Journal for the History of Science* 45, no. 1 (2012), 21.

²⁵ Ibid.

²⁶ In 1957 a National Survey was conducted to study the research potential of the mathematical sciences. The survey committee visited and conducted interviews at fifty-six departments of mathematics around the country to support the survey’s recommendation. Under the headline “the mathematician in the university,” the committee made the following two recommendations: “we recommend that university administrative officers be reminded that the staff office is the laboratory of the mathematician, and that private office space is just as necessary a research facility as is the laboratory of the experimental scientists”; and “we recommend that university administrative officers be asked to recognize the importance of a library, readily accessible to the mathematician in his staff office, as a research tool, and that libraries be made so accessible and adequately supported.” Committee on the Survey, *A Survey of Training and Research Potential in the Mathematical Sciences: Final Report. Part II*. (Chicago, IL: University of Chicago, 1957), 3–4.

assistants, and traveling.²⁷ As long as mathematicians insisted that there was no clear way to separate pure from applied mathematics, the place of pure mathematics was secure. Throughout most of the postwar period, mathematicians were able to exist both within the military-industrial-academic complex and outside of it. The field enjoyed the fiscal benefits of the Cold War like the rest of the sciences, but managed to maintain incredible autonomy over the development of the field. The growth of the field as a humanistic pursuit persisted and the study of mathematical theories separate from real world concerns only increased.

Mathematics is unique among academic disciplines. Its development during the Cold War stands alone among the natural sciences, the social sciences, and the humanities. The ongoing back and forth between the intellectual making of the field and its disciplinary and institutional formation is laid out in the open when it comes to mathematics. Discussions about what is mathematics were always at heart about the future growth of the field. The relation between pure and applied mathematics and between mathematics and the rest of the sciences were disciplinary concerns as much as they were philosophical rumination. As the reconfiguration of the mathematical field during the second half of the twentieth century shows, the dynamic relation between the natural and the human sciences reveals as much about institutions, practices, and nations as it does about epistemological commitments.

A Countable Set

Chapter One focuses on the tension created early on in the war between the newly established scientific establishment, composed mostly of physicists, and the leadership of the American

²⁷ This change marked a substantial transformation for mathematicians as it provided them with more freedom and time for research (and less teaching responsibilities). Nonetheless, the budget for mathematics represented only a small fraction of the total federal funds devoted to scientific research and development.

mathematical community. The two groups did not agree on how mathematicians should be incorporated into defense research. Unlike American physicists, who were known for their pragmatic and experimental approach, the relatively small American mathematical community tended to focus on theoretical research in pure mathematics and was for the most part isolated from the rest of the sciences and engineering. The conception of American mathematicians as non-utilitarian and non-pragmatic divided the country's mathematicians and scientists and had real effects on the way the federal government mobilized mathematicians during the war. The strained relation between mathematicians and the scientific leadership, however, was not merely a dispute about power and authority. Rather, it was rooted in real philosophical disagreements about the nature of mathematical work and the relation between mathematics and science that continued to mark the development of the mathematical discipline during the postwar period. Nowhere were these tensions more clear than when it came to applied mathematics and applied mathematicians.

The military effort called for advances in fields such as fluid dynamics, plasticity, ballistics, electromagnetic field theory, and statistics. Yet these areas of research were almost completely neglected by American mathematicians, who were better known for their study of abstract algebra and topology. The war changed that. Overnight, applied mathematics became the center of attention to mathematicians, scientists, and policy makers. Yet it was not just research in the field that came into existence during the war, it was also the applied mathematician as a distinct professional identity. A new mathematical "type" that was born out of war research, the applied mathematicians was fashioned as a mathematician by training and a scientist by disposition. Despite, or rather because of, the tremendous research conducted in the field, at the end of the war it was not quite clear what constituted applied mathematics. Did the

various research fronts, methodologies, and techniques cohere into a unified whole? And further, how did it fit within the broader framework of mathematical research in the United States?

Chapter Two follows this line of argument by examining how the division between pure and applied mathematics was articulated in the first decade and a half after the war in light of the changes in funding regimes. Despite the substantial federal support provided to applied mathematics in the postwar period, the field did not become a staple of academic research in the decade after the war. Applied mathematicians bitterly argued throughout the 1950s that an imbalance existed between the development of pure and applied mathematics in the United States. They were not vying for money, which was for the most part easily afforded to research in the field; rather, they struggled for recognition. A presumed hierarchy that placed pure squarely above applied research impeded, they argued, the development of the field. While military funding was necessary for the growth of the field, it was in no way sufficient. The chapter challenges, therefore, the reigning historiography that maintains that military funding dictated the development of scientific research in the postwar period. As the case of applied mathematics demonstrates, research agendas were as much dependent on philosophical commitments, historical precedents, and mythologies as they were on dollars.

The chapter specifically focuses on the way in which mathematicians on both sides adopted the rhetoric of basic and applied research and the way such language was mapped onto the distinction between pure and applied mathematics. The emergence of research in applied mathematics in specific war projects construed the field under the rubric of applied research. Yet, as many mathematicians argued, research in applied mathematics could just as easily count as basic research. Developing the theory of elasticity did not imply that practical applications were immediately apparent. Pure mathematicians similarly sought to present research in the field in

terms of basic research. However, pure and basic research are not one and the same thing. Whereas the latter assumed that the research holds potential for application, the former is regarded as research for its own sake. Within the new funding regime pure and applied mathematicians found themselves engulfed in a battle over definitions and terminology. This battle over rhetoric only served to further sever the ties between pure and applied mathematics in the postwar period. Despite mathematicians' insistence that intellectually there was no way to draw a line between the two, the line was nonetheless carved forcefully and enforced through the military agencies and the NSF funding mechanisms that divided scientific research according to basic and applied research.

Chapter Three continues by analyzing how the separation between pure and applied mathematics was reinforced via the construction of two opposing professional identities. The first two programs dedicated to training applied mathematicians emerged during the war at Brown University and New York University (NYU). While both programs were founded in order to fulfill the demands of the defense establishment, they developed according to two different philosophies. The program at Brown University aligned applied mathematical training with engineering concerns and industrial demands, whereas the one at NYU aimed to develop research in applied mathematics as a theoretical field close to the physical sciences. The existence of these differing visions continued to mark training in the field in the postwar period, which did not cohere around a central doctrine. Applied mathematicians were neither pure mathematicians, nor scientists and engineers, but it was not quite clear how to translate this sentiment into a coherent training regime. The marginalization of training in the field was further increased by the fact that most mathematicians could not agree on what constituted applied mathematics. Mathematician William Prager perfectly summarized this state of affairs when he

noted during a conference on training in applied mathematics that if you would ask those in attendance to provide a definition for the field, one would end up with as many answers as participants.²⁸ Applied mathematics was therefore described as a willingness to use mathematical theories to solve problems arising across a variety of fields.

The applied mathematician was to be constructed as a separate professional identity according to his personality characteristics and proclivities. Training in the field was conceived of, at least in part, a way of training mathematicians to assume industrial positions. That the pure mathematician was a poor industrial researcher was taken as a truism.²⁹ Programs in applied mathematics were supposed to solve this problem. Yet mathematicians' inability to decide what are the contours of the field and how should educational agendas separate the applied mathematician from the pure mathematician, on the one hand, and the engineer, on the other, implied that they focused instead on the persona of the mathematician.

Chapter Four turns to the 1960s and the reconfiguration of mathematical knowledge in light of the discipline's immense growth in both scale and scope. The expansion in mathematical applications both within and outside the academic domain was met in the two decades following the war by a similarly growing trend toward increased abstraction and generalization. To account for these two seemingly opposing tendencies, a new term – the mathematical sciences – gained credence. This change in terminology denoted a new conceptualization, which sought, at least for a short while, to present a more unified image of the mathematics profession. The mathematical sciences included statistics, computing, management sciences, operations research, mathematical economics, mathematical biology, as well as abstract algebra, topology and number theory.

²⁸ William Prager, "The Graduate Division at Brown University," in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 35.

²⁹ The criticism voiced against him was similar to the one heard during the war against mathematicians in general.

However, unlike the 1950s' antagonistic perception of the field that pitted pure against applied mathematics, the new term signified a more harmonious image of the field itself. "Pure" was replaced by "core," indicating a new relation between abstract theories and applications, and applied mathematics was now subsumed under the applied mathematical sciences.

However, this new image of the field that aimed to provide a more integrated view of mathematics and its applications only had the counter-intuitive effect of further separating "pure" mathematics from applied mathematics and mathematical applications. Specifically it resulted in the cordoning off of (pure) mathematics as an academic pursuit and the neglect of classical applied mathematics. The term both reflected and helped promote a growing institutional division among the mathematical sciences. The growing separation of computer science, statistics, and operations research into independent departments was understood in terms of increased autonomy. As these fields became independent they followed their own sets of standards, which were not dictated by those of pure mathematics. Thus, to a certain degree the successful growth of the mathematical sciences also brought about their dissolution. As a consequence, "mathematics" could safely return to its prewar state.

Finally, Chapter Five surveys the response of the mathematical community to the job market crisis during the first half of the 1970s. It argues that the collapse of the field in the 1970s exposed the central conflict that had characterized its postwar expansion: namely, its growth as an autonomous humanistic field of study on the one hand, and as a scientific practical field on the other. Core mathematics felt the changes in federal funding most acutely. When research and teaching jobs became scarce, mathematicians were confronted with the fact that training in the field has been almost solely focused on producing more academic researchers. The lack of training in applied mathematics and the growing tendency toward specialization implied that

most doctorates in mathematics were unsuited for non-academic positions. The major impetus behind the field's expansion was its promise of practical application, but this was not reflected in the job market. Many mathematicians began calling for a complete overhaul of the mathematics degree, focusing in particular on the lack of training in applied mathematics and science. Only in the 1970s, as the production rate of Ph.D.s began to drop, did the proportion between applied and pure mathematicians begin to change.

Chapter 1.

If and Only If: Mathematicians at War

For other purposes, action is served by elimination of immediate sense as far as possible. The attitude is prosaic; it is best subserved by mathematical symbolism; mathematical not signifying something ready-made, but being simply the devices by which mind is rigidly occupied with instrumental objects, by means of artificial inhibition of immediate and consummatory qualities, the latter being distracting for the activity at hand, the consummatory phase cannot be suppressed or eliminated however; nature pitched through the door returns through the window. And the common form of its return is falling down in worship or in fear before the resulting mathematico-mechanical object.

– John Dewey, *Experience and Nature*, 1925

The dinner had just ended and Marshall Stone took to the podium to begin his speech as the departing president of the American Mathematical Society (AMS). “The topic of my remarks this evening,” he began, “is a rather serious one to take up after a dinner so pleasant and so friendly as the one we all have been enjoying.”¹ The audience included members of the Society, the Mathematical Association of America (MAA), and the Institute of Mathematical Statistics (IMS), all of whom were at Wellesley College for the annual summer meeting of the three societies. “I fear,” Stone continued, “that the tone of my discussion cannot be altogether gratifying to our professional pride, since the history of American mathematics in the present war has been in many respects a story of frustration.”² It was August 13, 1944. The Western Allies had successfully invaded northern France two months earlier, but the war was not over. Yet here was Stone in a room full of mathematicians ready to summarize the activities of American mathematicians during the previous five years. Stone explained, “We should all look at the record at this moment when the urgency of our national cause gives every detail of that record the fullest possible significance – and yet a time when the inevitable note of protest can in no sense be

¹ Marshall Stone, “American Mathematics in The Present War,” *Science* 100, no. 2607 (1944): 529–535.

² *Ibid.*

interpreted as obstructing positive achievements.”³ The time was ripe, according to Stone, for a level-headed analysis of mathematicians’ engagement in war research.

The speech lasted for about an hour, and Stone provided a detailed account of the various efforts mounted by mathematicians in support of the war. His goal was to protest what he conceived of as the underutilization of the mathematical talent of the country during the present crisis. Stone’s history of frustration was a story composed of mathematicians’ repeated attempts to interest those in charge of their service only to be politely turned down. His main criticism was directed toward the National Defense Research Council (NDRC). To ensure that his point came across, Stone provided members of the audience with a chart summarizing the events of the previous five years (see Figure 1). The two parallel lines, the one on the right representing the activities of the mathematics profession and the one on the left representing those of the NDRC, visually summarized the essence of Stone’s complaint. Namely, the two lines were parallel with only one *dotted* line connecting them. The activities of each organization were pursued in separation with only minimal collaboration. Stone’s chart was intended as a critique, but in this chapter I use it to analyze the relation between the scientific establishment and the mathematics profession during the war and its implications for the development of applied mathematics in the United States during the war and its aftermath.

³ Ibid.

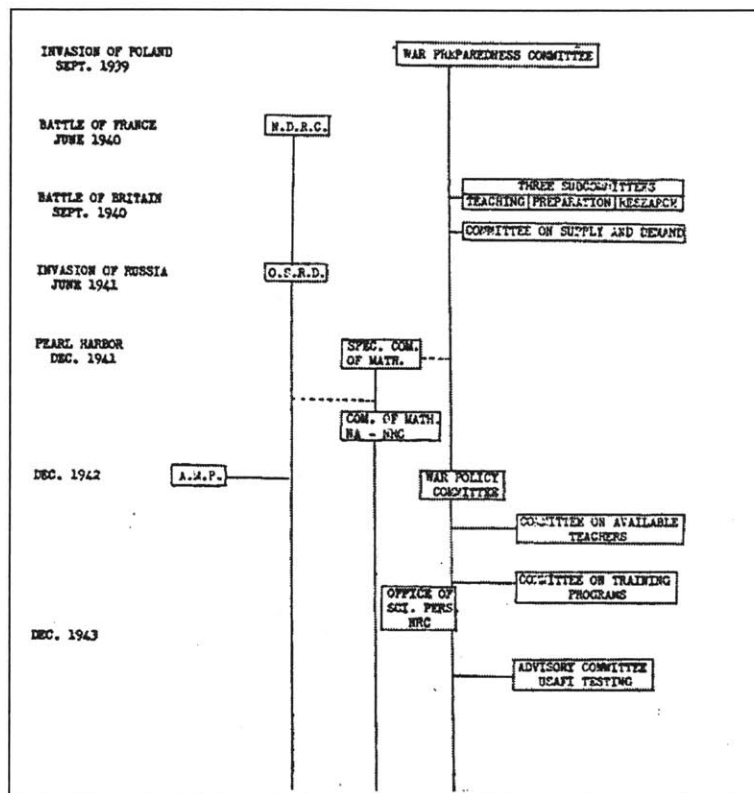


Figure 1: Stone's chart representing the activities of the mathematical profession during the war. When he introduced his chart to the audience, Stone noted, "I must confess that my chart is reminiscent of a topologist's nightmare!" Marshall Stone, "American Mathematics in The Present War," *Science* 100, no. 2607 (1944): 529–535.

Historians have noted that World War II stimulated the growth of applied mathematics in the United States.⁴ The field, which before the war had languished, overnight became an object of

⁴ The idea that the growth of applied mathematics in the United States was to a large degree an outcome of World War II has been advanced by both mathematicians and historians of mathematics. These studies, for the most part, highlight the rise of specific areas of research, such as operations research and statistics, or point to the influence of specific German immigrants, such as Theodore van Kármán and Richard Courant, in advancing applied mathematics in the United States. This chapter departs from earlier studies in two important ways: first, the emphasis here is not on the growth of applied mathematics research, but rather on the coming into being of the applied mathematician as a recognizable American type; second, by focusing on the war and its immediate aftermath, I show that applied mathematics was neither a stable nor a recognized category at the time. Of course, there were fields of research (such as mechanics and hydrodynamics) that practitioners at the time identified as applied mathematics, but any historical discussion that takes the term "applied mathematics" as a coherent entity fails to acknowledge the difficulties and

national attention for policy makers, military personnel, and scientists. As many scholars noted, war-related research not only prompted the development of new research agendas such as computing and operations research, but it also renewed interest in older fields such as mechanics, elasticity, and statistics. Yet during the war how mathematical research should be developed and how mathematicians should be incorporated into defense research became a source of heated debate between the newly established scientific civilian authority and the American mathematical elite. The two communities treated one another with skepticism and suspicion and could not agree on the role mathematics had to play in the war effort. This was due to the fact that American physicists and mathematicians espoused two opposing philosophies – pragmatism vs. idealism. Whereas American scientists were known for their instrumentalism, their elevation of practice over theory, and their emphasis on the concrete, American mathematicians promoted theoretical generalization, abstractness, and the study of mathematics independent of the physical world.

Nowhere was the friction between these two ideologies more strongly felt than when it came to the development of research in applied mathematics. The formation of the discipline in the war was shaped by this tension and continued to affect its development in the postwar period. World War II certainly promoted certain areas of research, but even more transformative was the

struggles mathematicians at the time faced when discussing the future of the field. For accounts by mathematicians see: Mina Rees, “The Mathematical Sciences and World War II,” *American Mathematical Monthly* 87, no. 8 (1980): 607–21; Barkley Rosser, “Mathematics and Mathematicians in World War II,” *Notices of the American Mathematical Society* 29, no. 6 (1982): 509–515; Peter D. Lax, “The Flowering of Applied Mathematics in America,” *SIAM Review* 31, no. 4 (1989): 533–541. For historical accounts, see: John L. Greenberg and Judith R. Goodstein, “Theodore Von Kármán and Applied Mathematics in America,” *Science* 222, no. 4630 (December 23, 1983): 1300–1304; Amy Dahan-Dalmédico, “L’essor Des Mathématiques Appliquées Aux États-Unis: L’impact De La Seconde Guerre Mondiale,” *Revue D’histoire Des Mathématiques* 2 (1996); Karen Hunger Parshall and David E. Rowe, *The Emergence of the American Mathematical Research Community, 1876-1900* (Providence, RI: American Mathematical Society, 1997); Michael Stolz, “The History of Applied Mathematics and the History of Society,” *Synthese* 133, no. 1 (2002): 43–57; Reinhard Siegmund-Schultze, “The Late Arrival of Academic Applied Mathematics in the United States: A Paradox, Theses, and Literature,” *NTM Zeitschrift Für Geschichte Der Wissenschaften, Technik Und Medizin* 11, no. 2 (2003): 116–127; Tinne Hoff Kjeldsen, “New Mathematical Disciplines and Research in the Wake of World War II,” in *Mathematics and War*, ed. Bernhelm Booss and Jens Høyrup (Boston: Birkhäuser-Verlag, 2003): 126–153; Reinhard Siegmund-Schultze, *Mathematicians Fleeing From Nazi Germany: Individual Fates and Global Impact* (Princeton, NJ: Princeton University Press, 2009): 149–213.

emergence of the American applied mathematician as a new professional identity.⁵ Formed during the war, the applied mathematician was fashioned as a mathematician by training and a scientist by disposition. A pragmatic mathematician, this new type was not strictly defined by his research, but instead by *where* he worked, *how* he behaved, and his personal and professional character. Further, the development of research in applied mathematics as part of various defense projects called into question the constitution of the field. The accelerated growth of the field during these years meant that its contours and boundaries were in constant flux, responding to military needs rather than setting its own agenda. In the war's aftermath and throughout the postwar period, it was not clear what applied mathematics *was*. Finally, the rise of applied mathematics between science and mathematics only helped cement the prewar conception of pure mathematics. The applied mathematician was defined by who he was *not* – he was always understood (and often, understood himself) in contradistinction to the archetype of the pure mathematician.

In the prewar era, mathematics had occupied a unique place within the university, existing both within the natural sciences and humanities and apart from them. The American mathematics community, for the most part, privileged pure mathematics above applied mathematics and tended to insulate itself from other fields.⁶ Mathematics was still fashioned as a

⁵ In "L'essor Des Mathématiques Appliquées Aux États-Unis: L'impact De La Seconde Guerre Mondiale." Dahan-Dalmédico argues that the war brought about a "social and culturally different persona for the mathematician," for which she takes the figure of John von Neumann as "the symbol and exemplar of this type." As I make clear in this chapter, the newly constructed applied mathematician was in fact quite distinct from von Neumann. Specifically, von Neumann, a Hungarian immigrant, was a polymath who made contributions to a diverse set of fields. As such, he was more emblematic of nineteenth-century mathematicians than post-World War II ones. Dieudonné, for example, exclaimed that "Von Neumann may have been the last representative of a once-flourishing and numerous group, the great mathematicians who were equally at home in pure and applied mathematics and who throughout their career maintained a steady production in both directions." The postwar applied mathematician was considered to be distinct from the pure mathematician. He was not the 19th century polymath about whom Dieudonné writes, but a new and different sort of professional mathematician. Jean Dieudonné, "Von Neumann, Johann (or John)," in *Complete Dictionary of Scientific Biography*, vol. 14 (Detroit: Charles Scribner's Sons, 2008): 88–92.

⁶ Several historians have made this observation. For example, in their analysis of the emergence of the American mathematical community, Parshall and Row note that the most active research fronts at the turn of the century were

calling, an intellectual and vocational pursuit.⁷ As I show in this chapter, mathematicians' self-conception as detached intellectuals devoted to formal abstractness and generalizations, who have no interest in the outside world, had real effects on the way the federal government mobilized mathematicians during World War II. Mathematicians, or at least a high percentage of them, were deemed by those in charge as simply unsuitable for defense work. Practical individuals who could step out of their private university offices and take part in military-controlled research groups were needed, not individualistic free thinkers consumed by airy abstractions.

During the war, mathematicians and physicists disagreed on what sort of support the mathematical talent of the country could offer the defense establishment. And in committees dedicated to bringing mathematics in line with the war effort, discussions about how mathematics might serve the national agenda were always also debates about what constituted mathematical inquiry. Was mathematics instrumental, a toolkit of theories and results that could be used by other scientists? Or was it the underlying framework of science? Should mathematicians serve their country as team players or as leaders? Such a question was always framed in terms of whether mathematics was the queen or servant of science.⁸ What mathematics was and who was a mathematician were two questions that could not be separated. And who a mathematician was was necessarily answered with regard to how, where, and to what end he worked. An attempt to

in pure mathematics, from abstract algebra to analysis and geometry. Parshall and Rowe, *The Emergence of the American Mathematical Research Community, 1876-1900*. More relevant to this chapter, however, is the fact that American mathematicians' privileging of pure over applied mathematics is a constant refrain of commentators both before and during the war.

⁷ In "Science as a Vocation," Weber goes on to directly compare the work of artists and the work of mathematicians. "The mathematical imagination of a Weierstrass is naturally quite differently oriented in meaning and result than is the imagination of an artist, and differ basically in quality. But the psychological processes do not differ." In the turn of the century, American mathematicians adopted the German model of pure mathematics. Their commitment to pure research was accompanied with a conception of mathematics as a calling. Max Weber, "Science as a Vocation," in *From Max Weber: Essays in Sociology* (New York, NY: Oxford University Press, 1946), 125–156.

⁸ Of course, this question was popularized early on by Eric Temple Bell: Eric Temple Bell, *Mathematics: Queen and Servant of Science* (Providence, RI: Mathematical Association of America, 1951).

answer one of these questions *always* required mathematicians in the 1940s to at least contemplate, if not answer, the others.

I start the chapter by describing the tensions that arose between the professional mathematics community and the newly established scientific civilian authority. Despite various efforts by leading American mathematicians to get involved in defense-related research, mathematicians found themselves separated from the rest of the scientific community. This was due to a difference in scientific outlook, and to a lack of a clear organizational scheme with which to utilize the country's mathematicians. The mathematical leadership and the scientific authority did not agree on how best to do the latter. The section follows mathematicians' various attempts to exert their influence with those in charge on scientific mobilization during the war, calling attention to the way these efforts were rooted in specific mathematical ideology.

In the second section, I track a long correspondence between Warren Weaver, the chair of the Applied Mathematics Panel, and Marshall Stone at the end of 1944. I examine the way the pure and applied mathematician came to be defined in opposition to one another. Stone and Weaver could not have come from more different backgrounds. Stone, born into an intellectual family, was a pure mathematician educated at Harvard, and was recognized as one of the preeminent leaders of the mathematical community. Weaver, on the other hand, trained as an engineer at the University of Wisconsin and spent the prewar years at the Rockefeller Foundation. If before the war, it was Stone who embodied the prestige of the American pure mathematician, the war destabilized this hierarchy. By 1943 Weaver was not only in charge of many mathematicians' war work, but was also closely connected to the rising scientific establishment of the country. Their correspondence reveals how ontology and persona were entangled in the mutual construction of the pure and applied mathematician.

The last section opens at the end of the war, in 1946, with the uproar triggered by a proposal by mathematician John Tukey to create a separate organization within the Society for applied mathematicians. Here, I analyze a series of letters from (mostly applied) mathematicians all over the country regarding the desirability of establishing a separate division for applied mathematics within the Society. The original proposal might have dealt with a fairly simple organizational question, but the correspondence it ignited highlights the uncertainty surrounding the constitution of the field in the wake of the war. Before they could decide how best to support applied mathematics, mathematicians had first to decide what applied mathematics was.

A Mathematical War

American mathematicians' attempts to support the war effort began as early as the summer of 1939 when the AMS and the Mathematical Association of America (MAA) set up a joint War Preparedness Committee (WPC).⁹ The decision to establish the committee, as Stone noted in his speech and indicated in his chart, was made only a few hours after the invasion of Poland in September. The Committee was organized according to two main goals: the first aimed to support military mathematical curricula and education, the second examined war-related mathematical research. Whereas mathematicians were fairly successful in meeting the first objective in the next six years, this was not the case with regard to the second objective.¹⁰ Many American mathematicians were involved in defense projects during the war, but these efforts were

⁹ Throughout the dissertation I refer to the American Mathematical Society as the Society and to the Mathematical Association of America as the Association. This usage is consistent with mathematicians.

¹⁰ Marston Morse, "War Preparedness Committee of the American Mathematical Society and the Mathematical Association of America," 9 September 1940, AMSR, Box 15, Folder 30. In the report, which quickly describes the history of the WPC, Morse, who was the chairman of the committee, writes that after consulting various mathematicians, military authorities, and industrial concerns, "we found considerable confusion both as to what could be done, what should be done, and who was to do it." To a certain degree, this early confusion remained characteristic of the WPC work when it came to the question of research. Note that this version of the report is slightly different than the official one that appeared in the *Bulletin of the American Mathematical Society*.

dispersed. In his 1944 speech, Stone noted, for example, that even after Pearl Harbor, for most of 1942 “there was in fact nothing resembling a systematic mobilization of our mathematical resources...though an increasing number of individuals found their way into war work of mathematical nature.”¹¹ The first systematic attempt came in November 1943, with the establishment of the Applied Mathematical Panel (AMP) at the Office of Scientific Research and Development (OSRD).

Yet the AMP, placed in complete separation on the left in Stone’s chart, was not established in cooperation with the leadership of the mathematics professions and was in fact seen, at least by some, as a direct affront to the established mathematical profession. The problem of how best to employ American mathematicians during the war was not, however, simply a matter of bureaucratic organization. Rather, the “frustration” felt by leaders of American mathematics reflected a deep division in opinion regarding the nature of mathematical knowledge and mathematical work. Early on in their efforts, mathematicians realized that establishing an efficient organizational scheme for employing mathematicians in defense projects required that they first answer questions such as: Given a specific problem, where does the mathematical aspect of the work begin and where does it end? What qualifies one aspect of research as more mathematical than another? What amount of information regarding the original problem does a mathematician need to know in order to be able to do his work?

In July 1940, Stone, in his capacity as the Chairman of the Subcommittee on Preparation for Research of the War Preparedness Committee, wrote a letter to Dunham Jackson, who chaired the Research Committee, reflecting on the unique place mathematics holds in defense research.¹²

¹¹ Stone, “American Mathematics in The Present War,” 532.

¹² Whereas the Subcommittee on Research was in charge of directing problems to assigned consultants, the Subcommittee on Preparation for Research was in charge of gathering bibliographies and texts on mathematical research relevant to war and industry, as well as promoting writing in these areas. In the second report of the WPC,

Stone suggested that the committee should classify all technical problems of defense “first according to mathematical techniques involved,” and second according to level of difficulty.¹³ Noting the paucity of applied mathematicians in the country and the “existing strong cleavage” between pure and applied mathematicians, Stone reasoned that translational work needed to be done in order to take advantage of the intellectual prowess of American mathematicians. A given military problem needs first to be formulated in “mathematical terms” before a solution can be investigated. “I think it is true,” Stone wrote to Dunham Jackson, “that our pure mathematicians are better equipped to handle almost any kind of problem put in mathematical terms *with specifications as to the type of solution required*, than are all but the smallest minority of our applied mathematicians and mathematical physicists.”¹⁴ The familiarity of the mathematical physicists and the applied mathematician with physical theories, on Stone’s account, did not make them in any way more suited for the work at hand. He, therefore, recommended that a chosen number of qualified mathematicians be entrusted with just such a translational job, formulating given problems in mathematical terms, rather than problem solving. Mathematical theory, Stone believed, existed separate from any given problem to which it might be applied. Its efficacy arose out of its generality independently of a specific experimental context.

Stone’s father, Harlan Fiske Stone, had been an Associate Justice of the Supreme Court since 1925. His support for the New Deal gained him the favor of President Roosevelt, who in the summer of 1941 nominated him to the position of Chief Justice. Upon finishing his undergraduate degree at Harvard University, Stone was expected to follow in his father’s footsteps and continue his studies at Harvard Law School. However, Stone became so taken with mathematics that he eventually matriculated into the Department of Mathematics and earned his

in December 1940, Morse explained that “Stone’s committee is closely correlated with the problems of a significant revival of applied mathematics.” AMSR, Box15, Folder 41.

¹³ Marshall Stone to Jackson Dunham, 21 July 1940, in MMP, Box 13, Folder “Stone.”

¹⁴ Ibid.

Ph.D. under the supervision of George D. Birkhoff in 1926. He moved to Columbia and then to Yale, and in 1937 he was appointed as a full professor at Harvard. It is difficult to overemphasize the influence Stone had on the development of twentieth-century American mathematics.

Mathematician Felix Browder wrote that Stone's efforts over the years exemplified "the most highly developed form of charismatic leadership."¹⁵ He was, according to Browder, "a great revolutionary and a great traditionalist. The revolution he made is the only kind which has permanent significance – a revolution that founds or renovates an intense and vital tradition."¹⁶

Early in the war, Stone realized that new bonds forged between the defense establishment and science would entail a complete reorganization of scientific research in the US, and he resolved to ensure that mathematics received its rightful place among the sciences without succumbing to utilitarian principles. His determination and conviction came to fruition in 1946 when he assumed the chairmanship of the Department of Mathematics at the University of Chicago. For the following six years, Stone revitalized the department, attracting the country's top research mathematicians and students; the period was colloquially referred to as "The Stone Age" at Chicago.¹⁷

In his letter to Jackson, who during World War I had served in the Ballistic Units of the Ordnance Department in Washington, Stone suggested that the proposed organization, once set in place, should assign specific problems to highly qualified mathematicians while ensuring that these individuals did not work on straightforward problems. "There is the certainty that such a mathematician will undoubtedly work most efficiently under the conditions to which he has

¹⁵ Felix Browder, "The Stone Age of Mathematics on the Midway," *Mathematical Intelligencer* 11, no. 3 (1989), 23. Browder explicitly invokes Weber in his description of Stone when he contrasts his leadership with the more common academic policy leadership which, he writes, "falls within the rational bureaucratic mold (to use the classical terminology of Max Weber)."

¹⁶ *Ibid.*

¹⁷ For more about Stone's career, see: Marshall Stone, "Reminiscences of Mathematics at Chicago," *Mathematical Intelligencer* 11, no. 3 (1989), 20–25; Joseph D. Zund, "Stone, Marshall Harvey," *American National Biography Online* (2000).

already adapted himself.”¹⁸ Mathematicians, Stone explained, should maintain their academic positions and continue to teach, while substituting “for research problems of their own choosing such difficult problems of defense research as may be put in their hands.”¹⁹ Stone believed, as did many other mathematicians at the time, that theory could be pursued in complete separation from practice. According to this organizational scheme, the content of the work might change, but the nature of the mathematical work as a solitary pursuit would remain stable and similar to prewar mathematical research.²⁰

The members of the War Preparedness Committee realized that regardless of their organizational scheme, the work of the committee would be useless unless it established connections with officials in the military and industry. Marston Morse, as Chairman of the Committee, sent letters to officials in the army and scientific national committees, apprising them of the existence of the organization.²¹ Morse, like Stone, received his PhD from Harvard University in 1917 under the supervision of G. D. Birkhoff. He joined the army immediately afterward and served as an ambulance driver in France during World War I. During the 1920s, he moved between Cornell University and Brown University before returning to Harvard, where he stayed until taking up a position in the Institute for Advanced Studies in 1935. By 1940, Morse was well respected within the American mathematical community. His research on variational

¹⁸ Marshall Stone to Jackson Dunham, 21 July 1940, in MMP, Box 13, Folder “Stone.”

¹⁹ Ibid.

²⁰ Stone’s adherence to the pursuit of mathematics as an intellectual enterprise conducted by highly qualified individuals is also evident in his suggestion to Dunham that for those problems that would prove especially difficult to solve, a prize competition should be set up. “Finally, those problems requiring substantial theoretical advances are not to be handled in any way other than the time-honored one of placing them before all mathematicians as a challenge. It should be possible to divest such problems, once recognized, of their military character and to set them as public ‘prize’ problems.” Not a team of mathematicians, but a competition among individual mathematicians, would encourage timely solutions to such problems.

²¹ Among those contacted were Ross Harrison, Chairman of the National Research Council; General Marshall, Chief of Staff of the US Army; and the Chiefs of Staff of Ordnance, Field Artillery, and Coast Artillery.

analysis and his development of what has since been known as Morse theory is now recognized as one of the major contributions to American mathematics.²²

In May 1940, Morse wrote to the Chief of Ordnance of the Army, Major General Charles Wesson, regarding the committee's work. In his letter to Wesson, Morse explained that the Committee collected "a list of about 100 of the more important mathematicians of the country" who are ready to take part in any defense related research.²³ Two months later, he sent a similar letter to Vannevar Bush, offering that the mathematical community assists in activities conducted under the auspices of the recently established National Defense Research Council. Morse informed Bush that in addition to supporting education initiatives in military sciences the War Preparedness Committee has also compiled a list "of the abler mathematicians of the Society," with the areas of research in which they could offer their expertise.²⁴ He concluded by once more offering mathematicians' "fullest cooperation." Morse did not specify how the list of "abler" and "important" mathematicians was established, yet what is immediately evident is the sparse number of people whom the leaders of the AMS identified as *qualified* mathematicians.

At the outset of World War II, the membership of the AMS was approximately 2,300, but the number of active researchers and the Society's leadership itself was much smaller. The president of the Society, its council members, and its committee members tended to come from the same group of active mathematicians, most of whom were employed by top-tier American universities. The brevity of the list and its enumeration reflects the committee members'

²² On Morse's life and contributions to mathematics see: S. Smale, "Marston Morse (1892–1977)," *The Mathematical Intelligencer* 1, no. 1 (March 1, 1978): 33–34; Raoul Bott, "Marston Morse and His Mathematical Works," *Bulletin (New Series) of the American Mathematical Society* 3, no. 3 (1980), 907–936; Joanne E. Snow and Colleen M. Hoover, "Mathematician as Artist: Marston Morse," *The Mathematical Intelligencer* 32, no. 2 (June 1, 2010), 11–18.

²³ Marston Morse to Major General Wesson, 24 May 1940, MMP, Box 15, Folder "War Preparedness."

²⁴ Marston Morse to Vannevar Bush, 22 July 1940, MMP, Box 15, Folder "War Preparedness."

conception of military research.²⁵ They promote hand-selected *experts* rather than workaday trained mathematicians. The collective, team-driven research projects that characterized much of war research was incompatible with the Society's conception of mathematical work as an individual pursuit. Still, the small number of "capable" mathematicians on the list is also indicative of the fact that, for the most part, American mathematicians were not trained in the physical and engineering sciences. American mathematicians lacked the pragmatism emblematic of American scientists.²⁶ Thus, despite mathematicians' offer to full cooperate with defense research, on the whole, their efforts were for the most part ignored.

Silvan S. Schweber has argued that the growth of American physics during the first decades of the twentieth century was marked by a practical outlook. Unlike their European counterparts, American theoretical physicists were often involved in experimental work. "This integration of theoreticians and experimentalists," according to Schweber, "molded the empirical, pragmatic, instrumentalist character of American theoretical physics."²⁷ The same could not be said for American mathematicians.²⁸ As numerous mathematicians noted at the outset of the war, a chasm yawned between pure and applied mathematicians in the United States. The pragmatist

²⁵ In 1942, Morse remarked in a letter to Evans that it was hard to find qualified mathematicians, saying: "after I sent out those bulletins at the beginning of the Committee's work, I was flooded with letters from hundreds of mathematicians, not more than 1 in 30 of whom was really competent to do anything that was required by the government." Marston Morse to Griffith Evans, 28 February 1942, AMSR, Box 28, Folder 25.

²⁶ John Servos has suggested that the relatively slow development of theoretical physics in the United States from 1880 to 1930 can be attributed in part to the tendency of American mathematicians during the period to focus on pure rather than applied mathematics. The mathematical training of American scientists was either lacking or did not match their needs. John W. Servos, "Mathematics and the Physical Sciences in America, 1880-1930," *Isis* 77, no. 4 (1986): 611–629.

²⁷ Silvan S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States 1920-1950," *Historical Studies in the Physical and Biological Sciences* 17, no. 1 (January 1986): 58.

²⁸ Loren Butler-Feffer has argued that the prevailing "ideology" of American mathematicians was "a privileging of pure over applied mathematics, of research over teaching, and of educating future mathematicians over training others who needed advanced mathematical skills." Loren Butler Feffer, "Mathematical Physics and the Planning of American Mathematics: Ideology and Institutions," *Historia Mathematica* 24, no. 1 (1997), 66–67.

view, which celebrated practice and particularity above generality and abstraction, did not apply to the development of mathematics in the first four decades of the century.²⁹

The tendency of American mathematicians to deal with generalities was singled early on as a potentially impeding the utility of mathematicians for defense work. Reflecting in 1940 on the need to train more applied mathematicians, Jacob Pieter Den Hartog, a Professor of Mechanical Engineering at Harvard, noted that “the excursions of professional mathematicians into ‘applied’ fields are apt to be considered as ‘applied’ only from their own subjective viewpoints, whereas practicing scientists who cannot use the results sometimes regard these contributions as ‘sterile’ rather than ‘applied’.”³⁰ Not only did American mathematicians lack the necessary background in the physical sciences and were uninterested in research arising out of particular problems. More fundamentally, their mode of inquiry did not match the sort of concrete scientific agendas demanded by war research.

These conceptions of American mathematicians as non-utilitarian and non-pragmatic divided the country’s scientists and mathematicians. One of the earliest issues in which this division was felt was that of deferment. In December 1940, a report regarding deferment of scientific personnel appeared in *Science*. The report, which was prepared by Frank Jewett, chair of the NAS, and Isaiah Bowman, listed six fields of science in which the “present and prospective

²⁹ In this respect, the development of American mathematics during the late nineteenth and early twentieth century followed what historians of mathematics have referred to as the “modernist conception” of mathematics, which Jeremy Gray defines as “as an autonomous body of ideas, having little or no outward reference, placing considerable emphasis on formal aspects of the work and maintaining a complicated — indeed, anxious — rather than a naïve relationship with the day-to-day world.” Jeremy Gray, *Plato’s Ghost: The Modernist Transformation of Mathematics* (Princeton, NJ: Princeton University Press, 2008). On mathematical modernism, see also: Herbert Mehtens, *Moderne Sprache Mathematik* (Suhrkamp, 1990); Herbert Mehtens, “Modernism Vs. Counter-Modernism, Nationalism Vs. Internationalism: Style and Politics in Mathematics 1900-1950,” in *L’Europe Mathématique: Histoires, Mythes, Identités*, ed. Cathérine Goldstein, Jeremy Gray, and Jim Ritter (Paris, France: Fondation Maison des Sciences de l’homme Imprimé en France, 1996); Leo Corry, “The Origins of Eternal Truth in Modern Mathematics: Hilbert to Bourbaki and Beyond,” *Science in Context* 10, no. 02 (1997): 253–296; Jeremy Gray, “Anxiety and Abstraction in Nineteenth-century Mathematics,” *Science in Context* 17, no. 1/2 (2004): 23–47; Jeremy Gray, “Modern Mathematics as A Cultural Phenomenon,” in *The Architecture of Modern Mathematics: Essays in History and Philosophy* (New York, NY: Oxford University Press, 2006), 371–397.

³⁰ Hartog, Den. “Observations on Training in Applied Mathematics,” 20 December 1941, MMP, Box 11, Folder “NRC 1941-1951.”

personnel situation in relation to the needs of the defense program clearly require careful consideration of requests for deferment.”³¹ Mathematics, notably, was not included among those selected fields. In January, John Walsh, chair of Harvard’s Department of Mathematics, wrote to Marston Morse to bring his attention to the published report. “The casual reader might come to the conclusion,” Walsh explained, “that mathematicians are not of significance in connection with the defense program.”³² Contending that it surely gave a “false impression” to the public, he urged Morse to bring the matter up for discussion with Jewett.

At the time, though, the War Preparedness Committee had not made an official recommendation regarding the deferment of mathematicians. Morse and Dean Richardson recommended to Jewett that applied mathematicians should be considered for occupational deferment on a case by case basis, but their recommendation did not apply to trained mathematicians in general.³³ Only towards the end of 1941 did mathematicians begin to realize that a shortage of mathematicians was inevitable. After Morse futilely attempted to contact those in charge of deferment, a committee was established in order to study the nature of supply of and demand for American mathematicians. The committee hoped that statistical data would provide the evidence necessary to include mathematicians under special deferment policies. The final report of the committee predicted a drastic shortage of mathematicians in the coming two years. Whereas the number of mathematics graduate students had decreased by 23% over the following

³¹ Frank B. Jewett and Isaiah Bowman, “Deferment From Military Service of Scientific Men,” *Science* 92, no. 2400 (December 27, 1940): 607–608.

³² John Walsh to Marston Morse, 2 January 1941, MMP, Box 15, Folder “W”.

³³ According to the Selective Service Act, occupational deferment for scientists did not ensure that a given physics graduate student would necessarily be granted deferment, as such decisions were made by each local board. Yet the National Committee on Physics was in charge of providing advice to each local board in light of national needs.

year this trend was matched by an increased demand for undergraduate mathematical instruction.³⁴

In January 1942, Morse wrote to mathematician Gustav Arnold Hedlund of the University of Virginia, reflecting on the question of military deferment for mathematicians. “The difficulty,” Morse worried, “is not the unanimity of opinion of responsible mathematicians on this subject, but rather that of converting those outside mathematics to our point of view. A little experience in such problems demonstrates that while outsiders, including engineers and outstanding physicists, are tolerant, they do not accept the thesis of mathematicians that their field is essential for the defense.”³⁵ For example, Morse noted, upon his return from a recent trip to England, Conant reported that British mathematicians, unlike their compatriots in physics, engineering, and chemistry, were not in high demand. The Selective Service Act, of course, did not guarantee deferment for physicists, but many local boards nonetheless made recommendations that these professionals be considered for deferment. “The principal difference” between mathematicians and physicists, Morse concluded, was “that the local boards and General Hershey are convinced that the physicists etc. are important — they act *as if* there was a ruling which is binding.”³⁶ Mathematicians, much to the chagrin of Morse, Stone, and their colleagues, were simply dispensable to defense work.³⁷

According to Silvan Schweber, when war mobilization began, American physicists were uniquely prepared to take part in research conducted in wartime laboratories. “One of the key

³⁴ The report was based on responses to a questionnaire received by 35 mathematics departments that had graduate programs (it was sent to 55 departments). The increased demand for undergraduate mathematical instruction was due to the enrollment of engineers and scientists in mathematics classes. In a letter to the American Council of Education and the National Roster of Scientific and Specialized Personnel, Morse noted, for example, that the enrollment in undergraduate mathematics courses at Harvard was at an all-time high, and in fact was unmatched by any other course of study at the College. Marston Morse to Dr. Francis Brown and Dr. Stewart Henderson Britt, 10 January 1942, AMSR, Box 28, Folder 24. Also AMSR, Box 15, Folder 53.

³⁵ Marston Morse to Gustav H. Arnold, 30 January 1942, AMSR, Box 28, Folder 24.

³⁶ *Ibid.*

³⁷ It was not until April 1943 that deferment policies for mathematicians changed, when the National Committee on Physics was renamed the National Committee on Physics and Mathematics.

factors” responsible for the success of this work, Schweber explains, was “the symbiotic relationship that had existed between theoreticians and experimentalists, their shared pragmatism, the ease with which they could communicate and collaborate.”³⁸ Mathematicians, many of whom did not share physicists’ pragmatism, did not easily find their place within the new organization of wartime research. Moreover, as the war dragged on and defense research mushroomed, a chasm divided professional mathematicians the new civilian scientific authority. Mathematicians, at least as represented by their professional organizations, considered themselves outside the country’s newly established scientific elite, and to a certain degree this sentiment persisted throughout the postwar period. More fundamentally, during the war it had real effect on the development of research in applied mathematics and the construction of the applied mathematician.

A year after Morse sent his letter to Bush offering the help of the War Preparedness Committee, members of the committee still felt that the committee maintained only weak ties with officials in charge of defense work, especially the growing scientific civilian authority as represented by the NDRC, and that this marginal position impeded their progress. Frustrated with this state of affairs, Stone sent a letter to Morse in August 1941. “I have no reason for believing that anything like the proper use of mathematical techniques exists or is contemplated.”³⁹ Stone, then the chair of the Subcommittee on Preparation for Research, noted his own difficulties in trying to encourage interest in applied mathematics: he organized lectures and symposia, and published texts on the subject, all to no avail. Fed up, Stone groused in a letter to Morse, “it is time for us to stop sitting quietly by while the physicists, chemists, and engineers monopolize the contributions to be made by the exact sciences under the Office of Scientific Research and

³⁸ Schweber, S. S. *The Empiricist Temper Regnant: Theoretical Physics in the United States 1920-1950* (1986), 92.

³⁹ Marshal Stone to Marston Morse, 6 August 1941, MMP, Box 13, Folder “Stone.”

Development. I am afraid that it may be necessarily for us to exert real pressure to accomplish any change in the existing situation and to injure our modesty in the process. Nevertheless, it is my conviction that we should go ahead and do so without delay.”⁴⁰ Something had to be done.

Morse agreed with Stone that they must argue more forcefully for mathematics’ usefulness in the war effort, and the two joined forces with Griffith C. Evans, the chairman of the Department of Mathematics at Berkeley, to present their case to the OSRD. Beginning in 1939, the three men successively served two-term presidencies of the AMS – first Evans, then Morse, and finally Stone. Evans had also received his PhD from Harvard in 1910. After spending two years in Europe, Evans took a position at Rice University. He taught there until 1933, when he was hired by the University of California, Berkeley to turn their Department of Mathematics into a leading center of mathematics research. Evans’s colleagues deemed his efforts extremely successful – during his tenure as chair, he attracted several leading mathematicians to the department and transformed mathematics at Berkeley into one of the top departments in the country. Evans’s research focused on developing and applying mathematical theories, from potential theory to mathematical economy. Together the three men represented a portrait of the prewar American mathematical elite.

By December, they had devised a plan. They would arrange a meeting with either Bush, Jewett, or Conant, in which they would appear in person to argue their case for utilizing mathematics. In addition, they would draft a memorandum enumerating their main points of contention, as well as making suggestions as to how to move forward. In a letter to Morse, Stone explained that the memorandum would establish three key points: first, that mathematics had proven itself useful during the Great War; second, that during the present war, mathematics had already proven useful to the British war effort; and third, that in order to make the most of

⁴⁰ Ibid.

American mathematical talent, the efforts of mathematicians must be organized under the supervision of a “qualified mathematician of recognized standing with adequate authority in the official research program.”⁴¹ Regardless of the outcome of any meeting, Stone reasoned, at least their memorandum would ensure that their cause was on record.

The resulting memorandum, “Mathematics in War,” opens by calling for “the full employment of the mathematical skill and intelligence available to the country,” which to date had been “inadequate and short-sighted.”⁴² However, before outlining their specific policy recommendations, the memorandum opens with a philosophical discussion of the nature of mathematical knowledge and a historical assessment of the relation of mathematics to the natural sciences.⁴³ Mathematics, the authors write, can “fairly be described as the precision-tool of the human mind in its rational operations,” and therefore applicable in “every problem presenting rational aspects of any great degree of complexity.” Mathematics, in their view, is a cognitive activity. It is a “tool” of the human mind, not a tool of the sciences, and is by definition removed from science. It is the structure, the generalized mode of ratiocination, on which science rests, but it is not itself a science.

This view of rationality and of scientific practice which privilege mathematical analysis often independent of physical investigation was in direct contradiction to the instrumentalist approach espoused by American scientists. It is possible to read the disagreement that ensued during the war between mathematicians and the scientific establishment as a struggle over power, and to a certain degree that was indeed the case. Yet the inability of the two communities to agree on how best to utilize mathematics and mathematicians in defense research was rooted at its core

⁴¹ Marshal Stone to Marston Morse, 22 December 1941, MMP, Box 13, Folder “Stone”

⁴² “Mathematics in War,” undated, MMP, Box 13, Folder “Stone.”

⁴³ Before they began drafting their memorandum, Stone convinced Gilbert Ames Bliss to join their committee. Bliss was a generation older than the three, and had served at Aberdeen Proving Ground in World War I. His past experience, Stone reasoned, could strengthen their case. Later they also invited Dunham Jackson onto their committee.

in the communities' adherences to two differing philosophical traditions. Mathematicians saw the tendency towards abstractness and theoretical investigation as a strength, a way of maximizing the potential of mathematical theories. In their background remarks the authors note, for example, that "it cannot be too strongly emphasized that...the scientific function of mathematics is a much broader one than that most often put to use by the physicists or engineer—namely, the ancillary function of providing convenient solutions and means of computation for particular problems *already formulated and delimited by him in terms of those branches of mathematics with which he happened to be more or less familiar.*"⁴⁴ That is, as far as the authors were concerned, mathematicians were desirable not for their ability to perform routine computations but rather for their generalized and theoretical insights. However, it was exactly this tendency that rendered them ineffective as far as the physicists were concerned.

Evans, Morse and Stone continued their memorandum by insisting that the only way to guarantee that mathematical knowledge was fully utilized, was to appoint a liaison between mathematicians and defense research. This intermediary would consult with members of the NDRC on their ongoing research projects, and would direct researchers to specific mathematicians who might help catalyze the mathematical aspects of their research. The memo's authors emphasized that such an individual must possess three important qualifications: 1) "he should have a broad knowledge of modern *pure* mathematics," 2) "he should be of recognized high professional standing," and 3) "he should have a wide acquaintance among professional mathematicians in the United States."⁴⁵ Both professional reputation and a knowledge of pure mathematics were necessary. The right person for the job must have authority over and an intimate knowledge of the mathematical community, as well as an extensive understanding of

⁴⁴ Ibid.

⁴⁵ Ibid.

mathematical theory; these qualities would allow him to effectively direct defense problems as they arise.⁴⁶ At heart, their recommendation implied a hierarchy of mathematical knowledge, one in which a synoptic view of generalized and generally applicable mathematical theories was of greater utility than a detail-oriented understanding of specific subspecialties of applied mathematics. Further, the authors wished to ensure that pure mathematicians remained the ultimate arbiters of the uses of mathematics.

Morse at first had misgivings about whether there should be one or several liaisons. In a letter to Stone, he explained, "I have debated whether the *one* mathematician might not preferably be replaced by three.... I have an opinion that the authority of three mathematicians coupled with their combined knowledge might suffice to combat the tremendous urge of *pseudo* applied mathematicians to select men of their own kind, as is being done at present."⁴⁷ The designation "pseudo," as here used by Morse, questions the genuineness of the "mathematician" rather than of the "applied mathematician." More bluntly, applied mathematicians, for Morse (and ostensibly his peers) were not *real* mathematicians, tout court. It was when it came to the development of applied mathematics that disagreement between the two groups came to the fore.

In February, Stone wrote a letter to Conant asking to set up a meeting with Morse, so that the two could present their memorandum and argue their case in person. On March 27, a meeting was scheduled in Washington with Conant, Bush, and Jewett representing the OSRD and Morse and Stone representing the mathematical community. The meeting lasted for an hour. Morse and Stone did not achieved exactly what they wished, but their plea was acknowledged. Instead of a one man liaison, it was agreed by all parties that an advisory committee of mathematicians would be appointed to serve as liaisons between mathematicians and the NDRC. It was agreed that

⁴⁶ That the authors believed that one individual could have intimate knowledge of the national mathematical community again demonstrates the relatively small size of American mathematics, especially its elite.

⁴⁷ Marston Morse to Marshall Stone, 7 January 1942, MMP, Box 13, Folder "Stone."

Jewett would nominate members of the committee and that one member would be chosen to serve as liaison officer in the NDRC office. The evening after their meeting, Jewett penned a letter to Morse asking him, as chairman of the section of mathematics in the NAS, to suggest possible nominees for the committee. Three days later, Jewett mailed a second letter.

“The more I think of it,” Jewett wrote, “the more I feel that if the Mathematical Committee is to render a full measure of assistance, it should be not only eminent but should be composed of both fundamental and applied mathematicians.”⁴⁸ In the three days that had passed, Jewett had carefully reviewed the memorandum Stone and Morse presented to him in the meeting. While reading it, he explained, “I had the impression that the authors were proposing a committee wholly of fundamental or ‘pure’ mathematicians. While I can hardly qualify as a mathematician in either category, I have seen enough of the problems both in the last and this war to feel that a committee which did not include some applied mathematicians would be less effective than one which did. As a matter of fact, I think it might lead rather directly to the organization of a group of applied mathematicians.”⁴⁹ Evans, Mores, and Stone’s “mathematics at war” did not convince Jewett of the superiority of pure mathematics. Eight months later, just such a group would be established under the auspices of Jewett and the OSRD.

As the leadership of the AMS was making its case to Jewett, Conant, and Bush, another proposal regarding the role of mathematicians in war research was making its way to the NDRC. This one was authored by Richard Courant. Just a few days before their meeting, Courant had sent a letter to Dr. R. W. King at the NDRC, submitting his own organizational scheme for mathematics. Courant’s proposal was radically different than that of Stone, et al. The problem he identified was how best to utilize “scientists in the fields of mathematical physics and applied

⁴⁸ Frank Jewett to Marston Morse, 30 March 1942, MMP, Box 9, Folder “Jewett.”

⁴⁹ Ibid.

mathematics,” *not* mathematicians writ large.⁵⁰ Courant suggested that a consulting service in applied mathematics and mathematical physics be established under the NDRC or OSRD, consisting of teams of consultants working together on several research projects. Courant’s proposal built upon a similar mathematics consultation service he was currently providing at New York University (NYU), which, he noted, was already overburden by requests. Given the limited number of qualified individuals, Courant reasoned, such a service should include an advanced training program as well.

Courant was among the German Jewish mathematicians who immigrated to the US from Germany during the 1930s.⁵¹ He arrived at NYU in 1934, fresh from the University of Göttingen, where he had established the Mathematics Institute in 1922. By the mid-1930s, Courant, who received his PhD in 1910 under the supervision of David Hilbert and had conducted research in mathematical physics, began devising plans for promoting applied mathematics in the US. During the 1940-1941 academic year, in collaboration with a young mathematician named Herbert Robbins, Courant began working on a popular book, which biographer Constance Reid claims he “conceived as a patriotic service.”⁵² In the introduction to the book, which he titled *What is Mathematics?*, Courant inveighs against “the great danger in the prevailing overemphasis on the deductive-postulational character of mathematics.”⁵³ He proclaims that the most important task facing mathematics in the future, was “to establish once again an organic union between pure and applied science and a sound balance between abstract generality and colorful

⁵⁰ Richard Courant to R. W. King, 23 March 1942, MMP, Box 3, Folder “Courant.”

⁵¹ Siegmund-Schultze, *Mathematicians Fleeing from Nazi Germany*; Nathan Reingold, “Refugee Mathematicians in the United States of America, 1933–1941: Reception and Reaction,” *Annals of Science* 38, no. 3 (1981): 313–338.

⁵² Constance Reid, *Courant* (New York: Springer-Verlag, 1996): 223.

⁵³ Richard Courant and Herbert Robbins, *What Is Mathematics?: An Elementary Approach to Ideas and Methods* (Oxford University Press, 1996), 1.

individuality.”⁵⁴ The mobilization of science during the war were the perfect conditions in which Courant could advance his doctrine for the growth of mathematics.

In his memorandum to the NDRC, Courant further suggested that groups, rather than individual mathematicians, would be the most effective way to address mathematical problems raised by defense projects. This was quite a different conception of mathematical research work than the one envisioned by leaders of the Society: what was needed, according to Courant, was not lone mathematicians but a team of mathematicians. Courant based his proposal on his experiences running his consultancy group at NYU. As he notes, “within the last few weeks we were asked for advice on problems concerning wave propagation, gun flash, strain in airwings, forces acting on dive bombers, sound insulation in battleships, Rayleigh-Ritz method, and other topics.”⁵⁵ These were real practical problems that needed solving, not Stone, Evans, and Morse’s theorizing about mathematics as a “precision tool of the human mind.”⁵⁶

Morse, however, was unimpressed by Courant’s proposal. In a letter to Jewett, Morse noted that the situation in which the NYU group found itself was in no way unique, but was replicated in other parts of the country. Only a national committee with federal support, he argued, would be qualified to address the problem of mathematics consultancy for war-related research.⁵⁷ Jewett must have been convinced of that fact by Morse, and a month later he was ready to appoint the NAS-NRC advisory committee on mathematics. The committee constituted ten members, with Evans, Morse, and Stone serving as its executive committee. Of the rest of the seven members, four either had training or conducted research in applied mathematics or

⁵⁴ Ibid.

⁵⁵ Richard Courant to R. W. King, 23 March 1942.

⁵⁶ Courant was a strong adherent of the Göttingen tradition in which he was educated. Throughout his career, he argued that it was crucial to attend to both the abstract and concrete aspects of mathematics, both in research and in training. If before the war, Courant advocated applied mathematical research, he later became concerned with the separation of applied from pure mathematics. Yet the changing funding and institutional regimes in the 1950s and 1960s produced exactly such a separation. There was no going back to Göttingen days.

⁵⁷ Marston Morse to Frank Jewett, 11 April 1942, MMP, Box 9, Folder “Jewett.”

mathematical physics (Beatman, Weaver, Robertson, and Barkley). The committee also included leading American mathematicians such as Veblen and Birkhoff, but in its final constitution it was far from resembling the sort of organization of leading pure mathematicians envisioned in their original memorandum. The committee's makeup, therefore, represented a compromise between mathematicians and the OSRD.

In practice, however, the NAS-NRC advisory committee on mathematics had only negligible influence on defense research. By August, Stone was convinced that despite the effort put into establishing the NAS-NRC advisory committee on mathematics, the problem of how to utilize mathematicians in the war remained unsolved. Most notably, liaison personnel between the committee and Conant's office had never been appointed. Stone was furious. In a letter to Morse, Stone announced that the time has arrived to pressure the NDRC and OSRD. Direct and forceful political action by mathematicians was necessary: "I am afraid that mathematicians, being logical and... patient people have not fully realized just such political maneuvering would be called for and have not, as a group, got themselves in the frame of mind where they are ready to master their considerable political strength to secure a place which is theirs by the internal logic of nature."⁵⁸ Stone was certain, however, that rising frustration among many of his fellow mathematicians would make political action possible. According to Stone, the naturally peaceful nature of mathematicians had so far prevented them from taking their "rightful" place with the rest of the sciences, not because they were not qualified for defense work, but because they were not pushy enough to claim their territory. Stone took it on himself as the president of the AMS to advocate for American mathematicians.

Stone resolved to exert more pressure on specific individuals in the NDRC while simultaneously starting a publicity campaign to establish the importance of mathematics to the

⁵⁸ Marshall Stone to Marston Morse, 22 August 1942, MMP, Box 13, Folder "Stone."

war. For Stone, those scientists in positions of political power had failed to realize the importance of fully utilizing the country's mathematical resources in wartime.⁵⁹ The disjuncture between the nation's scientific authority and the established mathematical elite would grow even wider in the months to come: in November, following the reorganization of the OSRD, Bush established the Applied Mathematics Panel and placed Warren Weaver as its chair. An engineer by training, Weaver had been directing the fire control section of the OSRD during the past three years. He was well recognized among professional mathematicians, but his main research areas were in applied, not pure, mathematics.

It is hard not to see the foundation of the Panel as a direct insult to the leaders of the mathematical profession who had been working tirelessly since 1939 to ensure that mathematicians were being put to work on defense problems. In a letter to Weaver asking him to take over the activities of the Panel, Karl Compton noted that the "the committee headed by Marston Morse has not been effective; I think this is because it is too 'pure'. Apparently what is needed is to bring mathematics and the various instrument problems together through the intermediate ground of Applied Mathematics."⁶⁰ Despite the tireless efforts of the mathematical elite of the country, the scientific establishment did not see right to place a pure mathematician to chair the new organization. The affront was right there in the panel's title, which was notably not named the Mathematics Panel.

Weaver's ascension directly contradicted the opinions put forward by Evans, Morse, and Stone in their memorandum. In "Mathematics in War," they not only advocated that a pure mathematician be appointed as a liaison between the mathematical community and the defense

⁵⁹ Stone exclaimed, "Unfortunately, there is no sign that the men at or near the top have either individually or as a group a comprehension of total war which can be translated smoothly into detailed action." Ibid.

⁶⁰ Quoted in Kathleen Broome Williams, *Improbable Warriors: Women Scientists and the U.S. Navy in World War II* (Naval Institute Press, 2001), 168.

establishment, but they also explicitly argued that the job should not be given to an applied mathematician. “In view of the existing incompletely developed state of applied mathematics in the United States and in view of the lack of close contact between pure and applied mathematics,” they wrote, “it would be a *serious mistake* to demand the further and otherwise natural qualification of extensive experience in some field of applied mathematics.”⁶¹ The exact same qualifications that had made the leader of the OSRD choose Weaver to head the newly established AMP made him utterly unsuited for the job, according to the leaders of the AMS. The establishment of the AMP and the appointment of Weaver epitomized the irreconcilable philosophies of the mathematical leadership of the country and the scientific defense establishment.

Considering the well acknowledged paucity of research in applied mathematics in the United States prior to the war, the emergence of the Applied Mathematics Panel as the first national organization dedicated to research in the field under the auspice of the scientific community and in direct rejection of mathematics profession placed applied mathematicians in especially difficult position. In the war’s aftermath, applied mathematicians struggled to find their place back within the mathematical community, but it was not quite clear how they fit in.

Weaver vs. Stone

The Applied Mathematics Panel was envisioned as a service organization within the NDRC, but Warren Weaver questioned its relation to the NAS-NRC advisory committee on mathematics from the start.⁶² In a letter to Compton on November 12, Weaver inquired into the intended

⁶¹ “Mathematics in War,” undated, MMP, Box 13, Folder “Stone.”

⁶² Over its years of operation the Applied Mathematics Panel employed close to 300 people and supported research in statistics, numerical analysis, computation, the theory of shock waves, and operation research. Among the various groups which the panel contracted was the Brown University group in applied mechanics, Jerzy Neyman’s Statistical

functions and organization of the panel. Among his questions about the proposed membership of the panel and other bureaucratic details, Weaver asked: “Will the previous National Academy-NRC Committee on Mathematics continue to exist, or have they agreed to evaporate? If it is to continue, what is the interrelation?”⁶³ Weaver was himself a member of the NAS-NRC committee, but the creation of the AMP created friction between him and the leaders of the profession. In November 1943, the enmity boiled over with the publication of George Gray’s *Science at War*.⁶⁴ A *TIME* article reviewing Gray’s book quoted Gray openly criticizing American mathematicians for being overly “pure.”⁶⁵ The review further proclaimed that a lack of qualified mathematicians in the US damaged military mobilization. Needless to say, the mathematicians who had set up the War Preparedness Committee in 1939 were enraged.

Marston Morse sent a letter to the editors of *TIME* asserting that the problem was not the lack of “top flight” mathematicians, but the “failure of the civilian authorities” to marshal mathematicians for the war effort. Only in the past year had civilian authorities in charge of war research begun calling upon mathematicians. If only they had done so earlier, Morse noted, mathematicians would have participated in the War earlier and in greater numbers.⁶⁶ Courant, whose name was given in the *TIME* article as an example of the sort of mathematician the United States needed more of (pointing to his German roots), also submitted a letter to the magazine’s editors. Courant went out of his way to praise American mathematicians, noting that despite “the lack of specific training in applied sciences, many of them have been highly successful as war

Laboratory at the University of California at Berkeley, Courant’s group at New York University, a group at Princeton University under the directorship of Samuel Wilks, a statistical group at Columbia University, and the Mathematical Tables Project at the National Bureau of Standards. See: Rees, “The Mathematical Sciences and World War II”; W. Allen Wallis, “The Statistical Research Group, 1942–1945,” *Journal of the American Statistical Association* 75, no. 370 (1980): 320–330; Larry Owens, “Mathematics at War: Warren Weaver and the Applied Mathematics Panel 1942–1945,” in *The History of Modern Mathematics: Institutions and Applications*, by David E. Rowe and John McCleary (Academic Press, 1988), 286–305.

⁶³ Warren Weaver to Karl Compton, 12 November 1942, RAMP, Box 17, Folder “Inception of AMP.”

⁶⁴ George W. Gray, *Science at War* (Books for Libraries Press, 1943).

⁶⁵ “Progress Report,” *TIME*, November 29, 1943.

⁶⁶ Morse Marston, “On Mathematicians,” *TIME* 42, no. 25 (December 20, 1943): 6.

research workers.”⁶⁷ Yet the editors chose to ignore his letter, and instead published Morse’s response.

Courant, however, made sure to send copies of his letter to MacLane, Veblen, Moulton, and Morse. Courant had not only been mentioned by name in *TIME*, but he was also working closely with the civilian scientific authority Morse had criticized in his reply. In his letter to MacLane, Courant went as far as claiming that he was “disgusted” to find his name mentioned in the article.⁶⁸ Earlier in the week, Courant learned from Weaver that MacLane has criticized him as the Chief of the AMP in connection to the *TIME* article. Courant, in addition to informing MacLane of his own reply to the article, was hoping to draw attention to Weaver’s “sensitiveness in the matter.”⁶⁹ MacLane might have taken notice of Courant’s advice and did not bring up the topic with Weaver, but in the meantime Stone used the opportunity to begin a months’ long exchange with Weaver.⁷⁰

The correspondence between the two men began shortly after they had met at the annual conference of the AMS in Chicago, and followed a brief exchange the two men had had. Stone began his letter by lamenting the fact that he did not find enough time during the conference to tell Weaver his “little story.” Noting the *TIME* review, Stone wished to convey to Weaver that the relatively minimal involvement so far of the mathematical profession in the war effort was in no way due to lack of trying. The problem was not with mathematicians but with the leadership of the OSRD. For Stone this was evident by the fact that the NAS-NRC committee on

⁶⁷ Richard Courant, “Letter to the Editors,” RCP, Box 23(old), Folder “TIME 1943.”

⁶⁸ Richard Courant to Saunders MacLane, 8 December 1943, RCP, Box 23 (old), Folder “TIME 1943.”

⁶⁹ Ibid.

⁷⁰ In “Mathematics at War: Warren Weaver and the Applied Mathematics Panel 1942-1945,” Larry Owens analyzes parts of this correspondence in order to illuminate some of the difficulties and failures endured by Weaver and the AMP. Whereas Owens focuses on the work of the panel and Weaver, I wish to draw attention to the established mathematical community, and how the difficulties founding the panel not only reflected, but also continued to affect, the development of applied mathematics in the US. Specifically, I wish to draw attention to the way in which the personae of the applied and pure mathematician were constructed in opposition to one another. Owens, “Mathematics at War: Warren Weaver and the Applied Mathematics Panel 1942-1945.”

mathematics that was established after the conference with Bush, Conant, and Jewett, was a committee by name only. “It is necessary to add,” he concluded, “that Morse and I have never been able to interpret this conclusion of the conference as anything more than a moderately tactful ‘brush off.’” Whereas Conant held that the advisory committee’s ineptness was due to it being “too pure,” Stone believed that it never had a chance to succeed and was doomed from the start. Stone maintained that due to his unique position in the OSRD, Weaver now had a “special responsibility” to try and amend the “very unfortunate situation created by the lack of understanding between some of the leading mathematicians of the country and OSRD.”⁷¹ Stone did not specify what Weaver might do to ease this strained relation, but the letter makes it clear that he considered it to be a pressing matter.

Weaver, however, was not so quick to concede. On December 6, he wrote Stone a six-page reply, in which he rehearsed the history of the panel’s establishment from his perspective. Possibly referring to Stone’s “special responsibility,” Weaver noted that he was not eager to assume the chairmanship of the panel and in fact attempted to forsake the job on numerous occasions. He did not share Stone’s worry that the role of mathematicians in the war has not been appreciated. Yet he was well aware of the panel’s unique position between the scientific establishment and the mathematics profession. “I am quite aware,” Weaver wrote, that “those who know the constitution of the Executive Committee of AMP criticize us (or me) because there is not stronger representation of the *profession*.”⁷² This was in many way was the crux of the dispute that emerged between Stone and Weaver. It was never quite clear who the panel represented. The panel was in charge of directing the various mathematical studies related to the

⁷¹ Marshall Stone to Warren Weaver, 28 November 1943, AMP, Box 15, Folder “Correspondence 1944.”

⁷² Warren Weaver to Marshall Stone, 12 May 1943, RAMP, Box 15, Folder “Correspondence 1944.” Emphasis added.

military effort, but when mathematicians looked at the constitution of its leadership they did not recognize its members as representatives of their profession.

The original membership of the executive committee included Richard Courant, Samuel Wilks, Thornton Fry, and Elton J. Moulton. Courant, as Weaver clearly pointed in his letter, was left out of these criticisms. As was Wilks for the most part, especially “by those who know the role mathematical statistics plays in our activities.”⁷³ Fry, on the other hand, was an object of derision. Like Weaver, Fry received his PhD from the University of Wisconsin, after which he obtained an industrial position first at the Western Electric Company and later at Bell Labs. At the time such a professional trajectory for a mathematician was almost completely unheard of. He was not a member of the mathematics elite. Weaver nonetheless defended the inclusion of Fry in the executive committee, praising him for his administrative experience. He also used the opportunity to define more broadly the characteristics he deemed necessary in a member of the panel’s executive committee. A good committee member, according to Weaver, needed to be familiar with the NDRC, the military organizations, and the technological and scientific aspects of modern warfare. Just as importantly he had to be a “cooperative,” “tolerant,” and “unselfish” individual. Anticipating Stone’s criticism, Weaver offered that these qualifications “may lead you to wonder whether it is very directly concerned with mathematical ideas; and your wonder is justified.” However, given the administrative nature of the work, Weaver explained, the executive committee was not too consumed with mathematics.

Such a conception of the ideal technocratic mathematician was directly opposed to the popular image of the pure mathematician. In fact, as early as the summer of 1941, Fry published an article in *The American Mathematical Monthly* warning against the dearth of industrial

⁷³ The fact that Weaver had to indicate that the inclusion of Wilks, a statistician, was not questioned by those who were aware of the important role statistics played in defense research is indicative of the fact that statistics in the prewar era was considered somewhat separately from mathematics. Ibid.

mathematicians in the United States. In the article, which begins by posing the question “what is a mathematician?” Fry makes a strong contrast between the research mathematician and the sort of mathematician required for industrial work. The “typical mathematician,” he writes, “is a dreamer, not much interested in things or the dollars they can be sold for. He is a perfectionist, unwilling to compromise; idealizes to the point of impracticality; is so concerned with the broad horizon that he cannot keep his eyes on the ball.”⁷⁴ Weaver himself shared this sentiment to a certain degree. In his letter to Stone, Weaver complained about the difficulty of attracting qualified personnel to work on the panel’s contracts. The men that are needed, he explained, must be unselfish and capable of collaborating with other individuals. Yet these conditions, Weaver proclaimed, “exclude a good many mathematicians, – the dreamy moon-children, the prima donnas, the a-social genius.”⁷⁵ What becomes clear is that ideological differences between scientists and mathematicians were mapped onto their respective persona. Mathematicians’ tendency to pursue theoretical abstraction made them unsuitable for war research not just intellectually but also temperamentally.

Despite his unsympathetic view of some mathematicians, Weaver was eager to gain both support and recognition from the greater mathematical community. The Applied Mathematics Panel was established without the input of the mathematical profession, but its technical staff was composed of mathematicians and its main objective was to produce analytic studies in support of the other divisions in the OSRD. A few days before he sent his letter to Stone, Weaver wrote to Ward Davidson, a staff member of the NDRC, requesting permission to circulate a notice among chairs of mathematics departments, informing them about the panel and its work. The need for circulating the notice, Weaver explained to Ward, became clear to him when he attended an AMS

⁷⁴ Thornton C. Fry, “Industrial Mathematics,” *American Mathematical Monthly* (1941), 4.

⁷⁵ Warren Weaver to Marshall Stone, 6 December 1943, RAMP, Box 15, Folder “Correspondence 1944.”

meeting in Chicago the previous weekend. There, he was surprised to discover that “the mathematical profession, taken as a whole, is almost completely ignorant of our activities or even our existence.” He added that “it was something of a shock to me to have a number of my old mathematical friends casually and blithely inquire, ‘well what are you doing with yourself these days?’”⁷⁶ Most American mathematicians were unaware, so it seems, of the workings of the panel.

In the notice that he drafted for distribution among mathematics departments, Weaver briefly described the organization of the panel and called upon department chairs to forward information about mathematicians who might be interested in helping the panel’s work in one capacity or another. His notice, however, also included a description of the kind of individual suitable for the work. “Top-notch persons” need not necessarily apply, given that such individuals tended to find work on their own. Rather, the panel was in desperate need of qualified individuals “who may admittedly not be geniuses,” but who are nonetheless energetic, unselfish, and who work well with others.⁷⁷ Once more, the main qualification, Weaver emphasized, was not technical ability *per se*, but rather interpersonal skills.⁷⁸ What was needed was not only a new kind of mathematics, but also a new kind of mathematician.⁷⁹ In the immediate aftermath of the war and up until the 1960s, how exactly this new type of mathematician should be constructed remained a source of contention.

⁷⁶ Warren Weaver to Ward Davidson, 01 December 1943, RAMP, Box 15, Folder “Correspondence 12/42 - 12/43.”

⁷⁷ Warren Weaver, “A Statement Concerning the Applied Mathematics Panel of the National Defense Research Committee, 10 December 1943, RAMP, Box 15, Folder “Correspondence 12/42 - 12/43.”

⁷⁸ Weaver’s idea of the kind of mathematician that was required for war research was, of course, in direct contradiction to the list of “abler” and “important” mathematicians Morse composed a couple of years earlier.

⁷⁹ Through most of the 1950s, applied mathematicians often complained about the implicit hierarchy that dominated the development of the field and which placed pure mathematics directly above applied mathematics. In calling for individuals “who may admittedly not be geniuses,” Weaver could be seen as unintentionally helping to propagate this hierarchy. This hierarchy, of course, existed prior to the war, but the emergence of the applied mathematician during the war as a “worker” did not help dissipate with that assumption.

The goal of circulating the memorandum, however, was not just to inform the mathematical community of the panel's existence, but also to gain its support. Courant, who suggested Weaver circulate such a notice, stated this objective clearly when he reasoned that it might help "eliminate the danger of the Panel's becoming suspected of developing into a clique."⁸⁰ Saunders MacLane, who headed one of the AMP's contracts and was a well respected member of the mathematical elite of the country, attempted to influence Weaver as well. "I am still very keenly interested," he wrote to Weaver, "in the question of an additional highly able, pure mathematician in an appropriate high position in AMP. I shall continue to press for some step in this direction."⁸¹ Thus, from early on the panel found itself in the uncomfortable position between the scientific establishment and the mathematics profession. The "clique" that Courant was worried about overturned everything mathematicians knew and valued in American mathematics. Calls for fostering applied mathematics had been present prior to the war, but these were made from within the community, not prompted from outside it. The changing relations between the federal government, the military, and scientists had created overnight a new mathematical elite, and many in the mathematical community were surprised to realize that its membership did not match their expectations.

To further ease some of the tensions between the panel and the established mathematical community, Weaver suggested forming an advisory council consisting of men with "outstanding mathematical talent." He also sought to add Morse and Evans to the panel's executive committee. Stone was unconvinced. On December 14, he replied to Weaver's long letter voicing once more his dissatisfaction with the mathematicians' role in defense research.⁸² A year had passed since the panel was officially established but, according to Stone, there was not much progress to show

⁸⁰ Richard Courant to Warren Weaver, 09 November 1943, RAMP, Box 6 Folder "Courant."

⁸¹ Saunders MacLane to Warren Weaver, 30 October 1943, RAMP, Box 7.

⁸² Marshall Stone to Warren Weaver, 14 December 1943, RAMP, Box 15, Folder "Correspondence 1944."

for it. Despite Weaver's long explanation, Stone maintained that what disturbed him was that the "more skillful and experienced mathematicians of the country" represented only a small proportion of the panel's personnel. He doubted that an advisory board would do much to amend the situation. Having more recognized mathematicians on the panel might improve "morale and spirit," Stone wrote, but it would not deal with the main problem – namely the need for employing more qualified individuals. But more was at stake, Stone warned. In fact, the future of the field was in danger. To underline this point, Stone returned once more to *TIME* magazine's review of Gray's *Science at War*. As Stone reported to Weaver, the article was met by "dismay, alarm, annoyance or fury" by many of his acquaintances.⁸³

Stone's remarks had two objectives. On a personal level, he urged Weaver, who had been cited as one of the sources Gray used in his book, to distance himself from the views expressed in the book. "I am quite sure," he remarked, "that many of your personal friends in the field of mathematics would be very grateful for an expression of your ideas."⁸⁴ On a professional level, Stone felt that such a negative portrayal of mathematicians might have unforeseeable effects on the growth of the field. Considering the increased "federal-political control" of the educational and intellectual life of the country, Stone explained, scholarship might be increasingly marked by "shortsightedness" and an increased emphasis on "immediate social utility." Such an intellectual framework, he reasoned, could be detrimental to mathematical research in the country. It would be a mistake to ascribe to the view that puts "applications ahead of the things which are going to be applied."⁸⁵ Already during the war Stone began to worry that a utilitarian ideology would begin to define the development of the field.

⁸³ Ibid.

⁸⁴ Ibid.

⁸⁵ Ibid.

Weaver countered two weeks later with another long reply. He defended the panel's activity, noting that when dealing with bureaucracy, it was necessary to make some compromises when it came to personnel. In addition, he pointed out the names of various high-ranking mathematicians who were working for the panel, dividing them according to their affiliation with either pure or applied mathematics, as well as statistics. Yet the bulk of the letter was devoted to "a few remarks about pure and applied mathematics!"⁸⁶ Weaver began by suggesting that there were two schools of thought when it came to the constitution of the field: on the one hand, there were those who believed that mathematics is indivisible and that it therefore does not make sense to talk about different types of mathematicians. On the other hand, there were those who, like him, believed that there were two kinds of mathematicians, namely pure and applied, but that the former could in principle be converted into the latter. What Weaver understood was that embedded in these debates was a theory of knowledge, and an understanding of what mathematics is as a field of study. This murkiness in the underlying definition of the field would continue to spur debates about the professional identity of the mathematician in the next two decades.

Weaver conceded that there was some truth in each view. Yes, he analogized, one could talk about mathematicians similarly to the way one talks about musicians. Yet a pianist cannot necessarily play the cello or vice versa. He also agreed that some mathematicians whose interests were "very abstract and pure" could successfully turn to more applied areas, but the emphasis for Weaver was on the word "some" as opposed to all. Furthermore, even those mathematicians who did turn their attention to applied topics were often met with criticism by the "users." As an example, Weaver paraphrased for Stone what he saw as a common reaction of military personnel to work done by pure mathematicians:

⁸⁶ Warren Weaver to Marshall Stone, 29 December 1943, RAMP, Box 15, Folder "Correspondence 1944.

I have no doubt that your friend Professor X is a most distinguished and able pure mathematician. But for the love of God, keep him wrapped in cotton until the war is over! His sense of the practical, his controlling background of experience, his knowledge of air, water, earth and hardware seem all to be entirely lacking. However brilliant, imaginative, and profound he may be, he nevertheless just plain doesn't know a whole involved and ordered set of disciplines which happen to be necessary for my job.⁸⁷

Pure mathematicians were just not suitable for the job at hand. Both their prior knowledge and their persona prevented them from taking part in any meaningful way in defense research.

Weaver wanted to convince Stone of the fact that there *were* two types of mathematicians and that the two were *distinct*. A set of binaries distinguished the two: individualistic vs. cooperative, self-motivated vs. team worker, a thinker vs. a doer, imaginative vs. pedestrian, arrogant vs. humble. What becomes clear is that the applied mathematician was constructed not only in terms of his differences from the pure mathematician, but also in light of the newly established civilian scientific authority. The applied mathematician, by Weaver's view, became the prime example of the new American scientist, whose work was more a matter of national duty than of vocation. The new institutional reconfiguration of the American scientific elite during the war required fashioning a new kind of mathematician.⁸⁸

Still, Weaver was aware that his two-part definition dealt more with the distinctions between pure and applied mathematicians as "two sorts of people," than with the differences

⁸⁷ Ibid.

⁸⁸ Military mobilization during World War II not only transformed the organizational structure of scientific knowledge production in the United States but also the figure of the American scientist. As Steven Shapin shows, the transformation of science from a vocation to a job preceded the rise of Big Science and the military-industrial complex. Yet, in the postwar era, supporters of large-scale organized research believed in the "moral equivalence" of scientists. The new organization man, Shapin writes, became the symbol of postwar science. The virtues, the personalities, and the social standing of scientists were all remodeled during the war and its aftermath. As will become clear in the next chapters, by separating applied from pure, mathematicians were more resistant to World War II's effects on American science. Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation: A Moral History of a Late Modern Vocation* (Chicago, IL: University of Chicago Press, 2009).

between two branches of mathematical research. What separated the applied from the pure mathematician was also the sort of training he received. He noted that sound training for an applied mathematician must include courses in various fields including hydrodynamics, elasticity, statistical mechanics, and thermodynamics, among others. This sort of training program, Weaver was quick to point out, was not common in most American universities. Yet he himself had received such an education, grounded not only in contemporary mathematical theory but also in physics and engineering. “Is this heresy? Is this unreasonable? Is this anti-mathematical?” he asked quizzically.⁸⁹ The relation between the persona of the applied mathematician and his technical training remained vague. Was it the sort of training that had dominated the American scene over the last four decades that had fashioned the individualistic and self-centered pure mathematician? Or were imaginative and independent thinkers drawn to mathematics and, by their natural dispositions, determine the constitution of the field?

Stone replied a couple of weeks later. He disagreed with Weaver that the “prima donnas” do not belong in the defense establishment, arguing that a diverse organization should be able to accommodate a diverse set of personalities. He also chided Weaver, pointing out that if he wished to include more young and talented mathematicians in the panel’s work, he would need “the suggestions of abstract mathematicians who have trained them.”⁹⁰ As for the relation between pure and applied research, Stone’s opinion was somewhat different from Weaver’s.

Mathematicians were not technicians. If the problem one faced was clearly formulated, then a technician should be able to do the job. However, more often than not, this was not the case. “It is in this situation,” Stone explained, “that quality of mind rather than specific training is the key to

⁸⁹ Warren Weaver to Marshall Stone, 29 December 1943.

⁹⁰ Marshall Stone to Warren Weaver, 14 January 1944, RAMP, Box 15, Folder “Correspondence 1944.”

progress.”⁹¹ Further, relying solely on technical knowledge can be dangerous, as it might obstruct the researcher’s view from finding novel solutions. Mathematics, for Weaver, was instrumental, a tool that someone with proper training might apply to numerous situations. This was not so for Stone, who conceived of mathematics as a mode of thinking, a cognitive activity whose strength arose not from its particularity but its generality.

The correspondence between Stone and Weaver continued for another month. Weaver seemed eager to convince Stone of his perspective. He described in great detail the history of one of the panel’s projects, which Stone had previously criticized. Pointing out the complex relation between the panel, the Army, and Navy, Weaver highlighted for Stone the way in which the mathematical results achieved by the panel were formulated within a specific organization and in light of changing demands.⁹² Moreover, Weaver sent Stone a classified list of all the panel’s projects in hope that upon studying them more carefully, Stone would become more aware of the sort of work the panel did, and therefore become less critical of its work. Stone, however, was not convinced. By January, two months after they had begun their correspondence, Stone wrote to Weaver to say that he saw no reason for them to continue their conversation. Not only had they not reached agreement, but Stone saw the difference in their respective viewpoints as “so fundamental and so profound” that no good could come from further exchange.⁹³

⁹¹ Ibid.

⁹² Warren Weaver to Marshall Stone, 19 January 1944, RAMP, Box 15, Folder “Correspondence 1944.”

⁹³ In August 1944, Weaver learned that Stone intended to criticize publically the work of the panel in the upcoming annual meeting of the AMS. In response, Weaver sent a letter to many of the mathematicians and scientists who were involved in the organization of the panel (the recipients included Bush, Garret and George Birkhoff, Conant, Ward Davidson, Fry, MacLane, Morse, Richardson, Veblen, von Neumann, and Wilks) defending some of the criticisms he anticipated. Weaver also mentioned the long correspondence he and Stone were engaged in and quoted Stone’s assertion that their difference in opinion was too insurmountable for him to wish to continue. When Stone heard about this letter, he immediately wrote Weaver protesting that his words were quoted out of context. He asked Weaver to circulate copies of the entire correspondence (approximately 40 pages) to all the recipients of his letter. Around October, copies were sent to all of above names. Weaver and Stone’s correspondence, therefore, eventually became a public document among members of the established mathematical community. Unfortunately, I was unable to find any reactions to this exchange of letters and it is doubtful that any of the above mathematicians chose to

Stone's somewhat abrupt termination of their exchange might seem slightly dramatic, but in a sense he was right. He and Weaver did subscribe to different philosophies. Their objectives were similar as both men wanted to ensure that mathematicians lend their fullest support to the war effort. However, they could not agree on how this goal could best be accomplished. That applied mathematics in the United States arose in between a pragmatic and an idealistic conception of science had real effect on the postwar development of the field. Not only the contours and boundaries of applied mathematics were called into question, but also its objectives. As long as the war continued, research in field developed according to military needs, but at the end of the war it was not obvious how research should continue. Nor was it clear what was the relation between applied mathematicians and the mathematics profession writ large.

What is Applied Mathematics?

In June 1946, John Tukey, a topologist turned statistician, sent a letter to John R. Kline, the Secretary of the American Mathematical Society (AMS), warning him of as an impending "crisis" in applied mathematics.⁹⁴ Attached to the letter was a statement Tukey had circulated among various mathematicians during the preceding weeks, calling upon the council of the AMS to discuss the status of applied mathematics in its upcoming summer meeting at Ithaca. In the statement, which garnered the signatures of such leading mathematicians as Oscar Veblen, Marston Morse, and Saunders MacLane, he proposed that a separate division dedicated to applied mathematics be established within the AMS. As Tukey explained in his letter to Kline, it was only a matter of time before someone established an organization devoted to applied mathematics, and now was the time to ensure that it would be housed within the AMS, "where it

weigh in in writing on such a hotly contested matter. See: Warren Weaver to Marshall Stone, 12 August 1944 and Marshall Stone to Warren Weaver, 21 September 1941, RAMP, Box 15, Folder "Correspondence 1944."

⁹⁴ John Tukey to John R. Kline, 25 June 1946, AMSR, Box 32, Folder 23.

can strengthen and be strengthened by pure mathematics.”⁹⁵ Tukey’s proposal quickly gained the attention of many leading mathematicians, and while most did not agree with Tukey’s suggestion that applied mathematics should be a separate division, they commended him for prompting an open and lively conversation on the subject.

Even before the council met in August, members of the Society wrote to both Tukey and Kline, voicing their reactions to the proposal. Richard Courant warned Tukey against what he termed “segregating” applied mathematicians; he suggested advancing a broad, rather than a narrow, view of mathematics. What the war made clear, Courant wrote, was that the country needed more mathematicians with both theoretical training and practical knowledge of physics, mechanics, or other relevant fields.⁹⁶ Kline saw the issue altogether differently. In a letter to Saunders MacLane, Kline explained, “I feel that the war demonstrated the fact that the pure mathematician can contribute to at least the same degree as the applied mathematician and that the fundamental problems are the same. Look at the contributions of yourself, Morse, Evans, Whitney, Garrett, et cetera, all of whom are surely the *purest of the pure mathematicians*. Why make a separation which is unnatural and deprive various groups with somewhat divergent tendencies of the benefit of the ideas and results of the others?”⁹⁷ For Kline, the war established not that applied mathematics must be more actively pursued, but rather that pure mathematics already provides sufficient background for applied mathematical work.

Dean Richardson, whose work was fundamental to the establishment of the Applied Mathematics Group at Brown University, lauded Tukey for raising the issue, but resisted the proposed establishment of a separate division. “I don’t like to hear some people in pure mathematics speak disparagingly of the applications, or persons in applied mathematics scorn

⁹⁵ Ibid.

⁹⁶ Richard Courant to John Tukey, 16 August 1946, AMSR, Box 31, Folder 39.

⁹⁷ John R. Kline to Saunders MacLane, 20 June 1946, AMSR, Box 31, Folder 83.

those working in pure theory. It sounds so foolish.”⁹⁸ If only pure and applied mathematicians were more familiar with one another’s work, Richardson reasoned, American mathematics would benefit greatly. Members of the Seminar in Applied Mathematics at the University of Michigan also weighed in on the issue. In an official resolution they sent to Kline prior to the council’s meeting, they concluded that a new division should not be established. Instead, they recommended that applied mathematicians be better represented within the Society.⁹⁹ The volume and vehemence of the responses Tukey received reveal the instability and uncertainty American mathematicians faced at the end of World War II. Applied mathematics, a field almost completely neglected only a few years earlier, now demanded mathematicians’ attention, and the AMS, as the main professional organization of research mathematicians in the US, was confused as to how to make room for applied mathematics.

After receiving bachelor’s and master’s degrees in chemistry, John Tukey began graduate work in mathematics at Princeton University in 1937. His transition into the field was smooth. He completed his preliminary exams with flying colors, and at the end of his first year he received the prestigious Jacobus Fellowship, the top fellowship in the graduate school at the time. He submitted his doctoral thesis in 1939, and soon after was hired to be the Fine Instructor in the Department of Mathematics at Princeton.¹⁰⁰ By 1941, he was promoted to assistant professor, but his academic career took a surprising turn when he joined the Fire Control Research Group at Princeton in May of that year.¹⁰¹ Tukey remained part of the group, which was headed by mathematician Merrill M. Flood and supported in part by the AMP, until 1945. By the time he

⁹⁸ Dean Richardson to John Tukey, 12 July 1946, AMSR, Box 31, Folder 115.

⁹⁹ Raul Churchill to John Kline, 8 August 1946, AMSR, Box 31, Folder 39.

¹⁰⁰ John Tukey Interview with Albert Tucker, Oral History PMC41, 11 April 1984, The Princeton Mathematics Community in the 1930s. Oral History Project. Accessed online at: http://www.princeton.edu/~mudd/finding_aids/mathoral/pmc41.htm

¹⁰¹ The Fire Control Research Group was studying, for example, the most efficient way to fire a machine gun from an Air Force plane.

completed his work, his interests had shifted radically: he had moved from research in topology to statistics. He joined Bell Laboratories in New Jersey in 1945 with a half-time position, and soon began to divide his time between the laboratory and Princeton's Department of Mathematics.

In 1946, when he circulated his statement on applied mathematics among members of the AMS, Tukey's still rising career represented the possibility of a new type of mathematician, one who possesses a strong background in pure mathematics but whose interests span a variety of applications. If Stone and Weaver were the archetypes of the professional pure and applied mathematicians of the prewar era, Tukey was emblematic of a new professional identity that would not have been possible prior to World War II. His proposal questioned the "proper" place of applied mathematics within the AMS, questioning whether the field should be subsumed by the AMS or instead be represented by a separate organization. While it was certainly an institutional recommendation, his proposal, must be read biographically: it reflected Tukey's personal experiences and concerns as a mathematician whose career, both institutionally and intellectually, spanned the worlds of pure and applied mathematics.

When the council met in Ithaca to discuss Tukey's proposal, its members nearly unanimously felt that a separate division should not be established. Still, they also recognized that the Society's work was not successfully reaching applied mathematicians. Something needed to be done to amend the situation. Members therefore authorized the President of the Society, Theophil Henry Hildebrandt, to appoint a Special Committee on Applied Mathematics to investigate whether it would be worthwhile to establish a separate division for applied mathematics and to examine what might be done to encourage research in the area. On the Wednesday evening immediately after the council had convened, about fifty attendees met informally. For two hours, they put their heads together, mulling over Tukey's proposal.

Reporting back on the discussion that took place that night, Tukey noted that members' first and overarching concern was preserving the unity of mathematics. They believed, Tukey summarized, that there existed a "need for more convenient 'osmosis' between the different 'cells' of mathematics."¹⁰² In part, the committee was charged with determining exactly how to achieve this "osmosis."

Hilderandt appointed some of the leading applied mathematicians in the country as committee members. As Chair of the Committee, he nominated John L. Synge, an Irish mathematician and Chair of the Department of Mathematics at Carnegie Mellon, and for members he appointed Richard Courant (NYU), Ruel Churchill (University of Michigan), Griffith Evans (University of California, Berkeley), W. T. Martin (MIT), John von Neumann (Institute for Advanced Study), and John Tukey (Princeton University). On September 3, Synge, in his capacity as chairman of the committee, began canvassing members' opinions. Synge urged committee members to offer any "general comments or suggestions" they might have, but warned, "there is a danger of misunderstanding as to the meaning of the term 'applied mathematics'."¹⁰³ Each member of the committee, Synge proposed, should provide a statement defining what he thought "applied mathematics" meant. Synge's implication was that before the committee could decide how best to support applied mathematics, it first had to figure out what exactly applied mathematics was. To achieve this goal Synge also solicited opinions from dozens of other (mostly) applied mathematicians outside the membership of his committee.

By December, Synge had received twenty-six responses. He forwarded the relevant section of each reply to committee members noting that the "question of a separate division is a

¹⁰² John Tukey to Saunders MacLane 06 September 1946, RCP, Box 22 (new), Folder 13.

¹⁰³ John Synge to Churchill et al., 3 September 1946, AMSR, Box 31, Folder 40.

live question among the applied mathematicians of the Society as a whole.”¹⁰⁴ While the original members of the committee were nearly unanimous in their agreement that a separate division should not be established within the AMS (John von Neumann being for a short while the only exception), this was not the case with the other mathematicians Synge consulted. Of his twenty-six respondents, only nine were strongly against establishing a separate division. Eight were in favor of Tukey’s original proposal, and three more were in favor of creating a separate organization outside of the AMS.¹⁰⁵ All agreed that the current status quo was unsatisfactory, but the reasons and justifications that each of the respondents gave were diverse. What these twenty-six responses do make apparent, however, is how deeply entangled were institutional and ontological definitions of the field during this period. Applied mathematics had emerged from the war in complete disarray – the field, so the consensus went, must be advanced and developed, but mathematicians first had to agreed what it was, or what they wanted it to be.

Synge took up the challenge in October, circulating a memorandum among committee members in which he attempted to define “applied mathematics.” Synge, who had been educated at Trinity College, Dublin, moved to the US via Canada in 1939. His research mostly focused on mathematical physics. Yet his definition of applied mathematics was utterly divorced from the content of mathematical research; instead, he defined pure mathematics purely institutionally. If the members of the Chemical Society are chemists and members of the Society are mathematicians, Synge explained in his memorandum, then one can deduce who is a chemist simply by looking at the presidents of the Chemical Society in the past ten years. Similarly, by looking at the past presidents of the AMS, a composite image of the mathematician emerges. “We cannot form a similar picture of an ‘applied mathematician,’” he lamented, “because there

¹⁰⁴ John Synge to Churchill et al., 4 December, 1946, AMSR, Box 31, Folder 41.

¹⁰⁵ Not all respondents offered a clear statement one way or another, instead making various suggestions as to what the Society could do to support research in the field, which explains why the numbers do not add up to twenty-six.

has been no organization which, by a sifting process of presidential election, reveals a type.”¹⁰⁶ In part this was due, Synge wrote, to the fact that applied mathematicians were too heterogeneous, variously composed of mathematicians, physicists, engineers, and astronomers, thereby obfuscating any readily identifiable and isolable “type.”¹⁰⁷ “Possibly,” he added, “the applied mathematician of the present day is essentially an anarchist, in revolt against the division of knowledge into isolated compartments.”¹⁰⁸ Synge’s “anarchist” might appear a far cry from Weaver’s selfless worker. Nonetheless, it closely reflected the realization of many in the mathematical community that a proliferating constellation of subjects could now lay claim to the rubric of “applied mathematics.” Such circumstances, needless to say, made daunting any attempt to define the contours of the field.

By the time the committee had begun its investigation, Tukey had changed his mind, no longer believing that a separate division was unnecessary. He also took up Synge’s challenge. He agreed with Synge that it might prove impossible to strictly define “applied mathematics,” and therefore changed tacks: he tried to define “several kinds of applied mathematics.”¹⁰⁹ Tukey divided applied mathematics according to three criteria: “the subject to which applied,” “the importance of ‘explicit’ solutions” and “the importance of experimental data.” In the first column, Tukey included applied mathematical fields that had been recognized as such by nineteenth-century mathematicians (e.g., mechanics, electrodynamics, hydrodynamics, and probability), as well as more recent fields of application, such as mathematical economics, mathematical psychology, and mathematical biology. The second category included applications of mathematics to questions “proposed by non-mathematicians” or those whose “principle interest is not mathematics,” as well as the theory and technology of computing. Finally, the third

¹⁰⁶ John Synge, “Memorandum on Applied Mathematics,” 16 October 1946, AMSR, Box 31, Folder 40.

¹⁰⁷ Ibid.

¹⁰⁸ Ibid.

¹⁰⁹ John Tukey to John Synge, 30 September 1946, AMSR, Box 31, Folder 39.

criterion covered those applications in which either part of the problem or the entire set-up was experimentally driven.¹¹⁰ All told, Tukey devised fifteen subcategories of branches of applied mathematics, but was quick to note that this list was not exhaustive, representing “only *a part* of applied mathematics.”¹¹¹ Tukey’s decision to define applied mathematics by enumerating all its subdisciplines might seem at first to be a more satisfying – or at least synoptic – definition than Synge’s, but what unified all these subfields remained an open question. What did the various practitioners in these fields share? Why were they, as a group, different from, say, physicists? What, at the end of the day, did the mathematical biologist and the expert in hydrodynamics have in common?

Norman Levinson, an MIT mathematician, noted that the problem inherent in such a definition of the field was that it did not attend to commonalities. Arguing against the creation of a separate division, he wrote to Synge, “I do not believe that a man engaged in one of the many extremely diversified subjects which are called applied mathematics will necessarily have more interest in the work in other branches of applied mathematics than in other branches of pure mathematics.”¹¹² Applied mathematicians must remain closely connected to the other mathematicians in the Society, Levinson explained, since it is the use of mathematical theories, such as partial differential equations or integral equations, that truly unites them. Henry Wallman, one of Levinson’s MIT colleagues, also wrote to Synge to voice his opposition to the foundation of a separate division. Wallman, who had spent the war years working at MIT’s famed Radiation Laboratory, complained, “I have seen instance after instance of clumsy and roundabout mathematics employed by research workers having unquestionably high ingenuity and

¹¹⁰ It is these last two categories the symbolized most clearly the reconfiguration of applied mathematics in the war, since most of the defense related research conducted by mathematicians was in these two categories.

¹¹¹ John Tukey to John Synge, 30 September 1946

¹¹² Norman Levinson to John Synge, 23 October 1946, AMSR, Box 31, Folder 41.

intelligence, but lacking adequately broad basic mathematical training.”¹¹³ The ongoing boundary negotiations that Synge acknowledged in his memorandum were, thus, recognized by several others. Finally, William Prager, who during the war founded the *Quarterly of Applied Mathematics*, expressed similar sentiments. “Unfortunately,” he fulminated, “too many ‘applied mathematicians’ believe that one can take the mathematical formulation from the physicists, chemists, etc.”¹¹⁴ Applied mathematicians, according to all three men, must be distinguished by their strong grounding in mathematical theory, but it was not clear how they cohered as a group.

Still, the second measure of applied mathematics Tukey proposed, which he defined as “the importance of an ‘explicit’ solution,” was not recognized as applied mathematics by all those who responded to Synge’s letter. Merrill M. Flood, who served as the head of the Fire Control Group at Princeton where Tukey worked during the war, cast his vote calling for the creation of a separate division of the Society. Flood, who received his PhD from Princeton in 1935 but moved to statistics in the late 1930s, concluded his letter, “as a general comment, I consider it highly undesirable for the proposed applied mathematics division to concern itself with work which is simply application of mathematics to problems in other fields, and where the real content and interest arises because of the contribution of the work to the field of application rather than to mathematics.”¹¹⁵ Applied mathematics was not science, it was mathematics. Tukey might be content with including problems “whose principle interest is not mathematics,” but not so Flood.¹¹⁶ Frantisek Wolf, an applied mathematician who had immigrated to the United States five

¹¹³ Henry Wallman to John Synge, 12 September 1946, AMSR, Box 31, Folder 41

¹¹⁴ William Prager to John Synge, 19 September 1946, AMSR, Box 31, Folder 41.

¹¹⁵ Merrill M. Flood to John Synge, 31 October 1946, AMSR, Box 31, Folder 41.

¹¹⁶ Most likely the largest percentage of mathematicians agreed with Flood. In the early 1980s, mathematicians Berkley Rosser wrote letters to every mathematician that was still alive and took part in some war related research. In an article he published in 1982, Rosser summarized the responses he received when he asked for “an account of their mathematical activities during the war.” Many mathematicians, Rosser noted, did not reply. And, many of those who did reply “said they did not really do any mathematics. I had a one-sentence answer from a man who said he did no do a thing that was publishable.” Many mathematicians who took active part in war research did not consider

years earlier and who had joined the Berkley Department of Mathematics, put things succinctly: “the good ‘applied mathematician’ must necessarily be a good ‘pure mathematician’.”¹¹⁷ Both men felt that it was the theoretical aspect of applied mathematics that needed to be fostered, not the instrumental.

Eric Reissner, another MIT applied mathematician, disagreed. “There is an essential difference in outlook between the pure mathematician and the applied mathematician,” he wrote to Synge.¹¹⁸ He added, “This appears to prevent a member of either group from fully appreciating the significance of the work of a member of the other group.” Despite the recent “trend of eminent pure mathematicians dealing with mathematical problems arising in engineering and physics,” applied mathematicians would be best served by cutting ties with pure mathematicians. Applied mathematician, according to Reissner, would benefit greatly from a new organization in which they will feel “united in spirit rather than as members of a sub group of an engineering organization or of a ‘down-to-earth’ division of a society whose members, in the main, appear to be concerned with making deeper and more abstract the body of mathematical knowledge.”¹¹⁹

The view that there is “an essential difference” between pure and applied mathematicians was also advanced by Alston Scott Householder, a mathematician at Oak Ridge Laboratories. In his letter to Synge, Housholder explained, “in Applied Mathematics the interest lies more in the semantic [sic] than in the syntactic problems... in general, syntactic elegance is not the chief criterion for applied mathematics, but rather the elegance of the system as descriptive of experience.”¹²⁰ Applied mathematics could be theoretical but it had to be rooted in the world.

their work mathematics as it did not fit with their conception of what mathematical research entails. Rosser, “Mathematics and Mathematicians in World War II.”

¹¹⁷ “Memorandum from Professor F. Wolf, University of California,” 18 September 1946, AMSR, Box 31, Folder 40.

¹¹⁸ Eric Reissner to John Synge, 30 October 1946, AMSR, Box 31, Folder 41.

¹¹⁹ Ibid.

¹²⁰ Alston Scott Householder to John Synge, 7 November 1946, AMRS, Box 31, Folder 41.

Finally, some of the mathematicians who replied to Synge's letter confessed to not being able to define the field at all. Reul Churchill, who headed the Seminar on Applied Mathematics at the University of Michigan, exclaimed that the problem was of intellectual but no practical concern, adding, "I cannot even define mathematics itself!" Still, Churchill observed, it is mostly those mathematicians who are interested in abstract fields that "indicate concern about the definition of applied mathematics."¹²¹ Students, he continued, "are clear enough" to know what sort of training they are receiving. John H. Curtiss began his letter to Synge, "In my role as acting Chief of a not-yet-formed Division of Applied Mathematics here at the Bureau [National Bureau of Standards], I have tried to do a little thinking lately on what applied mathematics really is. So far I have failed quite miserably to arrive at any conclusion."¹²² Before he listed the difficulties that arise when trying to conjure a complete definition, Curtiss wryly commented, "It seems too bad to be founding an institution with such a name and not to be able to define what the name means."¹²³ Curtis proceeded to provide a definition based on the source of the problem and the aim of the research, but he immediately points to the deficiencies of his own definition in accounting for all mathematical research.

What might account for this wide divergence in opinion? The reasoning of James Stoker might help answer this question. In his reply to Synge, Stoker explained that as a mathematician trained in both engineering and "pure" mathematics, he could attest that it is crucial *not* to draw a sharp distinction between pure and applied. "The history of mathematics furnishes very striking evidence of the mutual benefits to be expected from a constant interplay between the two tendencies," he wrote. He then added, "I see no reason to believe that this historical process has

¹²¹ Reul Churchill to John Synge, 4 October 1946, AMSR, Box 31, Folder 40.

¹²² John H. Curtiss to John Synge, 06 November 1946, AMSR, Box 31, Folder 41.

¹²³ Ibid.

ended.”¹²⁴ Yet the continuation of this historical process was exactly what was up for grabs. In 1946, it was still unclear how the war would affect American mathematics. The mathematical community was just beginning to come to terms with the rise of applied mathematics, the national interest it garnered, the changes the scientific patronage system had undergone, and the growth of new fields of research. In a sense, a certain “historical process has ended.” And while Stoker maintained that the intellectual development of the field would proceed as before, what several of the other mathematicians who replied to Synge’s letter realized was that the institutional formation of the discipline could not be contemplated in isolation from its intellectual makeup.

Many of the mathematicians who responded, regardless of their specific opinion, noted either prewar tendencies of American mathematicians to veer toward abstractness, or that of many pure mathematicians to create a sharp hierarchy between applied and pure mathematics. For example, Frantisek Wolf noted that the inclination of the Society’s addresses to be concerned with only “the most abstract of theory,” must be due to the “prevailing ‘fashion’ in mathematics,”¹²⁵ while Housholder complained that “I have the feeling that ‘pure’ mathematicians tend to look down their noses at the ‘applied’ man – I used to do so myself.”¹²⁶ This sentiment was echoed by Hillel Poritsky: “Personally I feel that Applied Mathematics has been treated in a stepfatherly fashion by American Mathematicians.”¹²⁷ Curtiss, however, were more optimistic. He exclaimed, “I am fully aware that the boys with highly abstract interests are pretty much in the saddle in the American Mathematical Society and always have been. But that is really only because the minority (or is it now a majority) with applied interests have not

¹²⁴ James J. Stoker to John Synge, 24 October 1946, AMSR, Box 31, Folder 41.

¹²⁵ “Memorandum from Professor F. Wolf, University of California,” 18 September 1946, AMSR, Box 31, Folder 40.

¹²⁶ Alston Scott Householder to John Synge, 7 November 1946, AMSR, Box 31, Folder 41.

¹²⁷ Hillel Poritsky to Jon Synge, 7 November 1946, AMSR, Box 31, Folder 41.

‘organized’, in the labor union sense.” He was quick to add, “The war did a lot to open the eyes of the topologists and abstract algebraists to the existence of other important parts of mathematics.”¹²⁸ The relation of pure to applied mathematicians remained a source of contention in the following decades, as mathematicians began to come to terms with the changing formation of their field.

Synge’s committee was ready to present its final report to the council in March of 1947. By December, during the annual meeting of the Society, the committee made three recommendations to the council, namely: (1) the establishment of a standing Committee on Applied Mathematics in the Society, whose members would be in charge of organizing programs in applied mathematics during the Society’s meetings and fostering joint meetings with other scientific organizations; (2) that the Society consider the support of the *Quarterly of Applied Mathematics*; and (3) the establishment of an annual 2-3 day symposium dedicated to a specific topic in applied mathematics. However, the committee was not willing to decisively recommend against the establishment of a separate division until March, when it instructed the council that the matter should be taken up again under consideration in two years’ time. The council adopted the committee’s recommendations. The Special Committee on Applied Mathematics eliminated the word “special” from its title but maintained most of its membership, and the first symposium took place at Brown University during the summer of 1947.

As members of the Society had recognized, it was only a matter of time before a separate society was established to represent the needs of applied and industrial mathematicians. Such a society was formed in 1951. The Society itself, despite an honest effort by some of its members to elevate applied mathematics, remained characterized by research in pure mathematics. In hindsight, Richard Burington was most prescient about what was to come. Burington was on

¹²⁸ John H. Curtiss to John Synge, 06 November 1946, AMSR, Box 31, Folder 41.

leave from the Case School of Applied Sciences while working as the head mathematician at the Navy's Bureau of Ordnance. Burington acknowledged the "magnificent job" the Society had done to support research in pure mathematics, though he remained unconvinced that the creation of a separate division would not change the makeup of the Society until a "new generation trained in both pure and applied mathematics appears in sufficient numbers to display their interests and energies in fields of wider horizon."¹²⁹ What Burington understood was that despite growth in applied mathematics, the Society, and for that matter most of the American mathematical community, was still dominated by pure mathematicians. Almost all of those mathematicians who had spent the war away from their home institutions, working on applied military research, returned at the end of the war to their previous research. Their experience during the war had not convinced them to alter their research trajectory.

To a certain degree, Synge's committee similarly acknowledged this state of affairs. When it presented its final report to the council, the committee requested that a short statement entitled "Instruction and Research in Applied Mathematics" be published in the *Bulletin of the AMS* as well as in the *Monthly*. The statement began by noting that while World War II had already begun reversing the half-century separation between mathematicians and scientists, "unless this continues under peace-time conditions, the prospect for the future is serious and warrants earnest consideration."¹³⁰ The development of mathematics, the authors of the statement held, could counter the increasing tendency towards specialization, though only if such an effort were properly supported. To that end, the statement called upon mathematics departments across the country to "consider the feasibility of enlarging their offerings in the direction of applied

¹²⁹ Richard Burington to John Synge, 18 November 1946, AMSR, Box 31, Folder 41.

¹³⁰ "Instruction and Research in Applied Mathematics," AMSR, Box 32, Folder 30.

mathematics, both on the undergraduate and graduate levels.”¹³¹ Like Burington, the committee recognized that unless direct steps were taken to encourage and promote training in applied mathematics, not much would change. However, while training in applied mathematics would garner the attention and concern of some prominent mathematicians, pure mathematicians continued to dominate most of the top mathematics departments in the country in the three decades to come.

¹³¹ Ibid.

Chapter 2.

Necessary But Not Sufficient: Mathematics and the Defense Establishment

“It is certain that few men of our times are as completely free as the mathematicians in the exercise of their intellectual activity. Even if some State ideologies sometimes attack his person, they have never yet presumed to judge his theorems. Every time that so-called mathematicians, to please the powers that be, have tried to subject their colleagues to the yoke of some orthodoxy, their only reward has been contempt.” – André Weil, 1951 [1948]

“Research and advanced training in applied mathematics is at present a ward of the Federal Government, and no change can be anticipated in the near future.... With negligible exception, all academic groups in this field, whether they are part of a department or an institute, receive a substantial fraction of their funds from the government contracts principally with Department of Defense agencies; most of them, specifically the larger ones, would not exist without such support.” – Report on a Survey in Research and Training in Applied Mathematics, 1954

“Mathematics, in a sense, bridges the gap, real or imaginary, which exists between the sciences and the humanities. The exigencies of modern technology have attracted many of the sciences away from their original orbits in the realm of natural philosophy. Mathematics, too, has had its practical part to play in the modern world, but in the process it has never lost its scholarly aura. It occupies an honored place perhaps equally among the humanities as among the physical sciences.” – Alan T. Waterman, NSF director, 1953

In 1950, the *American Mathematical Monthly* published an article entitled “The Future of Mathematics,” written by mathematician André Weil, a recent French émigré to the United States. In the article, which was translated from the French, Weil surveyed recent developments in branches of mathematics from algebraic geometry to differential equations, emphasizing the interconnections between those different subfields. Following in the Bourbaki tradition, Weil aimed to draw attention to the *unity* and vitality of modern mathematics. Yet the article begins not by considering the future of mathematics but, as Weil explains, the future of mankind. “Just as the faithful cleansed themselves before consulting the oracle,” Weil writes, it is necessary to investigate the place of mathematics in contemporary culture.¹ Written in the wake of World War II, Weil contemplates what he conceived as the distinctness of mathematics from the rest of the

¹ Andre Weil, “The Future of Mathematics,” *The American Mathematical Monthly* 57, no. 5 (1950), 295.

sciences. “While some sciences, conferring, as they now do, an almost unlimited power upon a ruthless possessor of their results, tend to become caste monopolies, treasures jealousy guarded under a seal of secrecy,” Weil explained, “the *real* mathematician does not seem to be exposed to the temptations of power nor to the straight-jacket of state secrecy.”² The “real” mathematician for Weil was undoubtedly a “pure” mathematician, and his image of pure mathematics as existing in complete isolation from the social and political world that surrounds it became in the postwar period a cause for both celebration and condemnation.

Only seven months earlier, John Curtiss, chief of the newly established Applied Mathematics Laboratory at the National Bureau of Standards (NBS), published “Some Recent Trends in Applied Mathematics,” in the *American Scientist*. Curtiss, who received his PhD in mathematics at Harvard University under John Walsh, joined the Navy in 1943. He was stationed in Washington DC, where he worked in the quality control section of the Bureau of Ships. In 1946, upon his discharge, Curtiss joined the NBS as Edward Condon’s assistant, and a year later became the chief of the new applied mathematics laboratories. Reflecting on the recent history of American mathematics, Curtiss wrote, “it has been said that World War I was a chemists’ war and that World War II was a physicists’ war. There are those who say that the next world war, if one should occur, will be a mathematicians’ war.”³ If for Weil mathematicians, and hence mathematical work, were completely independent of federal and military concerns, for Curtiss mathematics underpinned all future military work. This chapter accounts for how these two opposing views regarding the relation of mathematics to the military establishment came into being in the first decade after the war, the way in which they were articulated in relation to one another, and their effect on the development of the discipline.

² Ibid., 26.

³ John H. Curtiss, “Some Recent Trends in Applied Mathematics,” *American Scientist* 37, no. 4 (1949), 588.

Whereas during the war the professional mathematical establishment was eager to see mathematicians take part in the defense establishment (as detailed in the first chapter), by the end of the war, the relationship of the two was unclear. For some mathematicians, like Curtiss, the war offered new opportunities and opened up new directions for research, but many (possibly the large majority of) mathematicians, such as MacLane, Morse, and Stone, returned at the end of the war to their prewar research and interests.⁴ The work they produced during the war was an anomaly, not a new direction. Weil celebrated this tendency in his article, declaring, “let others besiege the offices of the mighty in the hope of getting the expensive apparatus.” A mathematician only requires pencil and paper, and sometimes, Weil was clear to add, he can even make do without that. Others like Curtiss believed not only that everything needs to be done to ensure the continued growth of those mathematical fields of research that proved useful for defense, but that the future of military research depended on mathematics. The growing importance of applied mathematics, Curtiss wrote in 1948, “is one of the most significant trends in science today.”⁵ These two pronouncements exemplify the kind of divisions that fractured pure and applied mathematics at the end of World War II, which were articulated not in terms of the research content or methodologies specific to either, but rather with regard to the military and defense work that gave rise to the latter.

A large body of literature in the history of science dedicated to science in the postwar era has investigated the effect of military patronage on knowledge production across a variety of

⁴ That most mathematicians would return to their university position and their prewar research was already acknowledged even before the war officially ended. In a letter to Ward Davidson on plans for demobilization, Weaver noted that many of the mathematicians involved in the Panel’s various projects are not likely to continue their war research. For example, commenting on the group working at the Institute of Advanced Studies, Weaver wrote, “I am by no means sure that the Institute (or von Neumann) would be interested in continuing a contract.” The statistical group at Princeton could in principle continue their war research, Weaver speculated, but “this does not... appear to me to be a very likely move.” Finally the large groups at Columbia and Northwestern University were composed of mathematicians from various institutions. “In only a negligible fraction of the cases,” Weaver added, “would the persons be interested in continuing work of this sort.” Warren Weaver to Ward Davidson, 23 October 1944, RAMP, Box 18, Folder “Demobilization.”

⁵ John H. Curtiss, “A Federal Program in Applied Mathematics,” *Science* 107, no. 2776 (1948): 257.

fields, from the physical sciences and earth sciences to the social sciences.⁶ Whereas most sociologists and historians of science agree that the incursion of military patronage during the Cold War influenced the setting of research agendas, the degree to which defense-related research directed scientific inquiry remains a debated topic. In its most extreme versions, the debate is often framed around the work of Paul Forman and Daniel Kevles. Whereas Forman claimed that physicists might have maintained the illusion of autonomy but were exploited by the defense establishment, Kevles argued that despite expanding military patronage, physicists continued to control the development of their field.⁷ More recently, in her study of oceanography in the Cold War, Naomi Oreskes suggested a more nuanced analysis of the relation between the putative freedom of scientists and the governance of the military. According to Oreskes, oceanographers appropriated the “ideology of pure science” in “private” as a way of gaining a degree of autonomy, while maintaining in “public” that their research was completely

⁶ See, for example: Paul Forman, “Behind Quantum Electronics: National Security as a Basis for Physical Research in the US, 1940-1960,” *Historical Studies in the Physical Sciences* 18, no. 1 (1987); Daniel J. Kevles, “Cold War and Hot Physics: Reflections on Science, Security and the American State” (1988), <http://authors.library.caltech.edu/27745/>; Chandra Mukerji, *A Fragile Power: Scientists and the State* (Princeton, NJ: Princeton University Press, 1989); Peter Galison and Bruce William Hevly, *Big Science: The Growth of Large Scale Research* (Stanford, CA: Stanford University Press, 1992); David H. DeVorkin, *Science With a Vengeance: How the Military Created the US Space Sciences After World War II* (Springer-Verlag, 1992); Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York, NY: Columbia University Press, 1993); Finn Aaserud, “Sputnik and the ‘Princeton Three’: The National Security Laboratory That Was Not to Be,” *Historical Studies in the Physical and Biological Sciences* 25, no. 2 (1995): 185–239; Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, IL: University of Chicago Press, 1997); Mark Solovey, “Introduction: Science and the State During the Cold War: Blurred Boundaries and a Contested Legacy,” *Social Studies of Science* 31, no. 2 (2001): 165–170; Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science* (Seattle, WA: University of Washington Press, 2005); Joel Isaac, “Thu Human Sciences In Cold War America,” *The Historical Journal* 50, no. 03 (2007): 725–746; Hunter Heyck and David Kaiser, “Introduction: New Perspectives on Science and the Cold War,” *Isis* 101, no. 2 (2010): 362–366; Joel Isaac, “Introduction: The Human Sciences and Cold War America,” *Journal of the History of the Behavioral Sciences* 47, no. 3 (2011): 225–231.

⁷ Daniel J. Kevles, “Cold War and Hot Physics: Reflections on Science, Security and the American State” (1988); Paul Forman, “Behind Quantum Electronics: National Security as a Basis for Physical Research in the US, 1940-1960,” *Historical Studies in the Physical Sciences* 18, no. 1 (1987).

uninfluenced by their patronage. Finally, “in practice” they learned to focus “on the positive” and appreciate the increased possibility afforded them by defense funding.⁸

The case of applied mathematics fits the narrative that maintains that military funding shaped the sorts of questions studied and the scientific fields developed during the Cold War. It is enough to look at the growth of computing, game theory, mathematical statistics, and fluid dynamics in this period to demonstrate how implicated postwar mathematical theory was in specific military and industrial concerns. The establishment of applied mathematics laboratories in the National Bureau of Standards and several academic universities was completely dependent on money from the federal government and various branches of the military. Yet the growth of applied mathematics also points to the limited influence of external funding alone in the development of new scientific knowledge. Despite the substantial federal support enjoyed by the field, it did not become a staple of academic research, and applied mathematicians remained a minority within the American mathematical community. In part this was due to the fact that what constituted applied mathematics was continually changing. More significantly, it is precisely the close association of the field with the military that accounted for its failure to thrive in American universities.

Applied mathematicians bitterly argued throughout the 1950s that an imbalance existed between the development of pure and applied mathematics in the United States. They were not vying for money, which was for the most part easily afforded to research in the field; rather, they struggled for recognition. A presumed hierarchy that placed pure squarely above applied research impeded, they argued, the development of the field. Yet the close association of applied mathematics with military objectives only further enforced this hierarchical division in the field.

⁸ Naomi Oreskes, “A Context of Motivation: US Navy Oceanographic Research and the Discovery of Sea-Floor Hydrothermal Vents,” *Social Studies of Science* 33, no. 5 (2003): 697–742.

Applied mathematics was marked by a presumed emphasis on utilitarian objectives, and therefore did not fit the existing academic mathematical landscape. This chapter argues that as far as applied mathematics was concerned, military funding was a Catch-22; the field relied on military support in the postwar period, but it also separated such work from most academic research in mathematics. Federal funding was necessary for the development of the field, but it was not sufficient.

At the heart of the literature on Cold War science is an analysis of the shifting relation between basic and applied science. The popular narrative promoted by scientists in the postwar era, most famously by Vannevar Bush, held that basic research would inevitably stimulate unforeseen discoveries and applications. Yet, as Oreskes has shown in the case of oceanography, it was instead the development of applied military research that encouraged the discovery of unforeseen pure results.⁹ Benjamin Wilson and David Kaiser have similarly shown how Cold War military technology was appropriated by and in turn enabled breakthroughs in theoretical physics. “Neither ‘pure’ nor ‘applied,’” they write, this new result “was Cold War science through and through.”¹⁰

For American mathematicians, the growth of applied mathematics was the cause for which to voice their opinions regarding the relation of basic to applied research, on the one hand, and federal patronage on the other. In the immediate aftermath of the war, most mathematicians, as is evident in Chapter One, still argued that the unity of mathematics must be preserved and that an “unnatural” separation of pure and applied mathematics should be avoided at all costs. Yet by the mid-1950s, this “unity” was harder to maintain. Despite mathematicians’ continuous assertions to the contrary, the postwar politics of basic and applied research enforced a separation

⁹ Ibid.

¹⁰ David Kaiser and Benjamin Wilson, “Calculating Times: Radar, Ballistic Missiles, and Einstein’s Relativity,” in *Nation and Knowledge: Science and Technology in the Global Cold War*, ed. Naomi Oreskes and John Krige, forthcoming.

between pure and applied mathematics across theoretical and practical lines. By appropriating the rhetoric of basic research, pure mathematicians were able to maintain almost complete autonomy over their field. This was also enabled by the fact that, relatively speaking, necessary support for pure mathematics was meager. A mathematician might need more than pencil and paper, despite Weil's suggestion, but support for the field did not require expensive apparatuses or large research facilities. Thus, the 1950s and 1960s are characterized, at least from the perspective of the academic milieu, by a turn toward abstractness and generalization – *not*, as one might expect, by increased applicativeness.

In 1952, the Division of Mathematics at the NRC appointed a committee to study the state of applied mathematics. The Korean War was still raging, and various mathematicians in the American community believed that the field had not sufficiently advanced in the years that had passed since the end of the war. The final report of the committee, which made a recommendation as to what could be done further to support research and education in the field, was published two years later. Yet the road to publication was strenuous. This chapter uses the report and the controversy that surrounded it as a window onto the state of applied mathematics in the United States during the 1950s. The report serves as a benchmark for analyzing the failure of applied mathematics to flourish in the first two decades after the war.

I begin the chapter by detailing the initial controversy that surrounded an early draft of the report. The dispute, which revolved around the “proper” appreciation of applied mathematicians (or rather lack thereof) by the greater mathematical community, brought to the surface deep seated, historically contingent, debates about the relations between pure and applied mathematics. The supposed hierarchy between the two was a crucial factor in the inability of applied mathematics to take hold in American universities. The reconfiguration of the relation between the state and the scientific establishment had a noticeable effect on the development of

mathematics in the postwar period, but so did mathematicians' conception of their field and its history. Mythology and ideology played a powerful counterpart to the defense establishment. These were old fights, but they were nonetheless being waged within a new social and political context. Specifically, they became completely implicated in the ideology of basic and applied research, which was mapped onto the distinction between pure and applied mathematics.

Examining how mathematicians on both sides of the divide appropriated this new language sheds light on the ways the relation of pure and applied mathematics was reconfigured at the end of the war.

The second section examines the effects of the close affiliation of applied mathematics with the defense establishment on the development of the field. Not only was research in the field a direct outgrowth of war research, but in the aftermath of the war it continued to be completely dependent on the support of military agencies. Even those applied mathematicians who believed that the field should be developed independent of practical applications recognized that dependence on defense contracts often implied that practical considerations could impede theoretical investigations. Finally, as during the war, research in applied mathematics continued to be structured around projects involving the work of several mathematicians, rather than single mathematicians working in isolation. In each of these aspects, applied mathematics was vastly different from academic research in mathematics.

The third section looks at the ways in which pure mathematicians appropriated the ideology of basic research to ensure continued fiscal support for the field and maintain its dominance in academic departments. It suggests that mathematicians began conceiving research in the field in terms of this prevailing ideology. That is, mathematicians began to themselves understand research in pure mathematics according to the philosophy underpinning the

distinction between basic and applied research, as distinct from pure research pursued for its own sake.

“Impalpable Ever-Present Haze of Suspicion”

“If the National Research Council is to support an attack on pure mathematics,” Saunders MacLane wrote to Leon Cohen in July 1954, “this attack should be specifically labeled as such and not mixed in with a careful analysis of the nature of the applications of mathematics.”¹¹ MacLane had just completed reading an early draft of the final report of the Committee on Research and Training in Applied Mathematics and he was angry. A few weeks before he received MacLane’s letter, Cohen, who only a year earlier had become the Program Director for Mathematical Sciences at the NSF, expressed his own dissatisfaction with an early draft of the report. In a letter to Joachim Weyl, the author of the report, he wrote, “I hope that it will undergo a fundamental revision before publication.”¹² In May, a copy of the Report was presented at the annual meeting of the Division of Mathematics at the National Research Council, leaving several attendees incensed. Most of the outrage was triggered by a specific section of the report entitled “Applied Mathematics in the Scientific Community.” Noting that the section “has been interpreted as a violent attack on the American Mathematical Society,”¹³ Adrian Albert, the current chair of the division, circulated a copy of it among members of the division and leaders in the society.

¹¹ Saunders MacLane to Leon Cohen, July 1954. MMP, Box 13, Folder “Stone.”

¹² Leon Cohen to Joachim Weyl, 21 June 1954, AMSR, Box 39, Folder 50.

¹³ Adrian Albert to Members of the Division of Mathematics, Undated, AMSR, Box 40, Folder 4.

Marston Morse circulated a memo among several mathematicians announcing that he would vote for the report only if the “paragraphs with the political implications are eliminated.”¹⁴ Stone, who was an official member of the committee, was even more outraged. In June, he wrote to Detlev Bronk, president of the National Academy of Science, and William Rubey, chairman of the NRC, in protest. Weyl and other members of the Committee, Stone proclaimed, desired to publish as part of the final report “statements about the mathematicians and mathematical organizations of the country, which seem to be misleading, offensive, and destructive.”¹⁵ He also took the opportunity to announce that because attempts to convince the committee to change its report has proven futile, he decided to file a minority report.

The Committee on Research and Training in Applied Mathematics had been established two years earlier. In April 1952, as chairman of the Division of Mathematics, Morse sent a letter to Allan Waterman at the NSF, Mina Rees at the Office of Naval Research (ONR), and representatives of the Office of Ordnance Research and Air Research and Development Command announcing the appointment of the new committee. Its goal, Morse explained, was to study what universities, the government, and industry could do to support research and training in the field. Asking for financial support for the committee’s work, Morse noted that the above organizations had a “natural interest” in the results of such a study.¹⁶ Despite the obvious growth of applied mathematics in the aftermath of the war, by the early 1950s the field did not prosper as some had anticipated. The confusion that surrounded the constitution of the field in the immediate aftermath of the war persisted. Moreover, the field failed to take hold in traditional

¹⁴ Marston Morse, “Comments on a Survey of Training and Research in Applied Mathematics.” 8 June 1954, MMP, Box 13, Folder “Stone.”

¹⁵ Marshall Stone to Detlev Bronk and William Rubey, June 27, 1954. MMP, Box 13, Folder “Stone.”

¹⁶ Marston Morse to Alan Waterman, Mina Rees, T. J. Killian, and Colonel C.G. Haywood, April 10 1952, JTP, Series I, Box 24, Folder “National Research Council – Committee on Training and Research in Applied Mathematics.”

academic departments, both in terms of faculty appointments and graduate training. The committee was in charge of surveying current trends and making future recommendations.

It met for the first time on June 5, 1952. Committee members included some of the leading applied mathematicians in the country: Richard Courant from NYU; Abraham Taub, a mathematical physicist from the University of Illinois; Edward J. McShane from the University of Virginia; John Tukey, a statistician from Princeton University; and Hendrick W. Bode, an engineer from Bell Telephone Laboratories. As a representative of pure mathematics, the committee also included Marshall Stone.¹⁷ Before the committee officially began its work, it was agreed that work on the survey would require the employment of a full-time investigator. Following Mina Rees's recommendation, Joachim F. Weyl was appointed for the job.

Joachim Weyl was the son of the famous mathematician Hermann Weyl. Father and son immigrated to the United States from Zurich in 1933 and settled in Princeton, New Jersey. Following in his father's footsteps, Joe Weyl received a doctorate in mathematics from Princeton University in 1939 under the supervision of Samuel Bochner. Standing beside him during the degree ceremony was John Tukey. After he graduated, he moved to the University of Maryland and Indiana University but, like Tukey, during the war he became involved in military research and slowly moved into applied mathematics. At the end of the war, Weyl began working in the Mathematics Branch of the ONR under Mina Rees, eventually succeeding her in 1949. In 1952, when work on the Survey began, Weyl was familiar with the federal support system for science. Whereas Tukey divided his time after the war between Bell Labs and Princeton University, Weyl's career, which revolved around the administration and organization of federal funds for research, represented a new path for mathematicians in the aftermath of the war.

¹⁷ Marston Morse, John Von Neumann, and John Curtiss were appointed as official consultants.

Early on in the committee's work, members decided that its report would be based in part on two conferences. The first conference, which held at Columbia University from October 22 to 25, was dedicated to the question of training in applied mathematics.¹⁸ It was followed a month later by a Symposium on Special Topics in Applied Mathematics at Northwestern University, whose goal was to present current research trends in the field to a broad audience of mathematicians.¹⁹ Both conferences were attended by leading applied mathematicians from public and private universities, technical schools, government laboratories, as well as industry, and the proceedings of both were published independently and as part of the final survey report. In addition to the two conferences sponsored by the survey, the final report and recommendations were also based on a questionnaire Weyl sent to representative departments of mathematics around the country.²⁰ Based on the responses to this questionnaire, as well as informal visits and conversations with scientists and mathematicians, Weyl began drafting the committee's report.

On March 3, Weyl send an interim report to members of the committee and the various representatives of the federal and military bodies. The draft, as he explained in his letter, was based on various discussions that took place during the committee's meeting, and was meant to elicit the reaction of its members. "It is hoped that corrections, re-orientations, insistence on certain specific points, and deletion of others will be precipitated by this report," Weyl wrote to the chair of the committee, Abraham Taub.²¹ Weyl welcomed criticism, and hoped that the draft of his report would generate continued conversations among the members of the committee, but

¹⁸ The proceedings of this conference will be discussed in greater detail in Chapter Three, which examines the training of applied mathematicians in the aftermath of the war.

¹⁹ Joachim F. Weyl, "Introduction and Critique," *American Mathematical Monthly* 61 (1954): 1–4.

²⁰ The questions included were divided into six groups: two focused on the training program, one dealt with the "attractiveness of the field," one with "research activities," another with the "support picture," and the last was devoted to general questions.

²¹ Joachim Weyl to Abraham Taub, 3 March 1954, JWTP, Series I, Box 24, Folder "NRC – Committee on Training and Research in Applied Mathematics."

even he was not ready for the strong reactions and quarrel his report instigated. Most of the criticism was directed toward one section.

“Applied Mathematics in the Scientific Community” began by declaring that there exists “a deep-running undercurrent of feeling to the effect that the applied aspects of mathematics and those who have a concern therewith fail to receive their due respect and recognition in the representative organizations of American mathematics.”²² It suggested that since the philosophy and interests of these organizations affect the development of the field writ large by influencing students, publications, and the allocation of funds, it is of paramount importance that these organizations have an accurate understanding of the constitution and importance of the field.²³ As detailed in the previous chapter, the “proper” development of research in applied mathematics became a source of fierce disagreement between the scientific establishment and the mathematics profession. In the aftermath of the war, the debate did not dissipate, but the parties involved did change. Instead of the civilian scientific authority, the calls for developing applied mathematics now came from a heterogeneous group of applied mathematicians, the majority of whom became active in the field through their involvement in war research. More than a decade after the Applied Mathematics Panel was established, applied mathematicians still felt that their place within the mathematical profession was insecure.

The report identified three factors that impeded one’s true appreciation of applied mathematics. The first misconception the report singled out had to do with the supposed “creative freedom” the applied mathematician, “especially when in non-academic employment, enjoys, -- or, rather fails to enjoy.” Since the applied mathematician is “supposedly bound to produce specific results,” he tends to be seen as “a professional craftsman, plying a trade he learned” as

²² “Applied Mathematics in the Scientific Community.” AMSR, Box 40, Folder 4.

²³ The AMS was by far the largest professional organization for research mathematicians at the time. The Mathematical Association of America was historically concerned with mathematical education. Thus, it was clear that the report was here directly referring to the AMS.

opposed to doing original and creative work. The second misimpression the report identified was the belief that applied mathematicians only solve “dull problems by repulsive means.” Finally, there was an “impalpable ever-present haze of suspicion” that abstractly-minded mathematicians could, if so inclined, do a better job than “their applied cousins by the bothersome problems of the world around us.”²⁴ All three misconceptions, the controversial section suggested, prevented applied mathematicians from fully taking part in American universities.

Many applied mathematicians felt that, at best, their research was marginalized within the mathematical community, if not been completely stifled by pure mathematicians.²⁵ They pointed to the fact that applied mathematicians were not equally represented on various national committees or among the leadership of the society. Research articles in the field rarely appeared in the top mathematical journals, and during annual conferences the scientific program tended to treat applied mathematics “on a segregated basis.”²⁶ All these factors, according to the report, hindered the growth of applied mathematics as an academic discipline and all could be traced back to pure mathematicians’ lack of understanding and appreciation of applied mathematics.

The report traced this view back to the Göttingen mathematical seminar at the turn of the century. Such “uncompromising insistence [*sic*] on the unworldliness of mathematics had its origin apparently in just those circles at the time.”²⁷ It goes on to ask its readers to speculate how

²⁴ “Applied Mathematics in the Scientific Community,” AMSR, Box 40, Folder 4.

²⁵ This sentiment was present in the society before the draft of the report was circulated. For example, in January 1953, William Whyburn, a mathematician at the University of North Carolina, wrote to Edward Begle, Secretary of the AMS, to report on the unhappiness of some of the society’s constituents. “There is one matter that quite a number of members of the Society have mentioned to me recently... this concerns the feeling on the part of a sizable portion of the Society membership that they are not receiving enough from the Society publications to justify the rather high dues that they are paying. This is particularly true among people who work in such applied areas as mathematical statistics, fluid dynamics, gas dynamics, etc.” In his reply to Whyburn, Begle acknowledged that “from time to time I get letters such as yours... expressing dissatisfaction with the treatment of applied mathematics by the Society.” William Whyburn to Edward Begle, 19 January 1953; Edward Begle to William Whyburn, 19 January 1953, AMSR, Box 38, Folder 55.

²⁶ The language of the report is not only militaristic, but written in 1954 it also uses rhetoric of social discrimination and class struggle when comparing the community of pure and applied mathematicians.

²⁷ *Ibid.*

a “modern observer” who is unfamiliar with “historical traditions” would assess the current disposition of most mathematics faculties given the “purpose they ostensibly are meant to serve.” But this was exactly the crux of the matter. Despite the profound changes the field had undergone in the aftermath of World War II, mathematicians could not, nor were they necessarily interested in, dispensing with their field’s “historical traditions.” The idea that a “modern observer” would be inclined to assume a completely different distribution of research specialties in most mathematics departments given the purpose they “are meant to serve” was exactly the sort of pragmatic attitude mathematicians were eager to fight against. Mathematicians’ belief that an instrumental conception of mathematics would be detrimental to the development of the field did not evaporate at the end of the war. If anything, it was only exacerbated. The goal of the report was to call into question this notion.

Aggravating existing disagreements between pure and applied mathematicians and rooted in historical rivalries and competing ideologies, the controversy was nonetheless waged within a new social, economic, and political context. Specifically, the rivalry between pure and applied mathematicians began to be articulated within the prevailing discourse of basic and applied research that dominated postwar funding policies. Together with the rest of the sciences, mathematicians began to divide research in the field according to these categories. However, it was unclear at first how mathematical research fit within this new taxonomy that underwrote the field’s development. In the process, attributions such as “theoretical,” “pure,” and “applied” became a source of tension for mathematicians, and the controversy that erupted around the draft of the report in 1954 brought them to the surface.

During the war it had already been evident – as well as a source of tension – in the work of the Applied Mathematics Panel that the division between basic and applied research did not map onto the one between pure and applied mathematics. In a November 1943 diary notice

following a trip to Washington, Courant reported that recent memoranda issued by the panel had been criticized by officers in the Naval Ordnance for being “too academic.” Noting that this criticism was only partly justified, Courant remarked that the issue must be brought up for further discussion.²⁸ Two months later, following a discussion with mathematician Hermann Weyl, Courant wrote to Weaver, “Weyl’s papers on shock waves were discussed. They seem to be of such purely mathematical almost axiomatic character that publishing them as N.D.R.C. reports or memoranda might seem objectionable.”²⁹ Applied, even during the war, did not mean practical.

In the postwar period applied research as appose to basic research came to denote projects that had well defined ends. The acquisition of new theoretical knowledge could arise out of applied research, but it was not its main objective. Research in fluid dynamics or elasticity theory, which belonged squarely within applied mathematics, could be directed toward practical ends, but it could just as easily be labeled basic research. When attached to mathematics, that is, the adjective “applied” became misleading. In the aftermath of the war, mathematicians and policy makers alike had to learn how to parse mathematical research according to the reigning funding regime, and the task was far from straightforward.

Immediately after she concluded her work for the Applied Mathematics Panel (AMP) as a technical assistant and Warren Weaver’s right hand, Mina Rees moved to Washington to head the newly established Mathematics Branch of the Office of Naval Research (ONR). Prior to the establishment of the National Science Foundation, the ONR served as the first federally funded organization dedicated to supporting basic research.³⁰ Early on, it was decided that the Branch would support research projects in both pure and applied mathematics, and a committee in the

²⁸ Courant, Richard, “Diary Notice,” 29 November 1943. AMP Box 6, Folder “Courant Diary.”

²⁹ Courant, Richard, “Diary Notice for W.W. Concerning Conversation with H. Weyl,” 8 January 1944, RAMP Box 6, Folder “Courant Diary.”

³⁰ Silvan S. Schweber, “The Mutual Embrace of Science and the Military: ONR and the Growth of Physics in the United States after World War II,” in *Science, Technology and the Military*, ed. Everett Mendelsohn, Merritt Roe Smith, and Peter Weingart (Boston, MA: Kluwer Academic Publishers, 1988), 3–47.

National Academy of Science was established to advise ONR's program in pure mathematics. John von Neumann became the chairman of the committee, and Morse, Griffith Evans, Stone, Hassler Whitney, and Oscar Zariski were appointed as members. The Mathematics Branch under the supervision of Rees was the first organizational body that sought to articulate a support policy for mathematics based on the doctrine of basic research.

In January 1948, Rees published an article in the *Bulletin of the AMS* in which she sought to explain to members of the society "the philosophy which has determined the mathematical research project which ONR is sponsoring."³¹ Rees explained that as a matter of policy it was recognized early on that in order to best support mathematical research in the United States, research in pure mathematics must also be included among the branch's contracts. Yet, noting the dearth of research in applied mathematics at the start of World War II, Rees established that the lion's share of Navy support went to applied research. "It is a fact that over 4/5 of the annual mathematical expenditure is in support of research in 'applied mathematics,' mathematical statistics, numerical analysis and computing devices."³² It was these fields, Rees explained, that the ONR deemed to have the highest priority given their potential future applicability to the Navy's affairs. That more than 80% of the budget for basic research in mathematics went toward supporting research in applied mathematics was a red flag for the mathematical community.

However, as soon as she noted how the budget divides between pure and applied mathematics, Rees acknowledged the difficulty and imprecision of these categorizations. "Although appropriations for theoretical studies in mathematics represent less than 1/5 of the annual total," she explained, "the number of contracts with theoretical objectives is more than 1/3

³¹ Mina Rees, "The Mathematics Program of the Office of Naval Research," *Bulletin of the American Mathematical Society* 54, no. 1 (1948): 1-5.

³² *Ibid.*

of the entire group.”³³ The distinction Rees maintained between “theoretical studies” and studies with “theoretical objectives” begins to demonstrate the problematic position of applied mathematics in the postwar period. Namely, the attribution of “theoretical” to refer to studies in “applied mathematics” indicates the rearticulation of applied mathematics across utilitarian objectives. An ONR contract, following Rees’s logic, could be in theoretical applied mathematics. Yet this terminology was rarely used. Not only was the category of applied mathematics at the end of the war much more expansive than what it was during the first few decades of the century, but it also began to be associated with an emphasis on direct practical results (as it was in the war), as opposed to theoretical studies arising out of empirical conditions.³⁴

In an article in *Science* describing the new National Applied Mathematics Laboratories organization, its first director, John Curtiss, also saw fit to comment on the development of applied mathematics when he divided research in the field into two “levels.” The first, the “research level,” was characterized “by complicated chains of original mathematical reasoning quite similar to those which characterize creative research in pure mathematics,” and the second, the “level of applications of mathematics,” included work which consisted of “fitting already established (or trivially provable) mathematical propositions to situations in other sciences.”³⁵ Both levels were crucial to work in the field, though as Curtiss pointed out, they depended on institutional settings. Surveying the current state of research in applied mathematics, Curtiss

³³ Ibid.

³⁴ In a 1969 interview, Rees commented on the changing meaning of applied mathematics before and after the war. “The thing they [Weaver and Fry] were talking about was much more restricted when they said applied mathematics than what we mean now. I think what we mean now is essentially anything where you use mathematics in [attacking] a real problem, and [this] calls on all branches of mathematics. There just is nothing that is excluded.” If before the war, applied mathematics was more akin to mathematical physics understood as basic research (e.g. the mathematics involved in relativity theory or quantum mechanics), after the war it not only expanded but it also came to be defined through its usefulness in solving specific problems. Mina Rees Interview, 19 March 1969. Computer Oral History Collection, 1969-1973, Archive Center, National Museum of American History.

³⁵ John H. Curtiss, “A Federal Program in Applied Mathematics,” *Science* 107, no. 2776 (1948): 257.

distinguished between educational institutions and government laboratories. While almost all centers of applied mathematics in universities participated in federal scientific research programs, it was in federal laboratories that “the level of application, as contrasted with the level of research,” was “more difficult to meet.”³⁶ Thus, from early on, applied mathematical research in federal settings was associated with a more practical approach, which often involved what mathematicians considered relatively “trivial” mathematics.

The relation between applied and pure mathematics only added to the confusion. In September 1948, physicist Gaylor Harmwell sent a letter to statistician Samuel Wilks asking him to assume the chairmanship of a new subpanel on pure mathematics at the Research and Development Board (RDB) of the Department of Defense. According to Kevles, the RDB, which was established in 1947, “was designed to institutionalize Bush’s vision of a coequal interplay between civilian scientists and professional military officers in the formation of policies for the development and use of new weapons.”³⁷ In his letter, Harmwell informed Wilks of the Committee of Basic Research that had been formed within the RDB. Harmwell has been appointed chair of the Panel of Physics and Mathematics, and it is in this capacity that he urged Wilks to join the organization. At the time, Wilks was one of the (if not the) leading mathematical statistician in the United States. He served on the executive board of the AMP and directed Princeton’s Statistical Research Group during the war. He became an expert on quality control and was well acquainted with the defense establishment.

Yet Wilks did not immediately agree to join the RDB. In a letter to Harmwell, he explained that he felt the need to first consult his colleagues. These discussions prompted him to stipulate his acceptance of the chairmanship on two conditions. “The first proposal concerns the

³⁶ Ibid.

³⁷ Daniel J. Kevles, “Cold War and Hot Physics: Reflections on Science, Security and the American State” (1988): 246.

title of the sub-panel,” he wrote to Harmwell. “I am not a ‘pure mathematician’ in the sense of the term as used in the mathematics profession. My field is mathematical statistics. Therefore, I would not feel that it would be appropriate to accept the chairmanship of a sub-panel on ‘pure mathematics.’” He then added, “actually, I gathered from your letter and even more from our talk that your use of this term is similar to the mathematical profession's use of the term ‘applied mathematics.’”³⁸ Harmwell was hoping to establish a sub-panel in charge of basic research in mathematics. Naming it a committee on pure mathematics, Wilks explained, would be inaccurate. Its title should therefore be altered. Wilks’s second proposal included separating mathematics from physics.

How research in mathematics should be parsed did not become clearer when the NSF, with its dedicated aim of supporting basic research, began supporting research in mathematics. It was not obvious at first what kind of research would be supported and how the NSF would allocate its budget. In January 1953, William Duren, who just completed a short term as the Acting Program Director for Mathematics at the NSF, published an article in the *Bulletin of the AMS* regarding support for mathematical research. The experience, Duren explained, had made one thing clear: the NSF Act of 1950 did not “in itself create a new era in which a non-military arm of the Government will support *basic* research in theoretical mathematics without the demand for direct military or even physical science relatedness.”³⁹ Mathematicians, Duren noted, did not seem to realize that research in mathematics, even if of a theoretical nature, still needed to be justified if it was to receive any support.

During those early days of the NSF, confusion ran high. The justification for support of basic research was always made by reference to future applications. The 1952 annual NSF report

³⁸ Samuel S. Wilks to Gaylord Harmell, 11 October 1948, AMSR, Box 34, Folder 22.

³⁹ William L. Duren, “The Support of Mathematical Research by the National Science Foundation,” *Bulletin of the American Mathematical Society* 59, no. 1 (1953): 1.

stated the case clearly when it noted in a section describing the general policies of the organization that “the technological sequence consists of basic research, applied research, and development.”⁴⁰ In making the case for support of basic research, the report went on to quote Alexis de Tocqueville’s century-old description of American science as highly pragmatic. Tocqueville’s proclamation that “scarcely anyone in the United States devotes himself to the essential theoretical and abstract portion of human knowledge,” the NSF report assured its readers, “is no longer true.”⁴¹ However, the expectation that theoretical knowledge could shape practical knowledge remained an American staple. It was clear that aerodynamic theory had military and practical “relatedness,” but what about algebraic topology? Where did research in pure mathematics fit within the “technological sequence” advanced by the NSF? Did research in abstract algebra independent of “real” world concerns deserve the label of basic research? Put somewhat differently, if research in applied mathematics occupied the position of basic research, than what position was left for research in pure mathematics?

Pure mathematicians were nervous, and they had good reason to be. In November 1952, Adrian Albert, who succeeded Marston Morse as the chairman of the Division of Mathematics, wrote a memorandum on the support of mathematics at the NSF. Albert was a member of the Department of Mathematics at the University of Chicago and a well-respected algebraist. While most of his research was in pure mathematics, Albert became involved early on with research in cryptography and was influential in bringing techniques from abstract algebra to bear on the study of secure communication. In 1961, after he directed several research projects for the Air

⁴⁰ *The Second Annual Report of the National Science Foundation* (Washington, D.C., 1952).

⁴¹ On the formation of the NSF, see: Daniel J. Kevles, “The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945: A Political Interpretation of Science--The Endless Frontier,” *Isis* 68, no. 1 (March 1, 1977): 5–26; James Merton England, *A Patron for Pure Science: The National Science Foundation’s Formative Years, 1945-57* (Washington, D.C.: National Science Foundation, 1982); Jessica Wang, “Liberals, the Progressive Left, and the Political Economy of Postwar American Science: The National Science Foundation Debate Revisited,” *Historical Studies in the Physical and Biological Sciences* 26, no. 1 (1995): 139–166.

Force Office of Scientific Research, Albert became the first director of the Communications Research Division of the Institute of Defense Analysis. He was not shy about developing mathematical applications. Yet his alliances lay clearly with pure mathematics. The foundation has been supporting research in mathematics for only a year at the time, but Albert felt the need to give voice to mathematicians' dissatisfaction with its current policy.

Albert began his memorandum by listing the fields of mathematics, which according to him were algebra, analysis, and geometry and topology. "The field called applied mathematics," he wrote, "has not earned the right to be called a major field since the great advances in mathematics have taken place in the pure fields listed above, and it is the ideas, techniques, and modes of thought of these fields which are finding the broad applications to other sciences today."⁴² According to Albert, applied mathematics, at least as developed in the United States, was just not sufficiently advanced to warrant equal representation among the rest of the mathematical fields.⁴³ Moreover, the recent applications of mathematics were direct outcomes of research in pure mathematics, not applied. Yet Albert reported that an investigation of the recent support patterns of the NSF revealed that 77% of the budget afforded to mathematics went to fields of applied mathematics, 17% to analysis, and only 6% to other fields of pure mathematics. Given the significance of research in pure mathematics, Albert suggested, there was a clear imbalance in the foundation's allocation of funds to the field.

This meager share for support in pure mathematics was even more worrisome, Albert argued, given that the fields of applied mathematics supported by the foundation, unlike studies in pure mathematics, were already receiving support from military bodies. There was no reason,

⁴² Albert, Adrian. "The Support of Mathematics at the National Science Foundation," 13 November 1952. AAAP, Box 1, Folder "A.A. Albert – Personal."

⁴³ Albert went even further in reaffirming the supposed hierarchy between pure and applied mathematics. He wrote that while he fully appreciated the importance of fields such as mathematical statistics and fluid mechanics, these fields were receiving "an emphasis with respect to support which is entirely unjustified by their importance relative to that of basic mathematics" because "such fields are more readily understood by the lay mind." Ibid.

according to Albert, that four out of the eight mathematics consultants in the NSF were applied mathematicians. Albert elaborated on that point in an earlier draft of the report. The field of applied mathematics, he wrote, “can be readily justified as a ‘related’ topic by a military agency,” and so its support will undoubtedly continue. Moreover, since analysis serves as the bedrock of most work in the field, “the concept of relatedness can be stretched to include that field also.”⁴⁴ Despite the fact that “the really fundamental advances in mathematics” have been in “the more abstract parts” of the field, they have for the most part been neglected by the various military agencies. According to Albert, mathematicians did not object to this policy by the military, but they nonetheless expected the foundation to balance this tendency by emphasizing its support for pure mathematics. The incompatibility between mathematicians’ classifications and military and federal ones called attention to the inherent murkiness of the division between basic and applied research. As Albert noted, “the concept of relatedness” can be extended, and pure mathematicians wanted to ensure that such an extension would include abstract mathematics. It was not only applied mathematicians that were feeling that they were not receiving their proper appreciation. Pure mathematicians, at least as far as support was concerned, also felt under attack.

By 1954, the ONR has decided to cut back its program in pure mathematics. In an editorial in *Science*, Rees explained that this change in policy was prompted by the appearance of the NSF. “It is natural that, in mathematics, it [the NSF] should support an outstanding program in some of the more abstract fields, where much of the most significant research is going on.”⁴⁵ As the NSF budget expanded in the mid-1950s, the share of research in pure mathematics grew as well. Between 1953 and 1955, the total amount of funds devoted to support research grants in

⁴⁴ Albert, Adrian. “The Needs of American Mathematics,” undated, AAAP, Box 1, Folder “A.A. Albert – Personal.”

⁴⁵ Mina Rees, “Mathematics and Federal Support,” *Science* 119 (1954): 3.

mathematics increased from \$104,500 to \$562,400. Yet when the committee was drafting its report, it was unclear how much research in the field would receive federal support.

It is against this background that the heated debate the report caused should be understood. After all, the disagreement it inspired was not so much about the specific recommendations made in it, but rather about its language, definitions, and characterization of both mathematics and mathematicians. The report began with the seemingly innocuous question, “what is applied mathematics?” Yet it then explains that the survey “is emphatically not concerned with any one particular segment of mathematics or even with a collection of such segments which it should try to identify on the basis of subject matter as applied mathematics.”⁴⁶ That is, the working definition under which the survey operated was not based on a given set of subjects that could be included under the rubric of “applied mathematics.”⁴⁷ Instead, the report explained that its aim is to consider a “specific kind of activity” which occupies mathematicians: namely, “this activity consists in the creation, the adaptation and communication of mathematics, inspired by and knowingly related to the effort of advancing our rational understanding of some aspect of the world around us.” Applied mathematics, the report succinctly put it, is a matter of “attitude,” not “subject matter.”

So far the final report reflects almost perfectly the earlier draft that circulated among mathematicians in mid-1954. Yet where the final draft describes the relation of mathematics to other sciences, the earlier draft draws a distinction between applied and pure mathematics. Applied mathematics, this first draft explains, is “in one word – *secular mathematics*, as distinct from the *monastic mathematics* which, in turning down the road of completely detached, self-

⁴⁶ Fritz Joachim Weyl, *Final Report on A Survey of Training and Research in Applied Mathematics in the United States* (Society for Industrial and Applied Mathematics, 1956), 1.

⁴⁷ *Ibid.*

motivated abstraction, renounces any commitment to the particular world in which we live.”⁴⁸ The label “pure” was problematic. As Rebecca Lowen noted, “the postwar years did witness a significant rise in prestige and support for ‘basic’ science. But ‘basic’ science should not be confused with ‘pure’ science, or the search of knowledge for its own sake.”⁴⁹ By characterizing pure mathematics as monastic, the report challenged the idea that research in abstract algebra or topology would have any future applicability. Interested in “self-motivated abstraction” pure mathematicians on this view were pursuing knowledge for its own sake with no consideration as to how or whether it might have future uses. Even worse, these mathematicians, the report suggested, actively disregarded any “commitment to the particular world in which we live.”

This was not lost on the pure mathematicians who read the early version of the report. These mathematicians might not have wanted their research to be directly guided by military considerations, but they did hope to benefit from the changing funding regimes of science. Commenting on the use of the adjectives “secular” and “monastic,” Morse wrote, “no knowing mathematician will be misled by these terms, but men such as Chester Bernard [Chairman of the NSF] and Reverend Hesburgh, President of Notre Dame, who are on the Board of the National Science Foundation, might be misled.”⁵⁰ He further added that while he does not object to the use of the word “secular,” the term monastic is “superficial.” “*Creative and uncommitted mathematics* (which I would oppose to secular mathematics) is monastic in no important way,” he concluded.⁵¹ Morse realized that a characterization of pure mathematics as “monastic” could hinder mathematicians’ funding opportunities. He did not attempt to present a pragmatic view of

⁴⁸ “Summary and Recommendations,” RCP, Box 16 (old), Folder “NRC.”

⁴⁹ Rebecca S. Lowen, “The More Things Change...: Money, Power and the Professoriate,” *History of Education Quarterly* 45, no. 3 (2005): 442.

⁵⁰ Marston Morse, “Comments on a Survey of Training and Research in Applied Mathematics.” MMP, Box 13, Folder “Stone.”

⁵¹ Morse argued that “the applied mathematician is much nearer to the monk in his attitude toward applications and objectivity. Like the monk he lives in a world not self created.” Ibid.

the field, characterizing it as “creative” and “uncommitted,” but within the prevailing discourse of basic research he did not need to.

Following an article that had recently been published in *Harpers Magazine* by Whitney Griswold, President of Yale University, in his minority report, which was almost as long as the report itself, Stone described the committee’s disagreement as a philosophically grounded opposition between those who espouse liberal education for its own sake and those who call for a utilitarian end.⁵² “Mathematics, at once a pure creation of the mind and an indispensable tool of modern technology, is fatally caught up in this ancient conflict,” Stone explained.⁵³ “It is small wonder that in these times,” he continued, “there have sprung up ardent champions of the utilitarian pursuit of mathematics.” Stone noted that various subspecies of mathematics have long been used in conflicts over the development of mathematics, as for example “bourgeois mathematics” and “socialist mathematics.”⁵⁴ “Now, I regret to say, the majority report offers us ‘monastic mathematics’ and ‘secular mathematics’ as terms of reference. It seems to me that the introduction of these terms, of which the first is obviously loaded, must be traced directly to the utilitarian component in the majority’s thinking.” The philosophical debates that hindered mathematicians’ involvement during the war did not end. The distinction between pure and applied mathematics was not really about how to classify a given publication, and at stake was not whether shock wave theory could give rise to novel mathematical theory. Rather, what

⁵² Whitney A. Griswold, “What We Don’t Know Will Hurt Us: The Power of Liberal Education,” *Harper’s Magazine* 209 (July 1954): 76–82.

⁵³ Stone, Marshall H. “Committee on Training and Research in Applied Mathematics: Minority Report.” MMP, Box 13, Folder “Stone.”

⁵⁴ The other terminology Stone mentions includes “idealistic mathematics” and “Marxian” or “Stalinist mathematics,” as well as, pointing to Germany in the 1930s, “Jewish mathematics” and “Aryan” or “German mathematics.”

mathematicians continued to argue about was whether the field should develop according to an instrumentalist conception or continue its pre-war idealistic state.⁵⁵

The characterization of pure mathematics in the report as “completely detached” from the world was met by the oppositional portrayal of applied mathematics as utilitarian. Stone’s condemnations of the committee’s utilitarian outlook might have been exaggerated but it did point to the problematic position of applied mathematics in the 1950s. The battle over the report was not really about terminology, nor was it just about the allocation of funds. The war made its mark not only on the field’s intellectual making, but also on its institutional formation. Applied mathematics exhibited a pragmatic outlook that continued to go against the prevailing dogma of American mathematics.

Secular

But what was applied mathematics in 1954? The report identified five sub-categories according to which research in the field had clustered: “the interpretation of physico-chemical phenomena;” “the analysis of engineering systems;” “the interpretation of bio-sociological phenomena;” “operations research, activity analysis, numerical simulation;” and “the role of numerical analysis.” This five-part division already highlights the expanding conception of applied mathematics in the aftermath of World War II. The Committee on Research and Training in Applied Mathematics was in charge of producing a comprehensive survey of the field. Its goal was not to define what applied mathematics was, but to describe the various research activities

⁵⁵ In his article, Griswold argues that the battle between a utilitarian and liberal education has much longer history than is usually presented. Instrumentalism and pragmatism, Griswold claims, did not start with Dewey and his followers but has a much longer history. Griswold’s aim was to reaffirm the place of liberal education by showing the longer history of this debate. This was not, Griswold insisted, a fight between the ancients and the moderns. That Stone chose to begin his minority report with Griswold’s article is revealing. It points to the ways in which mathematics was caught squarely in the middle of these educational debates. Stuck between the humanities and the sciences, the development of mathematics during the period exemplify the broader intellectual transformation undergoing American higher education.

around the country that now laid claim to the field. The report it produced, therefore, serves as a useful guide to the way mathematicians conceived of the contours of the field in the 1950s. It also highlights how deeply implicated the development of the field was in defense research. Most of the “newer” categories of applied mathematics research not only originated in specific military projects but also continued to rely on the defense establishment.

Prior to World War II, applied mathematics referred almost entirely to the first of these categories. As noted in the report, “for more than two centuries,” the interpretation of physical and chemical phenomena was “*the only*, and is still the most important, field for the realization of objective recognition about the outer world in terms of mathematical constructions.”⁵⁶ Research in the field revolved around the theories of hydrodynamics, gas dynamics, elasticity, plasticity, classical optics and the flow of heat, and for the most part involved differential equations. The war drew renewed interests into these areas of research. Not only were open problems in these fields solved, but just as significantly a new community of practitioners, centered for the most part at NYU and Brown University, grew around it.

The investigation of physical and chemical phenomena challenged most clearly the distinction between pure and applied mathematics. It was in this area of research that investigation of a specific physical theory could give rise to new results in theoretical mathematics and vice versa. Yet even within the field a distinction was present between practically oriented and more theoretical investigations, and while the latter was acknowledged and accepted by the broader mathematical community, the former became a symbol of the instrumental approach that threatened to take over the field. Kurt Friedrichs joined Courant at NYU in 1937. He became a leading member of the group and a well-respected researcher in

⁵⁶ Weyl, *Final Report on A Survey of Training and Research in Applied Mathematics in the United States*, 13.

continuum mechanics. It is enough to look at two papers Friedrich published in 1948 to observe how the distinctions between the theoretical and the pragmatic were drawn.

The first paper, which Friedrichs co-authored with Hans Lewy, was published in the *Communications on Pure and Applied Algebra*. The journal was established at NYU in 1948 as a publishing outlet for the group's work.⁵⁷ It joined the *Quarterly of Applied Mathematics*, which was established five years earlier, as the second American publication dedicated to publishing research in applied mathematics. Friedrichs and Lewy's paper was titled "The Dock Problem."⁵⁸ It was part of the group's research on wave theory, and joined other publications that year on "Waves Against an Overhanging Cliff," and "Waves in the Presence of an Inclined Barrier."⁵⁹ The paper gives a mathematical solution to the dock problem, which is stated in the first two lines of the paper. "Suppose one half-plane of an infinite water surface is covered with a rigid plate, the 'dock.' How does this dock influence waves standing or traveling perpendicularly to the edge of the dock on the free water surface?"⁶⁰ The problem, which asks one to imagine an infinite water surface, is an idealized description of what is otherwise a straightforward physical phenomenon. The paper begins with a mathematical description of the problem, and goes on to analyze the behavior of the wave where the water meets the dock.

That same year, Friedrichs published another article in the *American Journal of Mathematics*. The journal was the oldest publication of the American Mathematical Society.

⁵⁷ That the group had to establish its own journal is indicative of the growing fracture between pure and applied mathematics in the aftermath of the war.

⁵⁸ Kurt O. Friedrichs and Hans Lewy, "The Dock Problem," *Communications on Pure and Applied Mathematics* 1, no. 2 (1948): 135–148.

⁵⁹ See: J. J. Stoker, "Waves over Beaches of Small Slope, Under a Dock, Under an Overhanging Cliff, and Past Plane Barriers," *Communications on Pure and Applied Mathematics* 1, no. 2 (1948): 101–108; Kurt O. Friedrichs, "Waves on a Shallow Sloping Beach," *Communications on Pure and Applied Mathematics* 1, no. 2 (1948): 109–134; Fritz John, "Waves in the Presence of an Inclined Barrier," *Communications on Pure and Applied Mathematics* 1, no. 2 (1948): 149–200; Eugene Isaacson, "Waves Against an Overhanging Cliff," *Communications on Pure and Applied Mathematics* 1, no. 2 (1948): 201–209; Sherman Cabot Lowell, "The Propagation of Waves in Shallow Water," *Communications on Pure and Applied Mathematics* 2, no. 2–3 (1949): 275–291.

⁶⁰ Friedrichs and Lewy, "The Dock Problem," 135.

“Nonlinear Hyperbolic Differential Equations for Functions of Two Independent Variables,” begins by stating: “the present paper is concerned with uniqueness, existence, and differentiability of a solution of the initial value problem of systems of hyperbolic differential equations for functions of two independent variables.”⁶¹ Unlike the dock problem, this second paper was not concerned with the solution to some physically defined problem. Rather, it provided a solution to a certain class of differential equations. After introducing the type of hyperbolic differential equations considered in the paper, Friedrichs explains, “the investigations in the present paper are of a purely theoretical character. Nevertheless they throw some light on the question of numerical computation of solutions of hyperbolic equations.”⁶² The paper belonged to theoretical studies of partial differential equations, which did not seek explicit computable solutions but were rather concerned with proving the existence and uniqueness of solutions. These latter investigations were more theoretical and were not motivated by a specific applications.

Friedrich notes further that solutions to hyperbolic equations “recently attracted considerable attention among workers in the field of gas dynamics” and suggests that despite the fact that the present paper does not provide any explicit procedure for finding solutions to the study, it nonetheless may prove “helpful in answering such questions.” It is at this point that Friedrichs acknowledges in a footnote that the investigations in the paper were carried out in connection with an ONR contract on gas dynamics. It is telling that Friedrichs published the first paper in the *Communications* and the second in the Society’s journal. Both studies arose out of studies of physical phenomena, but they nonetheless represented two strains in applied mathematics. Whereas in the first paper, the emphasis is placed on the physical phenomena under

⁶¹ Kurt O. Friedrichs, “Nonlinear Hyperbolic Differential Equations for Functions of Two Independent Variables,” *American Journal of Mathematics* 70, no. 3 (1948): 555.

⁶² *Ibid.*

investigation, in the second it is referred to briefly but is not described in any detail. The emphasis in the latter is on the mathematical techniques, not their possible interpretation. As such, it belonged to analysis rather than applied mathematics per se. The contrast between Friedrichs's two papers reveals the tensions that defined the growth of the field in the postwar period.

Applied mathematics, however, encompassed much more than the theory of fluid dynamics or elasticity. Some applied mathematicians like Friedrichs were able to publish some of their results in the top mathematical journals of the time alongside papers in abstract algebra and topology, but this was not necessarily the case when it came to other domains of research whose methodologies were even more incompatible with the philosophical tendencies of pure mathematics. This is evident in the report's description of the other "categories" comprising applied mathematics. "From a world in which understanding is its own reward, we are now stepping into an atmosphere where understanding is of value only if it *suggests actions* which *influence* and *control* our environment."⁶³ In this way the report introduced the fields of study subsumed under the category of "the analysis of engineering systems." Here pragmatic considerations were placed front and center.

The applied mathematician working in this domain, the report explained, might call upon the same mathematical theories in his work as would the mathematical physicist, but his "intent is different." Specifically, "philosophical incisiveness and inherent elegance become secondary to the *practical appropriateness* and manageability of proposed mathematical constructions."⁶⁴ Studies included under this category were not aimed at producing new theories mathematical or physical; rather, the aim was to use mathematical techniques to solve particular problems arising

⁶³ Weyl, *Final Report on A Survey of Training and Research in Applied Mathematics in the United States*, 20.

⁶⁴ *Ibid.*, 20–21.

out of a variety of engineering demands. Aeronautical, communications, and control systems engineering were highlighted as three of the main fields where research was currently being conducted. Not only was the emphasis placed on practical appropriateness, but all three fields were directly connected to the defense establishment.

The same trend was noticeable in other descriptions given in the report. In describing recent developments in the use of mathematical techniques to interpret sociological phenomena, the report, not surprisingly, mentions game theory. “A significant start,” it writes, “has been made in providing mathematical access to co-operative and competitive interactions by the axiomatic constructions of game theory.”⁶⁵ The development of game theory was singled out as unique in that it did not rely on already existing theories, but was invented *de novo*. “It constitutes the first major mathematical structure in the present context for which there is no physical antecedent.” Yet as the report indicates, game theory was “at present being pursued, still predominantly, but no longer exclusively, in relation to military applications.”⁶⁶ Even in the case of game theory, in which mathematicians were producing new theoretical knowledge, its close connection with military demands was undeniable.

Operations research, activity analysis, numerical simulation, and numerical analysis were all areas of research that were included in the mid-1950s under the wide umbrella of applied mathematics. In all of those new fields the importance put on obtaining direct practical results was inconsistent with a pre-war non-utilitarian conception of mathematics. This was not lost on the authors of the report. In a section on the place of applied mathematics in the university, the report acknowledged that there exists a polarity in opinions regarding the field’s objectives. One side maintained that the goal of research in the field was “the production of new mathematical

⁶⁵ Ibid., 25.

⁶⁶ Ibid.

ideas and procedures, arising from the study of theoretical problems suggested by an applied context.”⁶⁷ This vision for the field aligned it more closely with the objectives of pure mathematics in that it elevated theoretical studies over practical results. While it is hoped that such studies “will be of immediate help in the subject matter field which has suggested them, not much is lost if they are not,” the report explained. Yet a competing agenda for the field, which was more present-oriented, was also palpable among members of the applied mathematics community. According to that view, the aim of the field was “increasing in breadth and detail our immediate understanding of the facts in particular subject matter sciences.”⁶⁸ According to this view, the objectives were reversed. That the mathematics developed was original would be appreciated, but it was not essential.

Norman Levinson, an MIT mathematician, who like Friedrichs moved freely between pure and applied mathematics and who clearly adhered to the former view, was dismayed with the authors’ decision to promote such a wide conception of the field. In a letter to the secretary of the society, he complained that the report “represents the viewpoint of those who would like to encompass in one organization the many disciplines which make use of mathematics at whatever level and for whatever purpose.”⁶⁹ Why, asked Levinson, is the Operation Research Society mentioned in the report while the American Institute of Radio Engineers is not? Regardless of the usefulness an application of mathematics might have, unless it contains “new and interesting mathematics” it should not, argued Levinson, be sponsored by the society as the professional organization of mathematics. “The author of the report may be looking for pastures more verdant with the ‘long green’ than the traditional grazing land of the AMS but why does he want to drag

⁶⁷ Ibid.,35.

⁶⁸ Ibid.

⁶⁹ Norman Levinson to Edward Begie, 13 July 1954. AMSR, Box 40, Folder 1.

the rest of us along with him?”⁷⁰ Levinson’s sarcastic remarks pointed directly to the problematic nature of the development of research in the field, which almost a decade after the war was still nearly completely dependent on defense support.

The reliance on defense money not only promoted the development of more pragmatic aspects of applied mathematics, but just as significantly it set it apart institutionally and organizationally from pure mathematics. The different funding structure that came to mark research in applied mathematics only further enforced a separation between the two communities. Friedrichs and Levinson might have been able to produce similarly diverse research breaching the theoretical and the applied, but they worked in completely different environments. The Department of Mathematics at MIT and the Institute of Mathematical Science at NYU functioned according to different sets of rules, and most work in applied mathematics in the aftermath of World War II was produced in the latter. The decade following the war saw the emergence of several semiautonomous research institutions dedicated to research in applied mathematics. The Applied Mathematics and Statistics Laboratory at Stanford, the Institute of Fluid Dynamics and Applied Mathematics at the University of Maryland, the Institute of Mathematical Sciences at New York University, the Graduate Institute for Mathematics and Mechanics at Indiana University, and the Institute of Numerical Analysis at the University of California at Los Angeles were all postwar products. The report went so far as to declare that founding these separate institutions constituted “the most important post-war development for training and research in applied mathematics.”⁷¹

These institutions did not adhere to a unifying organizational scheme or research philosophy, but they did exhibit some commonalities. In many cases the institutions were either a

⁷⁰ Ibid.

⁷¹ Additional institutions included: The Graduate Institute for Mathematics and Mechanics at Indiana University, the Digital Computer Laboratory of the University of Illinois, and the Computer Laboratory of the Institute for Advanced Study at Princeton.

direct continuation of, or closely related to, work done by the Applied Mathematics Panel during the war. Either in terms of personnel involved or research being conducted, these institutions were products on the AMP. Further, most of them were established with direct support and encouragement from the Mathematics Branch at the Office of Naval Research, and they continued to be heavily dependent on contracts from various federal and military agencies. Work in these institutions could be further traced to the AMP in that organization of research followed the collaborative project-based approach that had characterized war research. In all of these features, these institutions differed greatly from the traditional department of mathematics.

The Applied Mathematics and Statistics Laboratory at Stanford was established in 1950 under the directorship of Albert Bowker. During the war, Bowker was part of the AMP's Statistical Research Group at Columbia University, an experience, he acknowledged, that had a major influence on his career. In 1949, Bowker was recruited to Stanford in order to establish a separate Department of Statistics, and in 1950 he pushed for the establishment of the Laboratory.⁷² Bowker modeled the organizational design of the Laboratory directly on the Statistical Research Group, and some of the early statistical work it produced, for example on sampling inspection, originated in war research. During the 1950s, the scope of projects expanded to include research in mathematical economics, mathematical techniques in the social sciences and classical applied mathematics, but the organizational pattern remained the same. Bowker explained that "the idea from the beginning was to construct a research laboratory with students and faculty working on problems, many of which would come from applied fields; to treat students as colleagues."⁷³ Bowker's remarks point to another feature that set these

⁷² Bowker noted in an interview later in life that when he arrived at Stanford "the mathematics department received me with a certain detachment." He added that Gabor Szëgo, who was the chairman at the time, "explained to me very nicely that while what I did was very interesting – it wasn't mathematics." Ingram Olkin, "A Conversation With Albert H. Bowker," *Statistical Science* 2, no. 4 (1987): 475.

⁷³ *Ibid.*, 476.

institutions apart from traditional departments: namely, close association between research and training. The institute was built around a core of senior faculty, with research assistants, visiting scholars, and doctoral students coming in and out. It therefore provided unique training opportunities for students, whose work was not chosen on an individual basis, but was part of a specific project in the laboratory.⁷⁴

These institutions, as their names indicate, usually had a specific strength in one or two fields of research, unlike the traditional mathematics department, which sought, at least in principle, to include representatives of as many mathematical branches as possible. That is, overlap in interest was encouraged rather than avoided. The Institute of Numerical Analysis at the University of California at Los Angeles, for example, was established in 1950 specifically to study mathematical theory and practice of computation. Funding for the Institute came from the Office of Naval Research and the National Bureau of Standards. Mina Rees, who was the driving force behind the establishment of the Institute, explained, “we felt that the chances of getting mathematicians actively interested in developing the needed mathematical insights would be greatly enhanced if there were a place where groups of mathematicians could be brought together to exchange ideas and stimulate one another and work with the computer and see what the interplay between mathematics and the computer would be.”⁷⁵ Some of the research that was produced in the institute was highly theoretical, but the focus on teamwork and the melding of theory and practice set it apart from the work conducted across the street at Stanford’s Department of Mathematics.

⁷⁴ In a 1991 interview, Bowker proclaimed that the Laboratory was a “multi-million dollar operation financed largely by the Office of Naval Research.” Albert H. Bowker, “*Sixth Chancellor, University of California, Berkeley, 1971-1980; Statistician, and National Leader in the Policies and Politics of Higher Education*,” an oral history conducted in 1991 by Harriet Nathan, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1995.

⁷⁵ “Henry Tropp interview of Mina Rees,” 14 September 1972, Computer Oral History Collection, 1969-1973, 1977, Archives Center, National Museum of American History.

The mathematics institutes founded after the war not only continued the war mobilization in terms of research topics and research organization, but also with regard to funding structure, which relied almost completely on federal contracts. Bowker went as far as describing the Stanford group as “a kind of a holding company for government projects.”⁷⁶ This characterization was probably a relatively accurate description of many of the other postwar mathematics institutions. The best respected of the mathematics institutions was undoubtedly the Institute of Mathematical Sciences at NYU (also referred to as Courant’s group). It was the earliest and largest of these postwar institutions and in some ways served as a model for the rest. Unlike the other mathematics institutions, the NYU group originated during the war. Already in October 1944 as he began planning toward demobilization, Warren Weaver wrote to Ward Davidson at the NDRC suggesting that the group’s work continue after the war. Noting the substantial research the group had produced for the Naval Bureau of Ordnance and the Naval Bureau of Aeronautics, Weaver wrote, “I see no reason why, assuming that the Service organizations mentioned are sufficiently interested, they could not negotiate a direct contract with New York University.”⁷⁷ In many ways, Weaver foresaw things to come.

The first contract grant Courant’s group signed after the war was with the Office of Naval Research for a study on “fluid dynamics and mathematical physics.” This initial contract extended more than a decade, and was supplemented over the next couple of years by additional contracts. For example, in 1951 the ONR signed another contract with the group for a study of the “formation of jets and their stability.” The ONR was not, however, the only military agency that began supporting the group’s research. The Army Department signed a contract in 1952 for a study of the “theory of subsonic and transonic fluid dynamics.” This reliance on outside contracts

⁷⁶ Olkin, “A Conversation With Albert H. Bowker,” 477.

⁷⁷ Warren Weaver to Ward Davidson, 23 October 1944, RAMP, Box 18, Folder “Demobilization.”

steadily increased in the decade following the war. By 1958 the Institute was conducting research for twelve separate agencies, from the Air Force and the National Security Agency to the National Science Foundation.⁷⁸ Its annual budget rose from approximately \$125,000 in 1946 to about \$2,500,000 in 1958.⁷⁹

The NYU institution was completely dependent on these outside contracts. It was not only used to support the research staff and graduate students, but even some of the salaries of the senior mathematicians in the institution were paid through contract money. This dependency on contract money set applied mathematicians apart from their peers in pure mathematics, who were supported more directly by their universities and were free to pursue research on their own accord. In 1953, commenting on the support structure for the institute, Courant exclaimed, “we feel that this support can be accepted with good conscience. We are trying to help our sponsors in a number of partly classified specific and important subjects. Before all, we are convinced that our existence and our work contributes to creating a reserve of competent people which would be prepared to help if and when an emergency should occur.” Yet he immediately added “such one-sided support obviously is not a healthy basis for our far-reaching objectives.”⁸⁰ In enumerating the future objectives of the institute, Courant explained that despite the fact that “in the present emergency” a fair amount of classified work is justified, the emphasis in the future should be on “*basic* research.”

However, by 1958 not much had changed. The Korean War had ended, but the Institute was still supported mainly through contracts with federal agencies and dissatisfaction was rising.

⁷⁸ See: “General Statement – Brief History,” RCP, Box 45 (new), Folder 7.

⁷⁹ Even after adjusting for inflation, the annual budget increased by more than a factor of ten. More than half of the institute budget bankrolled the installation of the UNIVAC at NYU, a project subsidized by the Atomic Energy Commission. Even if one ignores the cost of UNIVAC, the institute’s budget increased by more than a factor of five. *Ibid.*

⁸⁰ “Institute of Mathematical Sciences New York University: Address of R. Courant to meeting of Advisory Board of the Institute of Mathematical Sciences,” 13 May 1953, RCP, Box 22 (new), Folder 2.

In a memorandum on the state of the institute and its history, James Stoker, who succeeded Courant as the director of the institute, outlined some of the difficulties arising from the current financial arrangement. Stoker reported that “the largest” part of faculty members’ salaries were paid through contract funds while those of all other scientific personnel (51 Ph.D.s) and clerical workers were entirely paid through contracts. The problem arose, according to Stoker, when “a gap occurred in changing from one sponsored contract to another.” During these periods of transitions a fair amount of financial maneuvering was required in order to ensure the continued employment of the staff. “This situation,” Stoker added, “has produced uneasiness and irritation on the part of the senior scientists who have frequently felt under pressure to contribute more than seemed reasonable to contract projects in order to provide cover and assure support for the ‘transient’ on the contract, for students, and for the clerical salaries.”⁸¹ The reliance of contract money implied that the mathematicians at the institute were not always free to pursue research on their own accord.

This is not to say that the Courant group objected in any way to embarking on projects with specific military objectives. Stoker states so clearly in his memorandum. “We are not closed off to new ideas, or new enterprises; if for instance, some of our clients from the Department of Defense should ask us to undertake work in some area in which we have special competence, we would certainly try to meet their wishes.”⁸² Rather, what the group was after was more recognition from the university. It was hoped that at least the salaries of the senior staff would be completely covered by the university. In concluding his survey, Stoker explains that these financial considerations were not just necessary for securing the future of the institution, they also represented a “psychological factor.” By this he meant the desire of the senior staff to be more

⁸¹ “General Statement – Brief History,” RCP, Box 45 (new), Folder 7.

⁸² Ibid.

fully supported by the university. It was within the university, however, that applied mathematics failed to take hold.

Of course some research in applied mathematics did take place within traditional academic departments. The Division of Applied Mathematics at Brown University was a prime example. However, it was an outlier rather than the norm. The majority of research in applied mathematics was conducted in such semiautonomous institutions. The reliance on federal support was further exacerbated by the fact that these groups often worked with large-scale automatic computing devices. The Digital Computer Laboratory at the University of Illinois at Urbana-Champaign (established in 1951), similar to the numerical analysis group at UCLA, worked directly on the mathematics of computing, but some of the other institutions relied less directly on computation in their work.

The problems arising from these mathematics institutes' reliance on contracts with federal agencies was recognized in the committee's 1954 report. Reflecting on the characteristic support structure of the larger institutions, the report observes:

Difficulties tend to arise therefore, in maintaining the integrity and standard of the institute's research program under conditions where sizeable sums can readily be secured for work on outside assignments, while basic research support is hard to come by in the amounts which such an establishment requires. The resulting potential instability of our most effective applied mathematical centers is among the most disquieting consequences of the current situation.⁸³

⁸³ Weyl, *Final Report on a Survey of Training and Research in Applied Mathematics in the United States*, 36.

Applied mathematicians were well aware that their heavy reliance on federal support implied that they did not in any way possess full autonomy over their research direction.⁸⁴ According to the report, this situation made it even more critical to ensure that applied mathematicians are present in universities where they could continue to pursue basic research and train the next generation of applied mathematicians.

But, of course, here lay the catch. In the decade following the war, research institutes ensured that research in applied mathematics continue to flourish, but in doing so they set it apart institutionally, organizationally, and financially. The emphasis on teamwork, organized projects and outside support created a clear structural distinction between research in applied and pure mathematics.

Monastic

Weyl and the members of the Committee were not immune to the criticism they received. By the end of June, Weyl redrafted the controversial section in hopes of ameliorating some of the disagreement, but the committee voted against entirely discarding it. John Tukey insisted that the inclusion of the section in the report was necessary, and Edward McShane, who headed the Ballistics Research Laboratory at the Aberdeen Proving Ground during the war, noted in a letter to the Secretary of the AMS, “I did not feel, as some other members of the Council [of the AMS] did, that the report was iniquitous and the controversial section should be completely suppressed.”⁸⁵ Richard Courant acknowledged that having the committee unanimously accept the report would be preferable, but added that “it would not do any good to soft pedal what seems to

⁸⁴ In the analysis of the questionnaire Weyl sent to the various centers where applied mathematics research was conducted, the report notes that one of the main complaints voiced by respondents was “the latent pressure exerted by the sale value of certain fields as areas of project research which tends to stifle freedom and originality in the choice of research topics.” Ibid, 55.

⁸⁵ Edward McShane to E. G. Begle, 18 July 1954. AMS, Box 40, Folder 1.

many of us very important aspects of the situation.” Courant acknowledged that many “leading mathematicians” have done much to improve the status of applied mathematics, but concluded that at stake was not simply a question of control. “In our troubled era,” he wrote, “it has become of real significance in what way American mathematics is guided.”⁸⁶ The development of applied mathematics, so the reasoning went, was important in service of the state; Weyl’s draft merely indicated “some of the sociological obstacles to satisfying it.”⁸⁷

Six months earlier in a private letter to Weyl, Courant was even more forceful in his opinion, when he pleaded with Weyl to emphasize in his writing the sociological aspects impeding the growth of applied mathematics. “The rulers of mathematics as an activity in the academic world,”⁸⁸ Courant explained, tend to exclude applied aspects of mathematics from influence. A prime example of this trend was the small number of applied mathematicians among the officers of the AMS, and within the National Academy of Science. This exclusion, Courant explained, was not necessarily done consciously but rather was an outcome of a lack of appreciation for the current state of applied mathematics. Nonetheless, Courant reasoned, “vital interests, not only of the insignificant mathematical fraternity, but of the country, and I dare say of the free civilization” were at stake. Courant saw mathematics at the center of a broader fight over the nature of scientific education in the country, which he believed needed to be more technically inclined. Together with many other young American applied mathematicians that got their training during the war, Courant hoped that the new world order would make clear once and for all the importance of mathematical applications in the education of young mathematicians,

⁸⁶ Richard Courant to Members of the Committee of Research and Training in Applied Mathematics, 24 June 1954. RCP, Box 16 (old), Folder “NRC.”

⁸⁷ Ibid.

⁸⁸ Richard Courant to Joachim Weyl, 5 January 1954, RCP, Box 24 (old) Folder Weyl F.J.

but he was also well aware that he was working against the prevailing dogma to which most American mathematicians ascribed.

Whereas pure mathematicians, like Stone, were concerned that the postwar development of the field would be marked by an overemphasis on utilitarian goals, for Courant and other members of the committee the danger was exactly the opposite. What they warned against was the development of mathematics as a purely abstract study disconnected from the physical world. This concern was clearly stated in the report. Despite officially pertaining to the state of applied mathematics in the country, the report did offer a description of the current state of research in pure mathematics. “The axiomatic approach,” the final report declared, “has emancipated mathematics from its bound state in science. Now known not to reflect as a matter of inherent necessity the structure of the unique and particular world around us, it is recognized to do so only at a special effort.”⁸⁹ This freedom afforded to mathematicians by the axiomatic method had artistic qualities. “Much as a score evolves under a composer’s hand,” so is the mathematician now free to choose, change, and modify the axioms he wishes to study. “There is no longer a need to scan a shifting reflection in the pool of his mind for the features of an alien reality forever looking over his shoulder.”⁹⁰ The association of creativity with the separation of mathematics from science is telling, as it presupposes that mathematical development based on natural phenomena is inherently less creatively free – a view that the first draft of the report sought to overturn.⁹¹

⁸⁹ Weyl, *Final Report on A Survey of Training and Research in Applied Mathematics in the United States*, 12.

⁹⁰ *Ibid.*

⁹¹ Jamie Cohen-Cole argued that during the 1940s and 1950s, liberal intellectual espoused creativity, freedom, and autonomy as the necessary personality characteristic to ensure the health of a democratic society and avoid the perils of authoritarianism. For liberal intellectual, Cohen-Cole writes, “creativity was more than a personal attribute. It had social ramification. It was to be the very foundation of the pluralist society that social critics hoped to build.” For mathematicians, autonomy, creativity and freedom were also closely connected and the three became a repeated refrain in mathematical discourse during the period. To a certain degree one can see mathematician’s occupation with the ideal of creativity as a aligning themselves with the liberal intellectual Cohen-Cole describes. However, for mid-century mathematicians creativity and freedom were afforded by removing themselves from the world. It is the

This transformation in the evolution of the field, the report is careful to note, profoundly changed mathematical pursuits and broadened the mathematical landscape. Yet this “diversification” was accompanied by a tendency toward “specialization.” The trend seen in the work of the “abstract analysts,” “class field theoreticians,” and “algebraic topologists” of the day, the report explains, was of great danger to the development of mathematics. “As these specializations run out of material worth axiomatizing,” they ran the risk of deteriorating “mathematics to the level of manipulating esoterically arbitrary symbols.” While acknowledging the benefits inherent in the axiomatic approach, the report is careful in warning against its pitfalls. When he commented on an early draft of the report, Courant spelled out what he saw as the danger of the current trend in pure mathematics. Unless it was connected to physical reality, Courant wrote, “mathematics will dry out and become a pastime for a small group of more or less queer people who masquerade as high priests and promote each other as long as the public will believe and support them.” He then added, “I think we are definitely in that kind of danger and I consider the need that led to the present committee as particularly urgent.”⁹²

The axiomatic structural conception of mathematics, which characterized the growth of mathematics at the beginning of the twentieth century, has received much attention from historians of mathematics. Whereas the name David Hilbert has been closely associated with the turn toward axiomatization at the turn of the century, according to Leo Corry, a qualitative difference appeared between Hilbert’s conception of the axiomatic approach and its appropriation and further development in the United States. Specifically, Corry argues that at the turn of the century Eliakim H. Moore and his students at the University of Chicago “turned the analysis of

separation of mathematical theories from the physical world that afforded pure mathematicians a higher degree of autonomy, freedom, and creativity. As such, it reflects desire to stay within the safe confines of the university. By Jamie Cohen-Cole, “The Creative American: Cold War Salons, Social Science, and the Cure for Modern Society,” *Isis* 100, no. 2 (June 1, 2009), 236.

⁹² Courant, Richard.. “Remarks in Connection with F. J. Weyl’s Draft of Applied Mathematics Survey Report,” 25 May 1953, RCP, Box 24 (old), Folder “Weyl Report.”

systems of axioms into a field of intrinsic mathematical interest in which the requirements introduced by Hilbert oriented the research questions and afforded the main technical tools to deal with them.”⁹³ It is this turn toward “the analysis of systems of axioms” that became associated with the view of mathematics as the abstract study of structures. The subject matter under investigation was no longer rooted in physical reality but instead abstract mathematical objects.

This view is most famously associated with the work of the French Bourbaki group. As several historians have noted, for Bourbaki the axiomatic method was a way of building the whole edifice of mathematics on consistent grounds.⁹⁴ According to David Aubin, the reliance on structure was a means by which Bourbaki could “purify mathematics of any reliance on the external world.”⁹⁵ Although the Bourbaki group was based in France, the influence of its philosophy on the development of mathematics in the United States has long been noted.⁹⁶ André Weil, one of the six founding members of the group, came to the United States in 1941. It was Marshall Stone who in 1947, upon assuming chairmanship of the Department of Mathematics at the University of Chicago, recruited Weil as one of his first hires. Bourbaki’s popularity both in France and the United States during the 1950s was unmistakable. It is therefore not surprising that in detailing the current developments in the field the report centers on the axiomatic approach.

⁹³ Leo Corry, “Axiomatics Between Hilbert and The New Math: Diverging Views on Mathematical Research and Their Consequences on Education,” *International Journal for the History of Mathematics Education* 2, no. 2 (2007): 26.

⁹⁴ See, for example: Leo Corry, “Nicolas Bourbaki and the Concept of Mathematical Structure,” *Synthese* 92, no. 3 (1992): 315–348; Leo Corry, “The Origins of Eternal Truth in Modern Mathematics: Hilbert to Bourbaki and Beyond,” *Science in Context* 10, no. 2 (1997): 253–296; Liliane Beaulieu, “Bourbaki’s Art of Memory,” *Osiris* 14 (1999): 219–251.

⁹⁵ David Aubin, “The Withering Immortality of Nicolas Bourbaki: A Cultural Connector at the Confluence of Mathematics, Structuralism, and the Oulipo in France,” *Science in Context* 10, no. 02 (2008): 297–342.

⁹⁶ Amy Dahan-Dalmedico, “An Image Conflict in Mathematics After 1945,” in *Changing Images in Mathematics: From the French Revolution to the New Millennium*, ed. Umberto Bottazzini and Amy Dahan-Dalmedico (New York: Routledge, 2001), 320.

However, the discussion surrounding that part of the report calls attention to some distinctions between the French and the American conception of the axiomatic method. The involvement of American mathematicians in the war had lasting effects on the conception of applied mathematics in the postwar period, but pure mathematics was not left unaltered. Many mathematicians returned to their prewar university positions after the war, but they fully acknowledged the potential of mathematics in war research. Moreover, the postwar ideology of basic and applied research did actively impact mathematicians' conception of their field. I suggest that the discourse of basic and applied research was not just a rhetorical device pure mathematicians were able to pick off the shelf when necessary in order to justify their research to funding agencies. The social and political milieu within which mathematicians were working in the 1950s helped shape the way mathematicians conceived of the bounds and borders of pure mathematics.

In his letter to Leon Cohen, MacLane protested against the characterization of the axiomatic approach given in the report. The first point MacLane took issue with was the report's suggestion that axiomatization leads to specialization. "*Exactly the opposite is the case,*" he wrote, adding, "Bourbaki uses the axiomatic method to provide an integration of mathematics."⁹⁷ Besides Bourbaki, MacLane also came to the defense of the three groups of specialists the report singled out, pointing out, for example, that "the abstract analysts are concerned with the formulation of quantum mechanics and with the application of abstract methods to differential equations." MacLane's endorsement of the axiomatic approach went even further. Not only was axiomatization a way of unifying mathematical knowledge, but it was also, despite assumptions to the contrary, deeply embedded in classical problems. "All mathematicians," regardless of their approach, "deal with basic mathematical substance which is based on *fact* and on *physics* in

⁹⁷ Saunders MacLane to Leon Cohen, July 1954, MMP, Box 13, Folder "Stone."

exactly the same way as are the applications listed in the report. Axiomatization is *not* the free play of the imagination.”⁹⁸ The view advanced here by MacLane begins to point to how American mathematicians appropriated the axiomatic method in the postwar period. Mathematics (and mathematicians) continued to be grounded in the world.

In his minority report, Stone made a similar case. Following the report, Stone also offered a short survey of the development of mathematics from the beginning of the century. Stone begins by claiming that “the essential condition” for the growth of mathematics was the recognition that “mathematics is not closely bound by its ties to physical reality – if, indeed, it is bound at all.” This statement is almost identical to the one made in the report, but after he praises the benefits and robustness of the axiomatic approach, Stone makes clear that this does not mean that mathematics belongs purely to the world of thought.

Though concentration of one’s attention on the details might at first seem to indicate the contrary, pure mathematics does in fact continue to revolve about the great central problems of number theory, of geometry, and of analysis, which deal with matters fully as concrete as the abstractions of the atomic or nuclear physicists.... In truth, it has continued to draw inspiration from the oracle of nature, and has remained constantly aware of the role it plays in adding to the resources available to the applied mathematicians for the understanding of the world in which we live.⁹⁹

Thus, even for Stone, axiomatization was not opposed to application. On the contrary, by “freeing” mathematics and making it more diverse and powerful, mathematicians were now able to apply their tools to an ever-growing body of problems.

⁹⁸ *Ibid.* Emphasis in the original.

⁹⁹ Stone, Marshall H. “Committee on Training and Research in Applied Mathematics: Minority Report,” MMP, Box 13, Folder “Stone.”

Concluding his article on “The Future of Mathematics,” Weil also conjured an oracle, though his was not the oracle of nature, but rather the oracle of Bacchus (“divine bottle”) from François Rabelais’s sixteenth-century *The Life of Gargantua and of Pantagruel*. Weil announces that unlike Panurge, who went to the Oracle to look for worldly answers, the mathematician is happy not to ask such “indiscreet” questions. Others can “have recourse to the muddy streams of a sordid reality,” he writes. “If he reproached with the haughtiness of his attitude... he will answer, with Jacobi: For the honor of the human spirit.”¹⁰⁰ Whereas Weil advocated the study of mathematics as “pure” science for its own sake, MacLane and Stone promoted it as a form of basic research. For them it was the usefulness and applications of mathematics, rather than the advancement of the human spirit, that was hailed as the cause for mathematical development and for continued research in the field.

Both Stone and MacLane were actively pursuing defense-related projects during the war. Both men returned to their prewar research, which was mostly abstract at the war’s end. And both were in leadership positions between the mathematical community and the military and federal establishment. It is easy therefore to see their insistence on the applicability of the axiomatic approach as a rhetorical device, a way of ensuring that pure mathematicians would also benefit from the new bonds between scientists and the state. After all, both their opinions were voiced in relation to national reports regarding the funding of mathematical research. Undoubtedly this is partially the case.

Yet a less cynical reading is also possible. Perhaps the involvement of American mathematicians in war defense research and the growth of mathematics during the period actually resulted in a different interpretation of the axiomatic approach, which was distinct from the French one espoused by Bourbaki. Axiomatization might have been a way of “freeing”

¹⁰⁰ Weil, “The Future of Mathematics.”

mathematics from science, but it was not intended to sever all ties with it. In 1956, Stone was invited to give the annual Josiah Willard Gibbs Lecture at a meeting of the AMS in New York. The Gibbs lectures, which began in 1923, were traditionally dedicated to the applied aspects of mathematics, and were usually given by applied mathematicians and other scientists. Entitled “Mathematics and the Future of Science,” Stone’s lecture was almost a verbatim recounting of his minority report.¹⁰¹ It is revealing that from a podium where he was to speak about mathematics and its applications, Stone chose to deliver his call for what he conceived of as a non-utilitarian development of applied mathematics. It also reflects one key difference between Bourbaki’s view of the relation of pure to applied mathematics and the one that was coming into existence in the postwar period.

In the introduction to the talk, which was (needless to say) different from the chronicles of the committee’s affairs that appeared in the report, Stone made clear that he did not belong to the group of mathematicians that despaired upon seeing their abstract theories put to practical use. Noting the importance of mathematical ways of thinking to the development of science, Stone went even further, putting the burden of integrating the two fields upon mathematicians. “However good the mouse-traps we invent,” Stone explained, “the world may be very slow to realize its need of them.” It is therefore the “task of mathematicians” to attract “the world’s attention to our stock of mouse-traps and [mark] out some of the approaches to it.”¹⁰² Stone’s call to arms is markedly different than the attitude voiced by Bourbaki. In 1950, Dieudonné, another founding member of the group, published “The Architecture of Mathematics” under Bourbaki’s name. The article, which has been described by Corry as Bourbaki’s manifesto, outlines the group’s structural-axiomatic conception of mathematics. In the conclusion of the article,

¹⁰¹ Marshall H. Stone, “Mathematics and the Future of Science,” *Bulletin of the American Mathematical Society* 63, no. 2 (1957): 61–76.

¹⁰² *Ibid.*

Dieudonné acknowledges that there exists “an intimate connection between experimental phenomena and mathematical structure,” yet contemplation of this relation is the work of the philosopher, not the mathematician. “We are completely ignorant as to the underlying reasons for this fact... and we shall perhaps always remain ignorant of them.”¹⁰³ Thus, while Stone called upon mathematicians to actively seek ways in which their theories might be applied, this was far from the case for Bourbaki. American pure mathematicians were operating in a different world.

By appropriating the ideology of basic research, mathematicians like Stone and MacLane were able to ensure that pure mathematics continued to receive federal support. Axiomatization was not the enemy. Rather, according to them, abstract and generalized mathematical research was the only path to secure unforeseen discoveries and applications.

* * *

The final report, including Stone’s minority report, was ready to be submitted by the end of July. Yet, because Albert intervened, the date was postponed to allow the committee to arrive at a unanimous and less controversial report. In September, the International Conference of Mathematicians met in Amsterdam. Several members of the committee and the Division of Mathematics were in attendance and a meeting was called. In December, Albert reported that during the September Congress in Amsterdam he “took the vital steps necessary to settle a very nasty dispute between M.H. Stone, representing the interests of pure mathematicians, and F.J. Weyl, J. Tukey, and S.S. Wilks, representing the interests of applied mathematics.”¹⁰⁴ By October,

¹⁰³ Nicholas Bourbaki, “The Architecture of Mathematics,” *The American Mathematical Monthly* 57, no. 4 (1950): 221–232.

¹⁰⁴ “Current Activities of A.A. Albert,” December 1954, AAAP, Box 1, Folder “A.A. Albert – Personal.”

the committee agreed on a revised version of the final report. The controversial section was completely rewritten and Stone withdrew his minority report.¹⁰⁵

¹⁰⁵ The section as it appears in the final official report of the committee does not mention the supposed hierarchy between pure and applied mathematics, and no mention is made of the relative creativity of each field. Instead, the section begins by a general assessment of the importance of mathematics to “the public welfare,” and gives an account of the two major bodies representing the needs of mathematics on the national level.

Chapter 3.

Neither-Nor: Making The Applied Mathematician

George C. McVittie: All we really have talked about so far is: what mathematics should we teach engineers? But what about the mathematics which is used and needed in the further interpretation of the world around us?

John W. Tukey: What is at stake here is the concept of the mathematical consultant. The statisticians have recognized this branch of their business for some time. This is different brand of applied mathematician than the one which Mr. McVittie has in mind.

George C. McVittie: That is right. I am concerned with the theoretical physicists, astronomers, or aerodynamicists, not with the man who is sitting in an office to do sums for the engineers.

John W. Tukey: Perhaps we should go a step beyond the terminology of Joe Weyl who would have us refer to secular, rather than applied, mathematics to distinguish it from the pure, monastic variety – and speak of secular and monastic applied mathematics!

Otis E. Lancaster: I should like to return to Mr. Tukey's remark. The mathematical community will have to realize sooner or later that the mathematical consultant is here to stay and should give him the esteem and the training he deserves.¹

On Thursday afternoon October 22, 1953 a discussion erupted among members of the audience as the session on “Mathematics in the Integrated School of Applied Science” came to a close. It was the second panel in a three-day Conference on Training in Applied Mathematics at Columbia University sponsored by the American Mathematical Society and the National Research Council. Members of the audience just finished listening to three talks by Jerome Wiesner, an electrical engineer and future president of MIT, Howard W. Emmons, a Harvard

¹ Fritz Joachim Weyl, ed., *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council* (National Academies, 1953), 29.

mechanical engineer, and Abraham H. Taub, a mathematical physicist, who was at the time building a computer based on von Neumann's design at the University of Illinois.² During his talk, Emmons noted that it becomes "increasingly essential" for the applied mathematician to know engineering and relevant basic science, "so that he can *talk shop* with the engineers and mechanics."³ Following Emmons, Taub described the Control System Laboratory at Illinois as a "consumer of mathematicians," explaining that it "uses them [mathematicians] to formulate problems."⁴ It is no surprise then that as he sat there listening to these three speakers, George McVitte, who received his PhD from Cambridge University more than twenty years earlier under the supervision of Eddington, found himself wondering out loud, "What about the mathematics which is used and needed in the further interpretation of the world around us?"

The emphasis on engineering mathematics that dominated all three afternoon talks and to which McVittie responded with fervor is indicative of the changing conception of applied mathematics brought on by mathematicians' involvement in war-related research. Whereas before the war there were hardly any trained mathematicians working in industrial or governmental positions, by 1950 the demand for mathematicians (mostly in government agencies and select industries) was slowly beginning to rise. The aim of the New York conference, therefore, was to generate discussion regarding the "important factors which pattern the development of applied mathematicians *in juxtaposition* with the considerations which are decisive for their employment."⁵ Yet, as evident in the short exchange quoted above, it was not

² The computer, the ORDVAC, was completed a year earlier and delivered to Aberdeen Proving Grounds.

³ Howard Emmons, "Applied Mathematics in a Division of Applied Science," in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 26.

⁴ Abraham H. Taub, "Two Examples of University Laboratories," in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 28.

⁵ Joachim Weyl to Warren Weaver, 15 June 1953, RCP, Box 24 (old), Folder "Weaver." Emphasis added.

quite clear what was the presumed role of the applied mathematician in government and industrial organizations. Was he a man of service providing mathematical support to engineers and physical scientists? Or was he, as Tukey suggested in his reply to McVittie, a consultant -- a new "brand" of applied mathematician?⁶ Finally, what about the applied mathematician McVittie described? The mathematical physicist, who was interested in theoretical interpretation of the world around him?

Mathematicians, as noted in the previous two chapters, were unable to define what constituted applied mathematics in the decade following World War II. They were, nonetheless, eager to determine who was the applied mathematician. In the process, the applied mathematician was constructed not in terms of a coherent body of knowledge, but rather in terms of the personality traits that distinguished him from the pure mathematician. Training applied mathematicians, it was well agreed, was not so much about introducing students to specific subjects as it was about imbuing them with a particular attitude toward mathematical research. Instead of mathematical techniques, particular skills, or specific practices, the applied mathematician was defined in terms of his personae.

A robust historiography on scientific education had called attention to the fundamental role pedagogy plays in disciplinary formation.⁷ While some scholars demonstrated how local pedagogical tools and procedures shape the emergence of distinct research schools and scientific personae, others have illustrated the ways by which scientific training binds dispersed

⁶ Throughout the chapter I use "he" to refer to the applied mathematician. In doing so I follow the sources. This is not to say that there were not any mathematically trained women working in industry at the time. However, the applied mathematician was almost always assumed to be a man.

⁷ For foundational work, see: Ludwik Fleck, *Genesis and Development of a Scientific Fact* (University of Chicago Press, 1981); Pierre Bourdieu and Jean Claude Passeron, *Reproduction in Education, Society and Culture* (SAGE, 1990); Thomas S. Kuhn, *The Structure of Scientific Revolutions* (University of Chicago Press, 1996).

communities around shared sets of practices.⁸ Looking at the development of applied mathematics in the postwar period, this chapter turns to pedagogy as a way of examining the co-construction of professional identities and disciplinary boundaries.⁹ Because applied mathematics lacked any clear intellectual boundaries, pedagogy came to play an especially important role in the institutionalization of the field. As the constitution of applied mathematics was continuously shifting, mathematicians were unable to point to a standard body of knowledge, a subject matter with identifiable methods, which they could label applied mathematics. Thus, training became a way of defining, not the field itself, but a body of practitioners that laid claim to its development. Developing applied mathematics in mid-century America implied producing a body of mathematicians centered not around shared practices, skills, or subject matter, but rather on a shared set of norms and values.

⁸ Sharon Traweck, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Harvard University Press, 1988); Kathryn Mary Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (Ithaca, NY: Cornell University Press, 1991); Kathryn Mary Olesko, "Tacit Knowledge and School Formation," *Osiris* 8 (1993): 16–29; Andrew Warwick, "Exercising the Student Body: Mathematics and Athleticism in Victorian Cambridge," in *Science Incarnate: Historical Embodiments of Natural Knowledge*, ed. Christopher Lawrence and Steven Shapin (Chicago, IL: University of Chicago Press, 1998), 288–326; Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago, IL: University of Chicago Press, 2003); Karen Hunger Parshall, "Defining a Mathematical Research School: The Case of Algebra at The University of Chicago, 1892–1945," *Historia Mathematica* 31, no. 3 (2004): 263–278; David E. Rowe, "Making Mathematics in an Oral Culture: Göttingen in the Era of Klein and Hilbert," *Science in Context* 17, no. 1–2 (2004): 85–129; David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago, IL: University Of Chicago Press, 2005); David Kaiser, ed., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge, MA: MIT Press, 2005); Natasha Myers, "Pedagogy and Performativity: Rendering Laboratory Lives in the Documentary Naturally Obsessed: The Making of a Scientist," *Isis* 101, no. 4 (2010): 817–828; Suman Seth, *Crafting the Quantum: Arnold Sommerfeld and the Practice of Theory, 1890–1926* (Cambridge, MA: MIT Press, 2010); Alma Steingart, "A Group Theory of Group Theory: Collaborative Mathematics and the 'Uninvention' of a 1000-page Proof," *Social Studies of Science* 42, no. 2 (April 1, 2012): 185–213.

⁹ The role the mathematical seminar in nineteenth century Germany in the establishment of the pure mathematician as a unique professional identity has already noted by scholars. See: Mehrtens Herbert, "T. S. Kuhn's Theories and Mathematics: A Discussion Paper on the 'New Historiography' of Mathematics," *Historia Mathematica* 3, no. 3 (August 1976): 297–320; Gert Schubring, "The Conception of Mathematics as an Instrument in The Professionalization of Mathematics," in *Social History of Nineteenth Century Mathematics*, ed. H. J. M. Bos, Ivo Schneider, and Herbert Mehrtens (Boston, MA: Birkhäuser, 1981), 111–134; Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelminian Germany* (American Philosophical Society, 1983); David E. Rowe, "Klein, Hilbert, and the Göttingen Mathematical Tradition," *Osiris* 5 (January 1, 1989): 186–213; Ronald Calinger, "The Mathematics Seminar at the University of Berlin: Origins, Founding, and the Kummer-Weierstrass Years," in *Vita Mathematica: Historical Research And Integration With Teaching*, ed. Ronald Calinger, 1996, 153–176.

In the introduction to *Pedagogy and The Practice of Science*, David Kaiser writes, “pedagogy is where the intellectual rubber meets the politico-cultural road.”¹⁰ The growth of applied mathematics in the postwar period clearly demonstrates that pedagogy is deeply entrenched in broader forces operating outside the classroom. In contrast to a PhD in pure mathematics, an advanced degree in applied mathematics was conceived of as a preparation for non-academic employment. The institutionalization of the division between pure and applied mathematics certainly effected changes in how professors taught mathematics; so too did pedagogical changes in turn shape how new generations of mathematicians conceived of the epistemological limits of their respective subfields. As the language of supply and demand came to dominate the discourse surrounding the field, regimes of training and professional opportunities began to be established in relation to one another.

I begin the chapter by describing the development of the first two advanced training programs in applied mathematics established during the war at Brown University and New York University. The roots of these two programs in the war and their close connections with the defense establishment, points to the early articulation of training the field in terms of service. The goal-oriented growth of the field continued to dominate pedagogical discussions throughout the 1950s. These two early programs, however, already denote the different philosophies that stimulated training in the field. Whereas the Brown group was directed more closely to engineering and industry, the group at NYU was aligned with the sciences and basic research. Next I describe the Conference on Training in Applied Mathematics that took place at Columbia University in 1953. I first focus on the way participants parsed the job of the applied mathematician in industry as distinct from that of the engineer. The applied mathematician was

¹⁰ Kaiser, *Pedagogy and the Practice of Science*.

first and foremost a mathematician. He had a theoretical proclivity and his most essential qualification was his ability to abstract and formulate in mathematical terms problems arising out of specific physical systems. However, his proclivity to think in abstract terms also made him ill-suited for industrial work. In the fourth section of the chapter, I show how in direct response to industrial demands, the applied mathematician was being fashioned in terms of his persona.

Here, it was not the engineer or the scientist that the applied mathematician was to be distinguished from, but rather the pure mathematician. Over time, it was his personality qualification more than anything else that set to distinguish the applied from the pure mathematician.

“Making Stronger Cans”

In January 10, 1942, F.C.W. Olson of the Research Department of the American Can Company in Maywood, Illinois wrote a letter to Brown University mathematician, Dean Richardson. Olson wanted to thank Richardson for the wonderful experience he had taking part in the summer session in Applied Mechanics offered at Brown University the previous summer. Olson sat down on a number of occasions to write down his letter, but time and again he found his letter to be so overly glowing that he was worried the reader might question his sincerity. Doubting that he had finally “mastered the art of praise,” he decided to nonetheless send the letter.¹¹ The courses that were offered, he explained, were fantastic, and the men who taught them could not imaginably be more qualified. Moreover, everyone involved from the Dean to the secretaries “seemed to be filled with a driving spirit” and the feeling that they “were working for something new,

¹¹ “Extract from a letter from F. C. W. Olson of the Research Department of the American Can Company, Maywood, ILL. Under date of January 10 1942,” MMP, Box 9, Folder “Dunham Jackson.”

something worthwhile.”¹² Olson’s goal, however, was not just to provide general praise for the program. He also wanted to tell Richardson how he had already put into use the knowledge he learned during the summer.

“As you may perhaps remember,” he wrote, “my principal task at Brown was to learn something that may lead to making stronger cans.” He explained to Richardson that when he arrived at Providence, he was naïve to think that all his problems would be solved by the end of the summer. However, soon upon his arrival, he understood the mathematical complexity presented by his problem. It quickly became clear to him, he wrote, that from the point of view of the theory of elasticity, a tin can is a more complicated structure than a battle-ship.¹³ Still, his course in elasticity had not gone to waste. Back at Maywood, the fundamentals that he learned during the course led him to “consider the problem of the buckling of can ends with some understanding of the factors involved.” After examining some idealized situations from a theoretical point of view, he suggested some simple experiments. These were performed and “the results were nothing less than spectacular.” The group, Olson happily reported, “may be able to affect considerable savings of tin plate as a result.”¹⁴ Olson was able to apply the knowledge he learned at Brown to make concrete changes in the production process at the American Can Company. He was not interested in advancing the mathematical theory of elasticity, nor in producing a comprehensive study of can manufacturing. Rather, it was immediate improvements in the production process of the company that he was after.

Olson was one of fifty-five students who attended Brown University’s first Summer School in Applied Mechanics in 1941. The summer session consisted of three separate courses.

¹² Ibid.

¹³ Elasticity studies the behavior of materials that return to their official shape after they are deformed.

¹⁴ Ibid.

In addition to the course on elasticity that Olson attended, the program included a course on the theory of fluid dynamics as applicable in aerodynamics and hydrodynamics, and a course on partial differential equations. The goal of the latter was to introduce students to some of the basic tools of physicists and engineers as applied to problems in potential theory, heat conduction, and wave propagation. All three subjects were basic to engineering considerations of design and structure and, hence, were of direct relevance to problems of ship, submarine, and plane construction, and ballistics. This was the first advanced training program in applied mathematics to be offered in the United States.

Brown's program was pioneering and depended on the zealot efforts of Dean Richardson. Richardson received his PhD from Yale University in 1906 at the relatively older age of twenty-eight. He was hired by Brown University immediately upon his graduation, but he assumed the position only after he spent a year at the mathematical seminar at Göttingen. In the mid-1920s, Richardson became the Secretary of the American Mathematical Society, a position that he held for twenty years. He was involved in almost all of the society's daily operations and was, therefore, well familiar with all the leaders of the American mathematical community in the prewar era. In addition to his active involvement in the AMS, Richardson also became an influential figure within Brown University itself. In 1926, he became the Dean of the "Graduate Department," which a year later became the Graduate School.¹⁵ It is this unique position of Richardson both within the university administration and the mathematical community that

¹⁵ Raymond Clare Archibald, "R. G. D. Richardson, 1878 - 1949," *Bulletin of the American Mathematical Society* 56 (1950): 256-265.

enabled him in 1941 to bring forth the establishment of the first Summer School in Applied Mathematics at Brown University.¹⁶

In March 1941, Richardson together with the president of Brown University, Henry Wriston, circulated a memorandum calling for the establishment of a summer school in applied mechanics.¹⁷ The proposal exclaimed, that while the United States is easily recognized as a leader in pure mathematics and has developed relative strength in fields such as statistics and mathematical physics, “it lags far behind Germany and many other European countries in applied mechanics.”¹⁸ The war, the memorandum continued, “intensified the need for remedying America’s inadequacies in industrial mathematics.” Mathematicians had an invaluable role to play in a host of industries related to defense activities, Richardson and Wriston proclaimed. Yet they noted that at present there was not any place where “men can obtain a broad training in the advanced reaches of mathematics applied to engineering.” Moreover, the few experts in the applied mechanics who were living in the United States were “so widely scattered that their work is relatively ineffective for instruction purpose.”¹⁹ It was this state of affairs that the Brown proposal sought to amend. By drawing upon experts from around the country and focusing on the application of mathematics to engineering problems, the program hoped to strengthen the national effort in the field.

¹⁶ For a more detailed account of the development of the Brown Division in Applied Mathematics and Richardson’s influential role in it, see: Clare Kim, “Math Derived, Math Applied: The Establishment of Brown University’s Division of Applied Mathematics, 1940-1946” (Senior Thesis, Brown University, 2011).

¹⁷ There are two versions of this proposal, one dated March 17, 1941 and another dated March 1941. The one dated March, 1941 must be an earlier version as the memorandum proposes the establishment of a summer school in applied mathematics and not applied mechanics as it was officially known. While the earlier proposal is signed by Richardson alone, the later one is also signed by Henry Wriston, who was at the time the President of Brown University. The change of title is somewhat revealing, as the Brown program was indeed focused, at least in its early years, on applied mechanics, which represented only one aspect of applied mathematics. There are several differences between the two proposals and while I use the official memorandum for most of the following discussion, I will clearly indicate when I use the earlier version.

¹⁸ Richardson, R. G. D. and Henry M. Wriston. “Memorandum Concerning The Establishment of Courses in Applied Mechanics at Brown University,” 17 March 1941, RCP, Box 19(old), Folder “Richardson.”

¹⁹ Ibid.

From the initial planning, the summer session was to be succeeded by a year-long expanded program in the field. The program at Brown University was seen as more than a local experiment. Not only did Richardson bring in experts from various institutions to teach courses during the summer, but he also established a committee of leading mathematicians and scientists to evaluate the program and make future recommendations pertaining to training in the field in general and the Brown program in specific. The committee consisted of Marston Morse, Warren Weaver, Theodore von Kármán, Mervin Kelly, and George Pegram. Both Kelly and Pegram were physicists by training. Kelly was the director of research at Bell Laboratories, and Pegram, who was the chair of the Department of Physics at Columbia University, was closely involved in the Manhattan Project. Von Kármán, a specialist in aerodynamics, was one of the most famous applied mathematicians who immigrated to the United States in the 1930s. He settled in California, where he assumed chairmanship of the Aeronautical Laboratory at the California Institute of Technology.²⁰ Morse, who was at the time the president of the AMS, was to represent the professional mathematical community. That Morse was the only pure mathematician on the committee was no accident. In establishing the program at Brown, Richardson wished to align his efforts with scientists and engineers, not pure mathematicians.

In November 1941, the committee submitted its final report. It consisted of two parts; a general discussion concerning the need for trained applied mathematicians and a detailed evaluation of the Brown program.²¹ The report begins by acknowledging the restricted sense by

²⁰ John Greenberg and Judith Goodstein, "Theodore von Kármán and Applied Mathematics in America," in *A Century of Mathematics in America, Part II*, ed. Peter L. Duren et al. (American Mathematical Society, 1989), 467–477.

²¹ In their report on the Brown experiment, the evaluating committee dedicated a considerable discussion to the possible institutional structure that would be most effective for training applied mathematicians. Besides the possibility of providing short-term courses at selected institutions, the members of the committee identified four possible institutional configurations: (1) an institution of applied mathematics established separate from an existing university; (2) a school of applied mathematics established in chosen universities and technical schools; (3) an

which the committee utilizes the term “applied mathematics.” Namely, the committee explained that it was concerned “with those branches of applied mathematics which employ the advanced techniques of mathematics to solve problems of engineering and industry, and which seek the mathematical principles underlying these applications.”²² From the start the committee restricted its recommendation to mathematical research that arose out of industrial problems, and aligned research in the field with the engineering profession. The report notes that although engineers in industry have recognized the need for more advanced mathematical training and made attempts to draw attention to the field, all these efforts were prompted by engineers and physicists, not by men trained primarily in mathematics. What was necessary, according to the report, was a “sufficient body of mathematicians or ‘mathematical engineers’ to mediate between mathematics and engineering.”²³

Whereas physicists and engineers who have “problems to solve” would undoubtedly gain from such training, the program at Brown, the report argued, should be targeted at mathematically trained individuals. Besides industrial mathematicians, the most significant contribution of such advanced training program, the committee reasoned, would be to train

interdepartmental program that could confer degrees in applied mathematics; and (4) an institute for applied mathematics not based in any specific location. The first and fourth options were quickly dismissed. An isolated institution would be too costly in both money and men and would find it difficult to attract students. A dispersed institution would not only be an administrative hell, but would also likely to lead to uneven results. The third option could be productive, but the committee reasoned that training would tend to “be more practical in an immediate sense but in a long-term sense less fundamental.” It therefore was agreed that at least for now the second option was most desirable. “The existence of such a school as a *separate* unit in the university is important for stressing the unity of approach which underlies applied mathematics.” Yet here again the committee stressed the importance of training teachers and a limited number of industrial workers. When the members of the committee were deliberating, there was not even one single program in the United States dedicated to advanced training in applied mathematics, but their discussion in effect foresaw many of the obstacles that would arise in attempting to build training programs in the field in the two decades after the war. How to integrate applied mathematics to already existing institutional framing remained a vexing problem throughout the 1950s. “A Report on Advanced Training in Applied Mathematics With Special Reference to The School of Mechanics at Brown University,” November, 1941. Tukey’s Papers, Series I. Correspondence, Box 5, Folder “Brown University.”

²² “A Report on Advanced Training in Applied Mathematics With Special Reference to The School of Mechanics at Brown University,” November 1941. JWTP, Series I. Correspondence, Box 5, Folder “Brown University”.

²³ Ibid.

mathematics teachers “who are to train engineers and physicists” and who “should themselves be trained to be more effective teachers of applied mathematics.”²⁴ The group blatantly missing from the committee’s analysis is that of researchers in applied mathematics who would continue to produce original research in applied mathematics. Training mathematicians like von Kármán, whose theoretical insights and practical outlook placed him squarely both in the academic and industrial world, was not, at least according to the evaluating committee, one of the program’s goals. In part this analysis was based on the student composition of the first summer seminar. Of the fifty-five students in attendance, eleven were already employed in industry, and twenty-six were teachers (five of whom taught mechanical engineering, while the rest taught mathematics to engineering students). The rest were graduate students in mathematics, physics, and engineering. Still, when they were asked how they hoped to put their training into use, forty of those in attendance replied they wished to continue research in industry or government.²⁵

In the summer of 1943, after the program at Brown was running successfully for two years, Richardson published an article in the *American Mathematical Monthly* describing the program’s objectives.²⁶ The article was almost a direct reproduction of Richardson’s original proposal and the committee’s evaluation report. It also provided updated statistics on the placements of students who went through the program: out of two hundred, more than half (a hundred and fifteen) continued as instructors or graduate students in academia, while the rest

²⁴ Ibid.

²⁵ “Statistics of Students in Summer Session in Mechanics,” 9 June 1941, RCP, Box 19 (old), Folder “Richardson”.

²⁶ Richardson reiterates that the “primary object” of the program was training teachers of applied mathematics. He adds that a “subsidiary aim” was training mathematicians for positions in industry and governmental agencies. However, unlike the original evaluation report, Richardson adds that “from this group a smaller number can be expected to extend the frontiers of knowledge by new developments arising out of the practical problems on which they work.” Roland GD Richardson, “Applied Mathematics and the Present Crisis,” *The American Mathematical Monthly* 50, no. 7 (1943): 415–423.

found employment across government and industry.²⁷ The Brown University program, at least during its first few years, was targeted at producing mathematics teachers (often for engineers rather than for mathematicians) and mathematical engineers. In the 1950s, at least some mathematicians rejected this close association to engineering practices. Mathematical engineers were engineers. Applied mathematicians, they argued, should be first and foremost mathematicians.

Napoleon's Army

During August 1941, Richard Courant received letters from industrial researchers working at companies such as Socony-Vacuum Oil Co. Inc, Curtiss-Wright Corporation, and the United States Rubber Company, thanking him for the courses offered at New York University during the summer.²⁸ None of the letters was as glowing as Olson's letter to Richardson, but all men expressed satisfaction with the courses they attended. M. G. Marrison from the Propeller Division at Curtiss-Wright Corporation, who attended Courant's course on Advanced Methods of Applied Mathematics, was pleased to report that "a great deal of success has been experienced in using the methods" presented in the course.²⁹ G. E. Bulloch of Bell Labs, who attended a course in transient analysis of electrical networks, explained that the course "not only increases my professional equipment but also my potential ability to serve the Company with which I am affiliated."³⁰ The courses were provided under the auspices of the Engineering Defense Training

²⁷ 40 went into government agencies concerned with aeronautics, ship construction, gun construction, radar, etc.; 20 went into war related industry; 10 assumed positions within the military; and 25 were employed as engineers in industry.

²⁸ Letters thanking Courant for the courses offered at NYU came from industrial workers at RCA Manufacturing Company, the Propeller Division at Curtiss-Wright Corporation, Bell Labs, United States Rubber Company, and Socony-Vacuum Oil Co. Inc. For a collection of these letters see: MMP, Box 3, Folder "Courant."

²⁹ M. G. Marrison to Richard Courant, 8 August 1941, MMP, Box 3, Folder "Courant."

³⁰ G. E. Bulloch to Richard Courant, 11 August 1941, MMP, Box 3, Folder "Courant."

Program of the U.S. Office of Education, and were given under the College of Engineering at the University. As the letters quoted above indicate, these courses, like the ones given at Brown, were targeted at industrial engineers.

In July, Courant circulated a memorandum calling for the establishment of an “emergency institute for advanced training in basic and applied sciences.” The memorandum opens by calling attention to the importance of securing “a steady supply of young men of the highest ability in pure and applied sciences.”³¹ Demand for such training was evident, according to Courant, from the positive reactions to the recent courses in applied mathematics given at NYU. Despite calling direct attention to the recent program offered at the College of Engineering, Courant made it clear that the institute he wished to establish was motivated by different objectives. A more systematic program was required with broader basis and greater intensity, offered to a “more select group.” Courant’s aspiration was not to train the casual industrial worker, who required additional training in mathematics, nor was it to produce more informed mathematical teachers. Rather, he wanted to “train a very small group of men with the highest qualifications.”³² The level of courses, which would include applied mathematics, mechanics, physics, as well as other related fields such as acoustics and optics, were meant to be above the level of a graduate work course. Students would work closely with the faculty, which would consist of “scientists of high rank,” and additional full-time younger assistants.³³ An intensive year of training, Courant reasoned, would produce highly qualified individuals who would be able to apply their training to numerous problems arising out of the present situation.

³¹ Courant, Richard. “Memorandum Concerning an Emergency Institute for Advanced Training in Basic and Applied Sciences.” 18 July 1941, MMP (106.10), Box 3, Folder “Courant.”

³² Ibid.

³³ Courant does make the provision that some research engineers would be “admitted as part-time members to individual courses and seminars.” He also suggests that the institute would provide consulting services, but he is careful to note that such a service would not come from the “false promises” of the “immediate effectiveness of such advice for specific engineering problems.” Ibid.

Courant's initial ambition was even greater. An earlier and much longer memorandum called for the establishment of a National Institute for Advanced Instruction in Basic and Applied Sciences. "The most decisive event in the history of modern higher education was the outcome of a national emergency," Courant began his manifesto.³⁴ The event Courant was referring to was the establishment during the French Revolution of the *École Polytechnique*. Courant explained that the Polytechnique was democratic, though it was "restricted to a small group of selected students." It fulfilled all expectations and its students achieved influential positions in the military and in industry. Even Napoleon's staff included "many polytechnicians." The key for the success of the *École Polytechnique* was, according to him, an "uncompromising insistence" on the highest standards and the close connection between research and training. It was this model that Courant wished to now import to the United States. The educational problems of the country, he emphasized, could not "be solved by an exclusive reliance on the method of mass-production."³⁵ What Courant wanted to establish was a new technical scientific elite.

In 1937, two young mathematicians, Kurt O. Friedrich and James Stoker, joined Courant at NYU. For the next two decades the three men served as the core of the mathematical group at NYU. They collaborated on numerous projects, wrote papers and books together, and trained an entire generation of American applied mathematicians. Courant supervised twenty-two students after his arrival at the NYU, Friedrich surpassed him by training thirty-six, and Stoker managed to exceed Friedrich by one. Friedrich received his PhD in 1927 at Göttingen under the supervision of Courant. On Courant's advice, he took a position in 1929 as an assistant to von Kármán, just a year before the latter immigrated to the United States. Whereas Friedrich himself

³⁴ Courant, Richard. "Memorandum Concerning A National Institute for Advanced Instruction in Basic and Applied Sciences," undated, RCP, Box 39 (new), Folder 16. The reason I state that this memorandum must have preceded the one quoted above, despite the fact that it is undated, is because the July 18 memorandum states that for "historical parallels" a reference is made to an earlier and more detailed memorandum.

³⁵ *Ibid.*

was not affected by Hitler's ascendance to power, in 1933 he fell in love with Nellie Bruell. Bruell was Jewish, and by 1935 Friedrich began devising plans to leave Germany. With Courant's help Friedrich arrived in New York in 1937 and secured a temporary job at NYU. Soon upon his arrival, Nellie joined him and the couple officially married. Friedrich was an expert in elasticity theory, mathematical physics, and several other aspects of applied mathematics.³⁶

Stoker was born in Pittsburgh, Pennsylvania and began his education as a mining engineer at Carnegie Institute of Technology. Upon receiving his Masters degree, Stoker moved to Zurich to continue his doctoral studies at the Technische Hochschule. While there his interest slowly moved into pure mathematics, and he eventually received his PhD under the supervision of Heinz Hopf and George Pólya. Pólya recommended Stoker to Courant, and he joined the Mathematics Department in 1937. The training program Courant proposed in 1941 was to draw upon the combined knowledge of himself, Friedrich and Stoker. Together they covered a broad range of specialties.

Despite Courant's various efforts, even his later more modest proposal did not receive the necessary support from policy makers and funding bodies outside of the university. The National Science Fund turned down Courant's request for financial support, claiming that the matter must be studied on a national level rather than a local one.³⁷ In August, Harry W. Chase, Chancellor of New York University, wrote to Frank Jewett asking him to meet with Courant to learn about his proposal. In his reply Jewett, who was already familiar with Courant's proposal, expressed grave doubts that an additional institution for training applied mathematicians should be established at the moment. Jewett noted the proximity of the two groups on the East Coast and added that the

³⁶ Constance Reid, "Kurt O. Friedrich 1901 -1982," *The Mathematical Intelligencer* 3, no. 3 (1995): 23–30.

³⁷ William James Robbins to Richard Courant, 15 August 1941, RCP, Box 16 (old), Folder "NAS."

number of men who stood to benefit from such training was “limited” as was the number of men “competent to give instruction.”³⁸

It is difficult to determine the exact relationship between Courant and Richardson, or the NYU and Brown Group more generally. On the one hand, the two men took part in a cordial correspondence during the period, in which they acknowledged their common objectives. Moreover, Friedrich participated as an instructor in at least one of Brown’s summer courses. Yet, as the letter from Jewett makes clear, at least at first some level of competition existed between the two groups and it is obvious that Courant’s efforts were hindered by Richardson’s success. In March 19, 1941, only a day after Richardson circulated his memorandum, Courant wrote to Thorndike Saville, Dean of the School of Engineering, informing him of the “threat to our plans from Brown University.” Courant argued that it would be improper if the plan they had been devising would be eliminated now due Brown’s initiative. He suggested that Thornton Fry must have used his close personal connection with Jewett and Bush to “pave the way” to Richardson’s plan.³⁹ Courant’s summer courses, as noted earlier, still took place in 1941, but his larger aspiration to found a training and research center for applied mathematics had to wait until the postwar period. Not only was Richardson better connected with the scientific elite of the country, but his vision for applied mathematics seemed to have fitted more directly the conceived demand. The immediate demands of the war were for practically trained mathematical engineers who could use their training to solve specific problems. The scientific elite was already well populated by physicists.

Eventually it was through its involvement with the Applied Mathematics Panel that Courant’s group at NYU rose to a position of leadership in the field and gained national

³⁸ Frank Jewett to Harry W. Chase, 27 August 1941, RCP, Box 16 (old), Folder “NAS.”

³⁹ Richard Courant to Thorndike Saville, 19 March 1941, RCP, Box 19 (old), Folder “Richardson.”

recognition. As noted in Chapter Two, the NYU group was able to continue its operation at end of the war through contracts with the Office of Naval Research. However, despite Courant's relentless attempts to convince university administrators to more fully support his group and provide it with greater autonomy, the institute was not officially established until 1953.

Nonetheless, the groups continued to train students in the department of mathematics throughout the period. At Brown, things advanced more smoothly. Richardson's efforts paid off, and at end of 1945 the Division of Applied Mathematics was officially recognized as an independent training program within the university. In the first decade after the war NYU and Brown continued to represent the two most concentrated efforts to provide training in the field. Yet a divergence in philosophies was already present during the first decade of activities at the two groups.

From 1946 until 1955 the Division of Applied Mathematics and the Institute at NYU conferred respectively sixty-four and sixty-seven doctorates in mathematics.⁴⁰ Possibly the most striking similarity between the two programs was the small number of graduates who obtained an academic position in a traditional department of mathematics (around seven from each program). Especially among the more distinguished departments the number was practically negligible. Murray Portter, who graduated from Brown in 1946, became a mathematics professor at Berkeley and Paul Berg, who graduated from NYU in 1953, became a professor at Stanford.⁴¹ Out of the other 130 doctorates who graduated from these two programs during the decade, not

⁴⁰ Here I only count the Brown graduates who graduated from the division of applied mathematics as opposed to the department of mathematics. The list of graduates, their thesis title, and their advisors can be accessed online through the Mathematical Genealogy Project (<http://genealogy.math.ndsu.nodak.edu/index.php>). Unless specifically stated, the information regarding the professional trajectories of the programs' graduate was compiled using *American Men and Women of Science 19th edition*. A complete record of all graduates and their employment history is impossible to attain. The data used here covers approximately 80% of all graduates during the decade, and provides a clear view of general trends and differences between the two groups.

⁴¹ Here it is worth noting that both Protter and Berg were supervised by Lipman Bers, who moved after the war from Brown to NYU.

one obtained a position in the departments of mathematics at Harvard University, the University of Chicago, or Princeton University, which were considered the top three departments in the field.

Though the graduates of the programs at NYU and Brown also offer a clear view as to some of the differing philosophies that underlined training the field, the relation between mathematics, engineering, and physics and the presumed role of the applied mathematician within these disciplines. The difference becomes clear when we compare the faculty of each program who were in charge of training this new generation of American applied mathematicians. At Brown, William Prager and George Carrier each advised eleven students (with one additional student co-advised by both of them) and Erastus Lee followed closely advising nine students during the decade. All three men were trained as engineers. Prager immigrated to the United States from Turkey after being forced out of his academic position in Germany in 1934. Dean Richardson recruited Prager to direct Brown's effort in applied mathematics and in 1946 Prager assumed leadership over the new Division. Prager like Courant and Friedrichs was a German émigré, but his training was not from one of the German mathematical seminars, but rather from the Technical University of Darmstadt. His research was in theoretical mechanics with a focus on the theory of plasticity.⁴²

Carrier arrived at Brown after working as a research engineer designing a high-speed cascade wind tunnel in order to study jet engine turbines. He received his PhD from Cornell University during the war on the topic of elasticity, and was known for his ability to intuit mathematical models of engineering systems. When he left Brown in 1952, he was appointed the

⁴² H. G. Hopkins, "Professor William Prager: 23 May 1903–17 March 1980," *International Journal of Mechanical Sciences* 22, no. 7 (1980): 393–394; Daniel Drucker, "William Prager, 1903-1980," in *Memorial Tributes: National Academy of Engineering, Volume 2* (Washington, D.C.: The National Academies Press, 1984), 233–235.

Gordon McKay Professor of Mechanical Engineering at Harvard University.⁴³ Lee followed a similar professional trajectory. A British-born Cambridge University-trained mathematician, he arrived at Stanford to study with Russian engineer Stephen Timoshenko in 1937, earning a doctorate in mechanical engineering and mathematics in 1940. Lee arrived at Brown after working as a technical engineer at the newly established British Department of Atomic Energy. He returned to Stanford in 1962 as a Professor in the Division of Applied Mechanics and the Department of Aeronautics and Astronautics.⁴⁴ Given that these three professors supervised half of all doctorates at Brown University in the period, it is not surprising that the academic trajectories of their students followed similar paths.

At least ten Brown doctoral students in applied mathematics obtained a position at some point in their career as a professor of either mechanical, aeronautic, or electrical engineering. Three Brown students, John Lyell Sander Jr., Bernard Budiansky, and Carl Pearson, were appointed as professors at Harvard University School of Applied Sciences. The program produced many renewed researchers, but they did not find their ways into mathematics departments. At least a third of all graduates obtained an industrial or governmental position upon earning their degree. General Electric, General Motors, Boeing, Lockheed Aircraft Corp, and the National Advisory Committee for Aeronautics (NACA, precursor to NASA) were some of the industries in which graduates of the program were employed. Bernard Budiansky's career can serve as representative of several of Brown's graduates.⁴⁵ He began serving as an aeronautic

⁴³ Frederick Abernathy and Arthur Bryson, "George F. Carrier, 1918-2002" (Washington, D.C.: The National Academies Press, 2007), 46-51; Arthur Bryson et al., "George Francis Carrier," *Proceedings of the American Philosophical Society* 152, no. 2 (June 2008): 229-233.

⁴⁴ Vlado Lubarda, "In Memoriam: Erastus H. Lee," *Journal of Applied Mechanics* 74, no. 4 (July 1, 2007): 601-602; Fre Ling, "Erastus H. Lee, 1916-2006," in *Memorial Tributes: National Academy of Engineering, Volume 12* (Washington, D.C.: The National Academies Press, 2008), 173-177.

⁴⁵ Budiansky is probably one of the more highly respected scholars who emerged out of Brown during the decade. I use him as an example more in terms of his professional trajectory than his success.

research scientist at the NACA in Langley Field, Virginia during the war in 1944. In 1947, he took a leave of absence to pursue a PhD at Brown under Prager and returned to Langley in 1950. Two years later, he was appointed the head of the structural branch, a position he held for three years before assuming an engineering professorship at Harvard.

Or take the career of William Nachbar, who wrote his dissertation under Lee in 1951. Upon graduating, Nachbar became a staff member of the mathematics service unit at Boeing, where he took part in designing the first commercially successful jet airliner, Boeing 707. From Boeing he moved to Lockheed Missiles and Space Division, where he was a member of the team that developed the submarine-launched ballistic missile Polaris. After twelve years in industry, Nachbar moved to the academic world, becoming an associate professor at the Department of Aeronautics at Stanford and later a professor at the University of California at San Diego. Both Burdiansky and Nachbar moved freely between the academic world and the industrial world. Their research was theoretical but it was fundamentally rooted in the physical world.

The graduates of NYU followed a different path for the most part. Together Courant, Friedrichs and Stoker supervised the work of thirty-six students, more than half of all PhDs produced during the decade. The most noticeable aspect pertaining to training in the institute during the period was the high percentage of its graduates who upon receiving their PhD remained at NYU as researchers. Almost a third of all graduates ended up spending either a few years or their entire career at NYU. As noted in the previous chapter, the group signed an increasing number of contracts with government agencies in the decade following the war and it consequentially increased in size. For the most part, the demand for new staff was fulfilled by graduates of the program. Peter Lax and Louis Nirenberg are representative of the group's graduate during the time. Both Lax and Nirenberg obtained their PhD in 1949 under the

supervision of Friedrichs and Stoker respectively, and both men remained at NYU for their entire career, where together they supervised about a hundred students.

Lax and Nirenberg were both prolific researchers, and each man published more than 150 articles during his career. The lion's share of their publications appeared in the institute journal *Communication on Pure and Applied Mathematics*. Yet like their advisors they also published in some of the top mathematical journals, such as *Annals of Mathematics* and *Acta Mathematica*. Celebrated analysts, both men won several of the most distinguished awards offered by the American Mathematical Society and other international mathematics organizations. Budiansky and Nachbar on the other hand did not publish in any leading mathematical journals.⁴⁶ Most of Budiansky's publications, for example, appeared in the *Journal of Mechanics and Physics of Solids* and the *Journal of Applied Mechanics*. Like Lax and Nirenberg Budiansky received several honors; however, these were awarded by the American Society of Mechanical Engineers.

A smaller percentage of NYU graduates found their way into industry, and of those almost all found their way into the aerospace industry. NYU's students were more likely to end up conducting research in mathematical physics. The Radiation Lab and the Applied Physics Laboratory at John Hopkins, Lawrence Livermore, and the Princeton Plasma Physics Laboratory were some of the research institutes where graduates of Courant's group were employed. If Brown graduates were directed toward engineering and industry, then NYU graduates gravitated toward physics and the academic world.⁴⁷

⁴⁶ Nachbar did publish one article in the *Quarterly of Applied Mathematics* and one in *SIAM Journal of Applied Mathematics*.

⁴⁷ Besides general courses in applied mathematics, partial differential equations, the theory of elasticity, and the mechanics of rigid bodies, the NYU group also offered courses in the calculus of variation and application to physics, theory of propagation of waves, transient analysis of electronic networks, and a seminar on the mathematical foundation of quantum mechanics. In a 1946 letter to Walter Bartky, Dean of the Graduate School at the University of Chicago, Courant informed Bartky that among its various present activities, the group was "trying

The two different portraits of the applied mathematician that emerge when comparing the graduates of the two programs were rooted in different ideas about the contours of the field.

Decades after the war, Friedrich noted in an interview:

Von Kármán knew Courant very well and he realized that Courant was not really an applied mathematician... Yes, mathematical physics -- Courant-Hilbert, fine -- but that is not applied mathematics. Karman had come to the United States before Courant. He had the engineering background. He also had a very good understanding of applying mathematics -- he wouldn't have said "pure" mathematics but rather "sophisticated" mathematics -- to engineering. For all these reasons Karman felt that he should be the one to develop applied mathematics in this country, not Courant.⁴⁸

Training at Brown followed much more closely the model set by Von Kármán, who was a member of its evaluating committee.

Of course, Brown University and NYU were not the only universities that offered training in applied mathematics during the period. Carnegie Mellon Institute and MIT, for example, also provided advanced training in the field, as well as Berkeley and the University of Michigan. However, there did not exist any standard regarding what training in applied mathematics entailed, or for that matter what was the goal of such training. In 1954, this plurality of approaches was placed front and center when representatives from various training programs in applied mathematics convened for a three-day conference to discuss the future of training in the field.

to integrate some of our work with theoretical work in the Physics Department." Richard Courant to Walter Bartkey, 22 November 1945, RCP, Box 36, Folder 9.

⁴⁸ Quoted in Constance Reid, *Courant* (New York: Springer-Verlag, 1996), 226–227.

“A State of Rather Considerable Confusion”

“The problem of applied mathematics has once more aroused the conscience of the American mathematical community,” Joachim Weyl wrote to Warren Weaver in June 1953.⁴⁹ Weyl, who, as mentioned in Chapter Two, was the principal investigator for the Survey Committee on Research and Training in Applied Mathematics of the National Research Council, wished to inform Weaver of the upcoming conference on training in applied mathematics which was planned to take place in October. Weyl hoped that Weaver would be willing to give the final address of the meeting, “perhaps under a title such as ‘applied mathematics as a responsibility of the mathematics profession.’” In the letter he explained that the goal of the conference was to gain a “rounded view” of the ways in which mathematics “currently interacts with scientific research, engineering practices and managerial planning.”⁵⁰ In an earlier draft of the letter, Weyl put things more directly, explaining “in short” that the purpose of the conference was to get a sense of “how mathematics is faring today as a member on the team of the science rather than their unapproachable queen or their uninspired handmaiden!”⁵¹ The problem the conference was to address was how to train applied mathematicians who were neither too theoretically removed from the world nor too pragmatic.

In order to achieve this goal, the organizers invited representatives from universities that offered training in applied mathematics to deliver talks describing the programs in their respective institutions. A full decade after the first summer session in applied mathematics at Brown University, advanced training in the field was far from stabilized on a national level, and

⁴⁹ Joachim Weyl to Warren Weaver, 15 June 1953, RCP, Box 24 (old), Folder “Weaver.”

⁵⁰ Ibid.

⁵¹ Joachim Weyl to Warren Weaver, undated, RCP, Box 24 (old), Folder “Weaver.” The letter is almost a direct replica of the one quoted before, which makes it clear it was written by Weyl. It must be an earlier draft that Weyl sent to Courant as this sentence is taken out and replaced by a reference to the secular as opposed to monastic mathematics.

local circumstances played the decisive role in the way programs were set up around the country. This became clearly evident in the organizational structure of the program. The four sessions dedicated to training programs cut across institutional affiliations. “Applied Mathematics in the Traditional Departmental Structure” included talks by the chairs of the Mathematics Departments at University of California at Berkeley, the University of Michigan, and the University of Wisconsin. “Mathematics in the Integrated School of Applied Science,” as mentioned in the introduction, included representatives from MIT, Harvard University and the University of Illinois. The programs at NYU and Brown University, together with those of Stanford University and the University of Maryland, were grouped together under the headline “The Graduate Institute of Applied Mathematics.” Finally, the fourth session focused on training in applied mathematics in Europe.

Once the conference began, it quickly became clear that there was quite a wide range of philosophies as to what training in applied mathematics should entail, and that these did not necessarily depend on institutional structure. Thornton Fry, who gave the concluding remarks instead of Weaver, commented on this early state of affairs:

The conference started in a state of rather considerable confusion. There were not only differences of opinion natural to the thinking of different people; there were even questions as to what in the world the conference was about anyway; was it mathematics, or was it mathematicians, that was being discussed?⁵²

The answer was both. In order to discuss the appropriate course of training for applied mathematics, participants in the conference had to agree, at least in principle, on what applied

⁵² Thornton C. Fry, “Applied Mathematics as a Responsibility of the Mathematical Profession,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 90.

mathematics was. However, as mentioned in Chapter One and Two, there just was not any consensus on the matter. Prager perfectly summarized the situation, noting, “at a meeting such as this, one hears almost as many definitions of applied mathematics as there are speakers.”⁵³ This inability to define the constitution of the field had the effect of shifting the conversation from mathematics to mathematicians.

Discussions during the conference did not revolve around which courses to teach or how to devise an effective curriculum. Applied mathematics, most participants seemed to concur, was a matter of attitude, not subject matter. Training applied mathematicians, therefore, had to do more with training mathematicians in certain modes of thinking and ways of interacting with the world than with imbuing them with fundamentals. Yet not all participants agreed as to how best to achieve this objective. Their disagreement was often rooted in different conceptions as to the professional identity of the applied mathematician.

The one thing almost all those in attendance agreed upon was that the more mathematics one knows, the better. The applied mathematician “must of all things be a mathematician,” Griffith Evans exclaimed. “He must acquire the breadth and depth of other mathematics students – algebra, number theory, analysis, topology, geometry, and (let me add) mechanics,” he added.⁵⁴ Theophil Hilderbrandt, of the University of Michigan, repeated the sentiment, noting, “[I]t certainly seems trite to remark that in order to apply mathematics, one must know some

⁵³ William Prager, “The Graduate Division at Brown University,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 35.

⁵⁴ Griffith Evans, “Introductory Remarks on Applied Mathematics in the Traditional Departmental Structure,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 12.

mathematics to apply.”⁵⁵ What was less clear was the level of instruction students were required to take in other sciences.

In the introduction to the conference proceedings summarizing the main conclusions that came out of the presentation and discussion, Weyl commented on this state of affairs. Pointing out that the various programs presented differed mainly in the number of scientific courses students were required to take, he remarked:

Here is one of the factors which gave some fine-structure to the broad-band concept of applied mathematician-in-training with which the conference was faced, extending all the way from an end-product who would have no more idea of the physical reality behind the names he gives to this symbols than his colleagues in pure mathematics, to the mathematicians who tops off his formal education by active participation in an experimental program.⁵⁶

What accounted for this wide variability in “end-product”? How did the applied mathematician with experimental experience differ from his colleague who never took any coursework in physics or engineering? The answer to these questions was closely related to the presumed job of the applied mathematician.

Already in the initial planning of the conference, the organizers wished to include both the “producer’s” and the “consumer’s” viewpoint. Thus, in addition to educators, the organizers invited representatives from government and industry to deliver talks on the nature of the demand for mathematicians in these sectors. Whereas a degree in pure mathematics was understood, for the most part, as a preparation for a career in either academic research or

⁵⁵ Theophil Hilderbrandt, “Applied Mathematics at the University of Michigan,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 18.

⁵⁶ Weyl, *Proceedings of a Conference on Training in Applied Mathematics*, viii.

teaching, the applied mathematics degree was being constructed with an eye toward industrial and governmental positions.⁵⁷ Future occupational considerations, outside of the university, became a decisive factor in determining the “proper” course of training in applied mathematics. The “producers,” the organizers of the conference seemed suggest, had to match what the “consumers” were looking for. What, however, was the presumed nature of the applied mathematician’s job in industry? How did it differ from the job of the engineer or the physicists? And what sort of training did it require?

In 1951, the *American Mathematical Monthly* published a report on “Professional Opportunities in Mathematics.” The report was the work of a committee, chaired by Mina Rees, of the Mathematical Association of America (MAA).⁵⁸ The report covered a wide range of possible occupations from high-school teachers to actuarial work. Although the report was directed at undergraduates, who were considering obtaining a BA in mathematics, an industrial position, the report explained, required more advanced training.⁵⁹ “Industrial mathematicians,” the report declared, “act primarily as consultants.” They are not responsible for specific engineering projects, which will fail to take advantage of their training; rather, their contribution comes at the design stage of a given experiment. “Whenever a mathematician becomes responsible for an engineering project, he ceases to function as a mathematician and becomes an

⁵⁷ During his remarks, Griffith Evans explained that the University of California had recently decided to inaugurate a degree in applied mathematics. Namely, the student’s diploma would clearly indicate that his area of research was in applied mathematics as opposed to just mathematics. Explaining the reasoning behind this decision, Evans said “we wish to emphasize the importance of applied mathematics for research, and for employment in civil service and in industry.” Evans, “Introductory Remarks on Applied Mathematics in the Traditional Departmental Structure,” 14.

⁵⁸ “Professional Opportunities in Mathematics: A Report for Undergraduate Students of Mathematics,” *American Mathematical Monthly* 58, no. 1 (January 1951): 1–24.

⁵⁹ Opportunities for BAs or an MAs in mathematics, according to the report, were mostly limited to computing positions, where the work for the most part included only routine computing. Moreover, many “industrial laboratories employ only women to fill positions in their computing groups.” The report made it clear that for a student interested in mathematics who wished to obtain an industrial position, a PhD was almost a necessary requirement.

engineer.”⁶⁰ This somewhat circular definition points to the conceived role of the mathematician within the industrial workplace. The mathematical consultant was not the mathematical engineer envisioned by Richardson, nor was he the applied mathematician called for by Courant. He was motivated to solve industrial problems rather than produce basic research, but he was not an engineer.

John Tukey was the main advocate of the mathematical consultant as a new professional identity for the applied mathematician. “What is at stake here is the concept of the mathematical consultant,” he replied to McVittie’s dismay at the end of the second session. Throughout the conference he continuously brought up the importance of training mathematicians for consulting jobs, arguing that it was in this capacity that mathematicians would be most likely to make their mark in industry. Tukey’s own consulting work served as a perfect model for the kind of mathematical expert he was now promoting. He divided his time between Princeton University, where his research focused on mathematical statistics, and Bell Labs.⁶¹ Over the years he worked as a researcher on a wide range of projects from the anti-aircraft Nike missile system, to the development of the high-altitude U-2 aircraft, and as an investigator of the statistical methodology behind *The Kinsey Report*.⁶² Starting in 1960 his many achievements gained him membership in the President’s Science Advisory Committee. He advised dozens of students and

⁶⁰ Ibid, 10.

⁶¹ In 1950, Tukey became a full professor at the Department of Mathematics at Princeton. In 1966, when the Department of Statistics was established, Tukey became its first chair and was appointed Professor of Statistics.

⁶² Tukey was not the only trained mathematician who served as consultant to industries and government laboratories in the 1950s and 1960s. Many of the graduates of both the Brown and the NYU group who did not work directly in industry served as consultants at companies throughout their careers. Budiasky, for example, spent some years as a consultant at General Motors and various aerospace engineering companies, while Lax was a consultant at Los Alamos National Lab. The list of organizations in which students worked as consultants is quite varied. It includes General Electric, Grumman Aircraft Corporation, Knolls Atomic Power Laboratory, RCA, IBM, and Raytheon Company. However, many of those graduates who served as consultants in these companies did so while holding a permanent academic position in either a mathematics or engineering department. It seems that this was not exactly what Tukey had in mind when he called for the formation of the mathematical consultant as a newly established and distinct professional identity.

continued to publish numerous mathematical papers. To add to it all, he was also an avid folk dancer.⁶³

Tukey was not one of the invited speakers. Yet, a month after the conference, he felt the need to put into writing his ideas about applied mathematics training. “Mathematics, Applied Mathematics and Mathematical Engineering,” as he explained to members of the Survey Committee, outlined his reaction to “some aspects of the New York Conference.”⁶⁴ As the title of his memorandum suggests, Tukey distinguished between mathematical engineers on the one hand and applied mathematics on the other.⁶⁵ Simply put, the distinction Tukey drew was between a mathematically trained engineer and a scientifically trained mathematician,⁶⁶ both of whom were distinct from the mathematical consultant, who represented, according to him, a novel professional identity for the mathematician. In an exchange with Mina Rees during the conference, Tukey explained that the mathematical consultant “would thoroughly understand mathematics and could be

⁶³ David R. Brillinger, “John W. Tukey: His Life and Professional Contributions,” *Annals of Statistics* 30, no. 6 (2002): 1535–1575.

⁶⁴ John Tukey to Members and consultant of the NRC Committee on Training and Research in Applied Mathematics, undated, RCP, Box 16 (old), Folder NRC. Although the cover letter itself is undated, the memorandum is dated November 28, 1943. Most likely Tukey mailed it a few weeks after he completed writing it.

⁶⁵ A few years later, Tukey published two articles based on the memorandum he sent to members of the Committee. The first, “Mathematical Consultant, Computational Mathematics, and Mathematical Engineering,” was published in 1953 in the *American Mathematical Monthly*. The second, “The Teaching of Concrete Mathematics,” was published in the same journal five years later. Both articles follow very closely the original memorandum Tukey circulated with several amendments he added over the years. In the 1953 article, after discussing the mathematical engineers, Tukey writes, “many readers may by now be muttering under their breaths, ‘But what *is* mathematical engineering?’” Acknowledging that no complete answer is available, Tukey offers the following definition: “mathematical engineering consists of those branches of engineering where the single most important tool is mathematics.” J. W. Tukey, “Mathematical Consultants, Computational Mathematics and Mathematical Engineering,” *The American Mathematical Monthly* 62, no. 8 (1955): 565–571; John W. Tukey, “The Teaching of Concrete Mathematics,” *The American Mathematical Monthly* 65, no. 1 (1958): 1–9.

⁶⁶ Despite Weyl’s conclusion that there was quite a wide variation among the various programs, in his memorandum following the conference, Tukey had a somewhat different opinion. “The recipe for theoretical mechanics in the U.S. seems to be:

2 parts mathematics, 1 part physics, 2 parts engineering.
mix well and serve labeled ‘Applied Mathematics.’”

This recipe, however, only pertained to theoretical mechanics as developed according to him in programs such as Brown University.

counted upon for effective help in the mathematical phases of an undertaking, but he would not make any original contributions to the field.”⁶⁷ Here was the “new brand of applied mathematician” Tukey described, one that was predominantly defined via his occupation.

However, what sort of training would prepare students to assume consulting positions? According to Tukey, “[T]he training should be a research training in (pure) mathematics (or in pure physics or in engineering for its own sake).” He added that the requirements should include an emphasis on research with as wide a background in mathematics as possible. However, the most important qualification was “an interest in other men’s problems (in these problems as wholes, not just in their mathematical aspects!).”⁶⁸ It was *interest* in other man’s here too problems that the mathematical consultant required, *not* training. Namely, it was the mathematical background, not the scientific one that prepared the consultant for his job. In the 1960s, Tukey became involved in environmental policy. He chaired presidential panels and wrote reports about environmental pollution and air quality. He had no prior background in the field, but rather came to it as a statistician learning the rest as he went along. The extent to which scientific training was required became a distinguishing marker of the various professional identities that emerged around the application of mathematics in the postwar period.

Unlike the consultant, the mathematical engineer required a much more thorough background in the sciences. Describing Harvard University’s program in applied mathematics, Howard Emmons explained that as the applied mathematician moved into the

⁶⁷ Weyl, *Proceedings of a Conference on Training in Applied Mathematics*, 70.

⁶⁸ John Tukey to Members and consultant of the NRC Committee on Training and Research in Applied Mathematics, undated, RCP, Box 16 (old), Folder “NRC.”

workforce he would need to “talk shop” with engineers and mechanics. The applied mathematician, therefore, must have a through training in engineering problems and the relevant science if he wished to be useful in the initial formulation of a problem. According to Emmons, Harvard “resist[ed] the temptation to put applied mathematics in a separate department,” because “we feel that *applied* mathematics must be applied to something if it deserves the name.”⁶⁹ An exhaustive course of study must include courses in both mathematics and other scientific fields. The applied mathematician Emmons described, was integrated much more seamlessly into the workforce. He was just as much a man of the shop as much as he was a man of the office. He was not the mathematical consultant Tukey championed. The man described by Emmons was, according to his scheme, a mathematical engineer and as such should be trained in engineering schools rather than mathematics departments.⁷⁰ Tukey, in fact, argued that following physicists and chemists, mathematical engineers should be taught in the engineering school not in mathematics departments. Mathematicians, he strongly believed, should welcome and support the establishment of Departments of Mathematical Engineering.⁷¹

Tukey was not the only attendee of the conference who promoted the idea of the mathematical consultant. In response to Tukey’s remark, Otis Lancaster asserted, “[T]he mathematical community will have to realize sooner or later that the mathematical consultant is

⁶⁹ Emmons, “Applied Mathematics in a Division of Applied Science,” 29.

⁷⁰ This distinction became clear during the discussion following Rudolph E. Langer’s presentation about the BA and MA programs in applied mathematics at the University of Wisconsin. The aim of the program, which Langer described as giving a thorough training in mathematics with a broad initiation into “engineers practices,” was producing industrial workers. In the discussion that followed Langer’s presentation Tukey suggested that the program he described actually belonged in the engineering school.

⁷¹ Tukey, John. “Mathematics, Applied Mathematics and Mathematical Engineering,” 28 November 1953, RCP, Box 16(old), Folder “NRC.”

here to stay and should give him the esteem and the training he deserves.”^{72,73} Following suit, Edward McShane added, “[T]he mathematical consultant is really an important member of our community.”⁷⁴ During the discussion that followed the first session of the conference, Abraham Taub interjected:

It seems to me that we are overlooking important points. The Survey Committee is worried about the lack of people who could be helpful in the large number of endeavors where mathematicians are currently needed. Few mathematicians realize as yet that half of the applied mathematician’s task is that of finding and formulating mathematical problems – once this is done any mathematician can work on them.

What can be done to help in supplying people of this type?⁷⁵

According to Taub, the job of the applied mathematician was not to provide solutions to given problem. Rather, his job was to formulate in mathematical terms the nature of the given problem. Here was another characteristic that distinguished the mathematical consultant from the mathematical engineer. Emmons, unlike Taub, insisted that the work of the mathematician did not end with the formulation of the problem. “It is not enough to stop with some complex integral. The man in the shop who wants the answer cannot generally be expected to learn the intricacies of advanced analysis. He wants, and we believe rightfully demands, a curve or a number.”⁷⁶

In carving out a place for the applied mathematicians within the industrial workforce and distinguishing the mathematician from the engineer, mathematicians were calling in a sense for

⁷² Weyl, *Proceedings of a Conference on Training in Applied Mathematics*, 29.

⁷³ Lancaster received his PhD in 1937 under the supervision of George Birkhoff. He published his dissertation in *American Journal of Mathematics* two years later, and moved into the Department of the Navy, becoming an expert in aerodynamics.

⁷⁴ Weyl, *Proceedings of a Conference on Training in Applied Mathematics*, 29.

⁷⁵ Weyl, *Proceedings of a Conference on Training in Applied Mathematics*, 23.

⁷⁶ Emmons, “Applied Mathematics in a Division of Applied Science,” 26.

an intellectual division of labor. Finding their place within the group implied identifying their role within the work itself. Mathematicians, Taub claimed, were needed in an increasing number of “endeavors.” Yet it was not quite clear how to integrate mathematicians into these novel projects.⁷⁷ The main task of the mathematical consultant was to help formulate the problems from a theoretical point of view, not to engage directly with the ins and outs of the work. But was it possible to isolate the mathematical component of the work? And what was the relation between theoretical abstractions on the one hand, and interpretation of specific physical phenomena on the other? As will become clear in the last section of this chapter, not everyone agreed that such a clear division of labor was possible or even desirable.

The question became not “what should applied mathematicians be taught?”, but “how should they be taught?” In his memorandum to members of the Committee, Tukey asked, “Polya wrote ‘how to solve it?’, who will write ‘how to formulate it?’”⁷⁸ Tukey proclaimed that despite the fact that the formulation was the most important stage in applied mathematics, there hadn’t been any attempt to establish a theory of formulation. “Yet if ‘applied mathematics’ is to grow properly, if there is to be something teachable and worthy of the ‘applied mathematics’, someone must tackle the problem – and eventually there must be developed a technique of wide usefulness and acceptability for teaching.”⁷⁹ Tukey’s call for a meta-theory of applied mathematics might not have come true. *How to Formulate it?* remains to be written.⁸⁰ Yet it did

⁷⁷ The question of how to integrate mathematicians into scientific projects was, of course, already evident during the war. As evident in Chapter One, at least some mathematicians like Stone believed that once a given problem was translated into mathematics, it could be studied independently. However, even during the war, this view was not adopted by all. The establishment of the Applied Mathematics Panel as a unit within the Office of Scientific Research and Development suggests that a more integrated approach was desired.

⁷⁸ Tukey, John. “Mathematics, Applied Mathematics and Mathematical Engineering,” 28 November 1953, RCP, Box 16 (old), Folder “NRC.”

⁷⁹ Ibid.

⁸⁰ As several other presenters at the conference noted, there was not any textbook that gave a general introduction to applied mathematics. There were monographs written about specific areas and problems, such as the theory of elasticity or the theory of aerodynamics. Further, there were some textbooks with titles such as calculus for

raise some crucial considerations as to *how* applied mathematics should be taught – especially as to the relation between theory and subject matter.

* * *

Pondering why courses in mechanics, despite being offered at several universities around the country, failed to develop in the prewar decades, Griffith Evans pointed to an important characteristic that affected teaching in applied mathematics:

Presumably, the reason why the courses in the mechanics of continua as given forty years ago – and indeed sometimes even not so long ago – did not lead to further development is because they were not given for the sake of the subjects of elasticity and hydrodynamics themselves. They were regarded, I believe, rather as illustrations of the theory of quadratic forms and quadratic surfaces or of complex variable and conformal transformation.⁸¹

In teaching was the subject matter supposed to drive the theory, or was it the other way around? Should students learn elasticity as an illustration of the mathematical theory behind it? Or were they to learn the mathematics involved as an outcome of learning the theory of elasticity?

Several speakers commented on the fact that this was a question of emphasis and presentation. Two departments could supposedly offer the same course, but the nature of the presentation would greatly differ. Students would either focus on the underlining

engineering students or matrix algebra for physicists. But there was no book that presented applied mathematics as a coherent methodology, a book akin to Tukey's *How to Formulate it*.

⁸¹ Evans, "Introductory Remarks on Applied Mathematics in the Traditional Departmental Structure," 11.

mathematical theory conceiving of the subject matter as a mere illustration, or they would learn the subject matter well and approach the mathematical theory as nothing more than a tool, a way of solving a given problem. During his talk, Hilderbrandt lamented that at the University of Michigan the increased competition for students lowered the availability of courses offered by the Department of Mathematics. “Adjacent departments,” he explained, “have installed their own courses and instead of getting a wide mathematical background, prefer to give only enough mathematics to cover the subject matter immediately under consideration.”⁸² Jerome Wiesner of MIT similarly noted that “much of the applied mathematics is...taught in the individual engineering departments who feed their students the mathematics which is needed at the time it is needed.”⁸³ In both of these cases, the necessary mathematics was not taught for its own sake but as a corollary of the physical theory under consideration.

However, this difference in emphasis between the abstract mathematical theory and the subject matter did not just depend on the specific department in which a particular course was given. It was also rooted in diverging philosophies as to what was the nature of applied mathematical work. Here once again a comparison between the program at Brown and the program at NYU serves to illustrate the point. Echoing Taub’s remark, Prager asserted that the “most significant service” an applied mathematician can render “consists in the mathematical formulation of the problem rather than the selection of an efficient method for its solution.” More than just a “casual acquaintance” with the technological and scientific background was necessary, according to Prager, in order to accomplish this task.

⁸² Hilderbrandt, “Applied Mathematics at the University of Michigan.”

⁸³ Jerome Wiesner, “Applied Mathematics Training at MIT,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 25.

After all, applied mathematics was not just “a collection of methods that can be taught divorced from the physical situation to which they are applied.”⁸⁴ The willingness to dedicate a considerable amount of time and energy to learning the necessary physical theory rather than the mathematical one was, for Prager, part of what distinguished the applied from the pure mathematician. “It is with a view to developing this frame of mind that we restrict to a minimum the number of courses concerned with purely mathematical subjects,” Prager explained. “For example,” he added, “tensor analysis is developed in the courses on elasticity, plasticity, and electromagnetic theory, and not as a separate subject.”⁸⁵ Following Prager’s logic, presenting the mathematical theory as an outcome of learning the physical theory rather than the other way around was a necessary condition for producing applied mathematicians. It also, however, assumed a practitioner whose goal was to apply his knowledge to particular situations, rather than one whose aim was to continue developing the theory itself.

The courses offered as part of the program at Brown included, for example, “mathematical methods of applied science,” “differential and integral equations of mathematical physics,” and “practical analysis” as well as several courses in mechanics. Most of these were not offered in traditional mathematics departments, where the emphasis would not have been on the application of a given theory, but on its internal coherence. At NYU courses in fluid dynamic and statistical mechanics were also offered alongside traditional mathematical courses. However, Courant, who declared during his talk that the institute at NYU did “not recognize a legitimate separation of applied and theoretical mathematics,” emphasized that theoretical mathematics, mechanics, and application

⁸⁴ Prager, “The Graduate Division at Brown University.”

⁸⁵ Ibid.

needed to be developed in tandem.⁸⁶ As such, research projects sought to move across both basic and applied research. For example, the theory of partial differential equations, “in all its ramifications from existence proofs to numerical methods important for physics and engineering,” has always been an active research front in the Institute.⁸⁷ That is, partial differential equations were studied both as an independent mathematical theory and for their applicability.

Echoing his war memorandum, Courant warned against a goal-oriented conception of the field. Presenters were asked to prepare (and possibly share) their comments ahead of the meeting, but after sitting and listening to the talks and discussions during the previous two sessions, Courant decided to add a short impromptu introduction to his prepared remarks.⁸⁸ It might be difficult to define applied mathematics, Courant began, but it is easy to be “explicit about what we do *not* mean by training in applied mathematics. This is an indoctrination of young people in certain mathematical skills which will perhaps make them more easily acceptable as employees in some industries or technical services.”⁸⁹ Courant was one of the only presenters to argue directly against a narrow conception of training in the field as a preparation for industrial work. Such pragmatic training, Courant explained, will only serve to justify the various “prejudices” against the field. Moreover, Courant argued, industry itself would not benefit from employees trained so narrowly.⁹⁰

⁸⁶ Richard Courant, “On the Graduate Study of Mathematics,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 32.

⁸⁷ Ibid.

⁸⁸ After the conference Courant sent a letter with the additional remark he made at the start of his talk to be added to the prepared mimeographed talk he delivered, which he explained he offered in response to the discussion during the previous day of the conference. Richard Courant to Mrs. R. Scott, 4 November 1953, RCP, Box 16(old), Folder “NRC.”

⁸⁹ Courant, “On the Graduate Study of Mathematics,” 30.

⁹⁰ Courant goes on to say that industry would “fare better if they would look for people who have studied Greek grammar.”

Applied mathematics programs, he exclaimed, should not produce “servants,” but “masters of the mathematical sciences.” Courant held that mathematicians had an important role to play in the development of science and industry, but it did not imply that training in the field should become overly pragmatic. It was basic research that needed to be developed.

The other speaker who emphasized the important of developing applied mathematics from a theoretical standpoint rather than specific application was Menahem Schiffer. Schiffer, who was born in Berlin in 1911, began his education under Issai Schur at the Friedrich-Wilhelm University of Berlin. However, in 1934 before receiving his PhD he immigrated to Palestine. He completed his studies at the Hebrew University and remained there until 1952, when he was appointed a professor of mathematics at Stanford University. At Stanford, Schiffer joined the newly established Applied Mathematics Laboratory, one of the postwar institutions established by the Office of Naval Research . During his talk Schiffer claimed that a mathematician’s interest would be “captivated by some concrete aspect of scientific reality *only if* he senses a theory behind it which he must discover.”⁹¹ Schiffer explained that the aim of the Laboratory was to cultivate in students the tendency to discover the theoretical underpinning of a given experimental situation. Like Courant before him, Schiffer emphasized the importance of developing pure and applied mathematics in tandem.

Schiffer acknowledged that applied mathematicians could not deal solely with theoretical development and that it was important to be clear “about the kind of solutions which the applied mathematician should provide.” Specifically, Schiffer noted that it was

⁹¹ Menachem Schiffer, “The Applied Mathematics Laboratory at Stanford University,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 38.

important to develop “schemes for numerical calculations.” Yet he quickly added that these methods often did not provide “deep insight into the problems considered.” Schiffer’s philosophy was close to that of Courant. The important thing was to develop robust theories and expert theorists. “It is our opinion,” he explained, “that the limits of applied, or rather applicable, mathematics should not be drawn too rigidly and that an elegant mathematical development is preferable to a brute force approximation procedure, at least in the long run.”⁹² Schiffer’s description of applicable mathematics is reminiscent of the portrayals of pure mathematics (“elegant”) rather than of engineering (“brute force”). Schiffer, in fact, was the only speaker who directly pointed out the difficulties applied mathematicians had in securing academic positions within departments of mathematics. Schiffer proposed that the lack of students’ interest in this sort of theoretical applied mathematics, which the Stanford Laboratory was hoping to produce, was due to the fact that these mathematicians were not interested in industrial positions but realized that they would find it difficult to obtain regular academic appointments.

When he began his presentation, Emmons took a stab at Marshall Stone, who was probably sitting in the audience, exclaiming that “Mr. Stone apparently would define it [applied mathematics] as the kind of mathematics which someone is ultimately willing to buy.”⁹³ This might have been a deliberate exaggeration, but the conference *was* organized according to the logic of supply and demand, whereby regimes of training and professional opportunities were being co-constructed. If we place the mathematical engineer on one side and the theoretical applied mathematician on the other, then Tukey’s mathematical

⁹² Ibid.

⁹³ Emmons, “Applied Mathematics in a Division of Applied Science,” 25.

consultant can be seen as providing a compromise between the two. He was theoretically inclined, but was driven by industrial problems.

“A Fling in the Abstract”

On Friday afternoon during the last two sessions of the conference, the emphasis shifted from “producers” to “consumers,” as representatives from governmental establishment and industrial organizations took to the podium. The speakers were not asked to talk about the sort of mathematics that is being used in their respective establishment, but rather about the role mathematicians played in their respective organizations. The organizers hoped to hear firsthand from industrial representatives about the productive contributions offered by mathematicians. The last session was titled “The Mathematician in Industrial Organization.” However, a more accurate title would have probably been “The *Ineffectiveness* of the Mathematician in Industrial Organization.” Speakers spent more time describing the incompetence of mathematicians rather than their usefulness.

Mathematicians were valued for their ability to think abstractly, but it was exactly this quality, or rather intellectual tendency, that made them unsuitable for industrial work and made them stand out among their peers. Consequentially, in describing the sort of mathematicians required in industry, most speakers emphasized not their training as mathematicians, but their temperament and personal characteristics.

Among the industrial spokesmen invited to speak at the conference was E. C. Nelson from Hughes Aircraft Company. Nelson was a theoretical physicist by training, not a mathematician. Yet he was in charge of the digital computer laboratory at Hughes, and it was in this capacity that he was invited to discuss the role of mathematicians in industrial

laboratories. Nelson's presentation focused on the use of mathematical models in industry, their construction, their analysis, and their interpretation. According to Nelson, at the first step of model construction, the need for close familiarity with the physical description of the system under consideration made theoretical scientists more qualified for the job than mathematicians. "Mathematicians," he added, "are normally trained in the processes of abstracting but usually at a higher level which is not readily applied to the initial mathematical formulation of the problem."⁹⁴ It was the nature of their training that made mathematicians ill-suited for industrial positions – being able to abstract was an important part of the process, but a familiarity with the physical system was even more so.

But the troubles with integrating mathematicians into the industrial workplace did not end there. The next step of model analysis, being inherently mathematical, was the obvious place to take advantage of mathematicians' unique skills. However, even in this step knowledge of the physical characteristics of the problem was necessary. The properties of a given model, Nelson explained, were most likely too numerous to be studied in a systematic and comprehensive way. In order to efficiently analyze a given model, it was important, therefore, to consider only those properties that were relevant to the problem. Nelson added, "[T]he probability that an unguided study would in a reasonable time consider these particular properties is infinitesimal." The only way to yield practical results, therefore, was to let the physical properties of the system guide the analysis. "This circumstance," he then added, "would seem to explain the frequent occurrence of situations in which mathematician members of research teams become

⁹⁴ E. C. Nelson, "The Role of the Mathematician in Industrial Laboratories," in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 78.

preoccupied with problems that the scientist members regard as unimportant.”⁹⁵ Either seduced by the mathematical properties of the model itself or simply unaware of the underlying physics, the mathematic members of the team would drift away from the original problem.

Interestingly enough in his description of the Applied Mathematics Laboratory at Stanford, Schiffer made a similar observation. After he explained that the group’s ultimate desire was to obtain elegant mathematical theory rather than brute force approximations, he added:

We are, therefore, adhering to our general research program in a loose fashion and do not mind if some members of our team get so interested in an abstract question as to lose sight of the applications altogether. We try to hold their interest by keeping them in the main stream of mathematical activity, otherwise letting them follow their bend, and wait until they get back to concrete problems after their fling in the abstract.⁹⁶

This generosity of spirit might have had a place in an academic environment, but the industrial laboratory was operating under different conditions.

A. A. Brown from Arthur D. Little Inc. consulting company also described the problems of integrating mathematicians into his research teams. The company, he explained, sold the ability of its employees to solve problems. The range of problems was quite wide and could include anything from “How can I stick two pieces of papers together firmly for a few minutes and then separate them easily and without damage?” to “Is my inventory policy correct under present circumstances?” However, “at no time is

⁹⁵ Ibid: 77.

⁹⁶ Schiffer, “The Applied Mathematics Laboratory at Stanford University,” 38.

the consulting firm faced with a client whose question is ‘What is the value of such and such definite integral?’”⁹⁷ The client’s problem needed first to be translated to quantitative language before it could be analyzed. Yet experience at the company had shown that scientists whose “basic orientation is toward concrete” were more reliable in being able to perform this translation work than those whose orientation was toward abstract thought. “Too often a mathematician tends to prejudge the problem, writing down a supposedly general solution much too early in the game. A laboratory training seems to lessen the tendency to do this.” Like Neslon before him, Brown suggested that mathematical training was inherently not a good preparation for industrial work.

Sometimes mathematicians just did not understand the task at hand. During his presentation, Harold Gershinowitz, the president of Shell Development Company, noted “it has been our experience...that is easier for a physicist to learn mathematics than for the mathematician to learn physics, or perhaps, to put it more kindly, to use his physical knowledge.”⁹⁸ Like the previous two speakers, Gershinowitz, argued that mathematicians were more often than not just too abstract, more interested in the mathematical theory than the physical problem. He illustrated this point with a concrete example. Recently his research group at Shell was interested in performing certain geophysical measurements. Specifically, the group was making correlations between very small deviations in the magnitude of gravity at the surface of the earth and variation in the density of the rock masses of which the earth was composed, their size and shape.

⁹⁷ A. A. Brown, “The Mathematician in an Industrial Consulting Organization,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 79.

⁹⁸ H. Gershinowitz, “Mathematicians in the Petroleum Industry,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 81.

Gershinowitz recalled that “at a fairly advanced stage of this problem it was discussed with a mathematician of world renown.”⁹⁹ Gershinowitz did not divulge the mathematician’s name, but he reported that “his only reaction was that there were an infinite number of solutions to any potential field of the kind we were dealing with which he proved in a few minutes, and that, therefore, we were wasting our time.” Yet, in the weeks that followed, members of the team were able to develop “computational techniques” that enabled them to produce “very useful estimate” of the rock masses they were modeling. “So much for why mathematicians are not more generally employed in industry,” Gershinowitz concluded. Different objectives and different understanding of the job precluded the mathematician from being of any help to the group. The group after all was satisfied with producing estimates, not exact solutions. And while the famous mathematician was able to quickly and effortlessly prove that such exact solutions were too numerous to compute, this was not the group’s objective. The physical investigation did not rely on exact ideal solutions. Good approximations were perfectly useful.

As they entered the workforce mathematicians faced an image problem. It was well recognized that strong mathematical skills were useful and that the ability to abstract was an important part of industrial work, but here also lay the danger. The tendency to deal with abstraction and the mental ability to do so was seen at once as both an intellectual and a social or personality proclivity. Pushed too far, abstraction was seen as a desire to disconnect from the world, an inability to deal with the concrete not just cognitively but socially, as if, when the mathematician abstracted the necessary characteristic of the world, he also removed himself from the world. This might explain why as the conference drew to an end the one thing almost all presenters (producers and consumers) seemed to agree upon

⁹⁹ Ibid: 82.

was a list of desired personality characteristics for the applied mathematician. If the applied mathematician was set apart from the engineer by his mastery of theoretical mathematics, it was via his personae.

As he began his presentation, William Prager suggested that it would be more productive to focus on the distinction between pure and applied mathematicians, than on the one between the pure and the applied mathematics. Acknowledging that his characterization might be somewhat exaggerated, Prager offered the following assessment:

In contrast to the pure mathematician who enjoys almost complete freedom in his choice of subject, striving for perfection and not minding if progress is slow, we are trying to form an applied mathematician who is more nearly an artisan or an engineer, appreciating perfection but able to forgo it in order to come up with reasonably good answers within a set time...The engineer is a man of action who deals with a given practical situation; he cannot wait indefinitely for the accumulation of complete data or the perfection of theories but has to base his decisions as best as he can on incomplete data or inadequate theories.¹⁰⁰

The distinction, as Prager himself noted, was a matter of temperament. Pure and applied mathematicians were defined in opposition to one another. If the pure mathematician was a perfectionist, a dreamer, and a freethinker, the applied mathematician was man of action who tended to compromise.

This sentiment that training applied mathematicians implies “forming” individuals with specific personality characteristics was repeated again and again by conference presenters. In *The Scientific Life*, Steven Shapin notes that in the post-World War II period

¹⁰⁰ Prager, “The Graduate Division at Brown University,” 35.

“the importance attached to the social virtues in the life of the industrial scientists,” implied that “a fairly standard list of twelve ‘personality traits’ that industry looked for in its recruits had evolved.”¹⁰¹ Thus, it is not surprising that in identifying the desired qualities of industrial mathematicians, participants in the conference emphasized many of the same traits described by research managers in industry. Yet the applied mathematician was constructed as much in opposition to the pure mathematician.¹⁰² It was his character that distinguished the applied from the pure mathematician, not his skills or theoretical arsenal. This process, of course, only served to reinforced and cement the view of the pure mathematician as the remaining relic of the ivory tower.

The first characteristic that was identified as crucial to the applied mathematician was a wide range of interests. Henrik Bode, who was the Director of Mathematical Research at Bell Labs, noted during his talk that “more important than actual breadth of training is breadth of interest.”¹⁰³ According to Bode, this was not a small matter. “As an index of probable flexibility and breadth of interest, I would regard even an undergraduate major in literature or philosophy as preferable to an undergraduate major in mathematics, for an applied mathematician with orthodox graduate training in mathematics.”¹⁰⁴ The applied mathematician had to be curious about the world around him and avoid narrow

¹⁰¹ Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation* (University of Chicago Press, 2008), 184–185.

¹⁰² David Kaiser argued that the expansion of physics in the postwar period transformed physicists’ self-image. Training a growing army of physicists, according to Kaiser, “reflected the need to maintain high outputs of certified workers, if not individual thinkers with a heightened sense of initiative.” That is, at the dawn of the Cold War what was needed were “workers” who could cooperate in teams, not free acting individuals. Modeling applied mathematicians, like physicists, as workers enable in turn pure mathematicians to maintain their self-image as free thinkers. David Kaiser, “The Postwar Suburbanization of American Physics,” *American Quarterly* 56, no. 4 (2004): 863.

¹⁰³ Henrik W. Bode, “On the Operation of a Mathematics Group in an Industrial Environment,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 87.

¹⁰⁴ *Ibid.*

specialization. Possibly such diverse set of interests would protect him from getting lost in the abstract, and bring him back to the world around him.

This breadth of interest had to be accompanied by a willingness to work on other people's problems. Tukey was not the only one who mentioned this as an important quality of applied mathematics. Mina Rees, who based her presentation on her experience as the head of the Mathematical Branch at the ONR, argued that "in general...the specific mathematical background is less important than the interests in applying it."¹⁰⁵ Strong mathematical skills by themselves were not enough. Without an accompanying desire to apply their expertise to specific problems arising out of industrial, military, or scientific considerations, applied mathematicians were ineffective. Unlike pure mathematicians, they did not have the freedom to follow their own whims guided by nothing besides their own intellectual curiosity.

Rees's assessment was not based only on her years at the ONR. Her war experience at the Applied Mathematics Panel was also influential in shaping her ideas about the nature of applied mathematics work. In an interview almost four decades after the war, Rees explained, "[R]esearch people are very jealous of their freedom to pursue their research independently of direction." She recalled that "at the beginning of World War II, there was a general feeling among mathematicians that they didn't want to work with industry, and of course, those who went to work for the military confronted many of the same problems that they would have confronted in industry. In the sense that they were asked to solve

¹⁰⁵ Mina Rees, "The Mathematician in Government Establishments," in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 57–59.

problems that the military people were apt to define.”¹⁰⁶ Explaining that she saw her own position as a sort of mediator between professional mathematicians and military officers, Rees continued, “I had a really unique opportunity to be closely associated with mostly men, who were trying to solve other people’s problems. In spite of the fact that their whole mindset was on doing things that they had defined themselves.”¹⁰⁷ As autonomy came to demarcate academic research from industrial work, it also served to divide the pure from the applied mathematician.

The ability to work as part of a team was also singled as an important aspect of applied mathematics work. Prager exclaimed that “working as a member of a research team constitute as essential part of the education of an applied mathematician.”¹⁰⁸ Unlike research in physics, which in the postwar period became increasingly characterized by large-scale collaborations, mathematical research was still conducted for the most part on an individual basis. Thus, the ability to work as part of a team was another important characteristic that distinguished the applied from the pure mathematician. Commenting on the nature of teamwork, Bode explained that certain qualities were especially desirable in applied mathematicians, among which he listed, “reasonable gregariousness and the ability to get along with other people.”¹⁰⁹ The pure mathematician could afford to be a recluse, but not the applied mathematician.

H. J. Miser, who represented the Operation Research group at the Air Force, added that the applied mathematicians “must have a flair for exposition, both in speech and

¹⁰⁶ Interview with Mina Rees, Women in the Federal Government Oral History Project, Call Number OH-40, Schlesinger Library, Radcliffe Institute, Cambridge, MA.

¹⁰⁷ Ibid.

¹⁰⁸ Prager, “The Graduate Division at Brown University.”

¹⁰⁹ Bode, “On the Operation of a Mathematics Group in an Industrial Environment,” 87.

writing.”¹¹⁰ Concluding his talk, A. A. Brown from Arthur D. Little Consulting remarked that applied mathematicians “must be able to communicate in both directions with their fellow workers, i.e., they must be willing to listen and be able to talk.”¹¹¹ W. W. Leutert, who was working at the Computing Laboratory at the Aberdeen Proving Ground, went as far as to suggest that any mathematician who was considering a professional career in mathematics participate in a “seminar on applied psychology in order to improve their ability in handling people and meeting them face to face.”¹¹² Being able to formulate a given problem in mathematical terms or even solve it was not enough. As they moved out of the university and into industry, mathematicians also had to learn how to communicate their ideas to non-mathematicians. They had to be able to explain their work not only to the scientists and engineers, but also to the non-scientific members of the group.

Conference attendees were unable to agree on what applied mathematics was. They had different opinions as to what training in the field should entail. How much mathematical knowledge was necessary? Or what degree of training in other sciences or engineering was crucial? These questions remained unanswered. Further, they could not really decide what was the goal of training in applied mathematics or what was the mathematician’s job in industry. The one thing they all seemed to coincide on was what his desired personality characteristics and temperament were and how he differed from the pure mathematician. This was evident in Fry’s concluding remarks:

The difference between an applied mathematician and a pure mathematician is not the kind of mathematics he knows, it isn’t even whether he can create epoch-making

¹¹⁰ H. J. Miser, “The Role of Mathematicians in Operations Research in the United States Air Force,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 66.

¹¹¹ Brown, “The Mathematician in an Industrial Consulting Organization,” 80.

¹¹² Miser, “The Role of Mathematicians in Operations Research in the United States Air Force,” 61.

new ideas, or like most of us his ability lies principally in interpreting things that are already known. The distinction resides instead in the nature of his interests; in his attitudes, not in his aptitude. It is almost a social distinction.¹¹³

However, if the only difference between pure and applied mathematicians was a “social distinction,” the question of how to train applied mathematicians became how to produce mathematicians whose interests and attitudes were those desired.

Conclusion

In 1958, Rees published an article in *The American Mathematical Monthly* entitled “Mathematicians in the Market Place.”¹¹⁴ The aim of the article, according to Rees, was to describe the nature and extent of the need for mathematicians in industry and government.¹¹⁵ In the years that followed, mathematicians still failed to secure what Rees conceived of as their rightful place among industrial scientists. Whereas in 1941, Fry estimated that only 150 mathematicians were working in industrial positions, by 1956 a national report estimated that the number of men and women thought of by their employers as mathematicians was anywhere between 7000 and 8000. This of course was a huge overestimation. The key was that many of those counted might have occupied the position of a mathematician within a given organization,

¹¹³ Abraham H. Fry, “Applied Mathematics as a Responsibility of the Mathematical Profession,” in *Proceedings of a Conference on Training in Applied Mathematics: Sponsored by the American Mathematical Society and by the National Research Council*, ed. Fritz Joachim Weyl (National Academies, 1953), 96.

¹¹⁴ Mina Rees, “Mathematicians in the Market Place,” *The American Mathematical Monthly* 65, no. 5 (1958): 332–343.

¹¹⁵ In the article Rees published, reporting on results of the committee’s work, however, she did not focus on the sort of mathematical and scientific training, namely the type of courses and research experience, which were suitable for industrial mathematicians. Instead, she emphasized his personality traits: “The *first* consideration is the question of personality,” she exclaimed. “Unless a man enjoys working with others, unless he is interested in considering other people’s problems, unless he finds it interesting to evolve the appropriate mathematical model for handling situations that are often not correctly or clearly described, and to bring his mathematical maturity to bear on situations he has not himself selected he probably does not belong in industry.” *Ibid.*

but they were not trained as such. More likely, they were physicists or engineers with strong mathematical abilities. A more conservative estimation, which was based on the number of those mathematicians who received their PhD during the preceding four decades, put the number at approximately 900 mathematicians. This huge gap in numbers served only to further the claim that more mathematicians needed to be trained to assume industrial positions. Rees, who was well enmeshed in the development of mathematics in government and industry, suggested that “mathematicians themselves have not recognized the strength of their position.”¹¹⁶

The article was based on a report by a Sub-Committee on Non-Teaching Opportunities for mathematicians Rees chaired. The Sub-Committee was part of a national two-year survey on Research Potential in the Mathematical Sciences. Its final report suggested, for example, that in order to increase awareness of these opportunities, students should visit industrial establishments, and that industrial mathematicians should write and widely distribute papers illustrating some of the uses of mathematics in non-academic environments. University mathematicians were just not aware, according to Rees, of the numerous research opportunities awaiting them outside the walls of the ivory tower. “Unlike the chemist (whose experience in industry goes back several generations, and whose production in industry is usually a concrete product) the mathematician has ideas to sell. There is useful missionary work to be done on the job by the articulate mathematician who understands his role well enough to be a missionary.”¹¹⁷ However, not everyone believed in the missionary role mathematicians had to play in industry. The late 1950s saw a small increase in the percentage of Ph.D.’s who sought non-academic employment. However, as will be described in greater detail in the following chapter, the during

¹¹⁶ Ibid.

¹¹⁷ Committee on the Survey, *A Survey of Training and Research Potential in the Mathematical Sciences : Final Report. Part II.* (Chicago, IL: University of Chicago, 1957), 47.

the 1960s, the percentage of doctorates seeking non-academic only decreased and remained less than twenty percent. Only in the early 1970s, with the collapse of the job market, did mathematicians once again begin to eagerly look for employment outside of the university.

Chapter 4.

A Well-Ordered Set: The Growth of the Mathematical Sciences

“We relaxed the other evening over a cocktail or two with a small and subdued group of mathematicians who had just put in a day trying to explain some of the fundamentals of modern higher mathematics to a large and perky group of writers on science matters.” So began a *New Yorker* short piece entitled “Mathematicians” in the November 16 issue, 1963. The cocktails were served at the end of a seminar at Columbia University, which was organized under the joint auspices of the Society of Industrial and Applied Mathematics (SIAM) and the Research Institute of Advanced Studies, an affiliate of the Martin Company. In the evening after the meeting concluded, the *New Yorker* writer joined the mathematicians and the science writers at the Statler Hilton’s Ivy Suit, where upon entering the room he ran into Jerry Clemans. Clemans, a representative of the Martin Company, immediately warned the writer not to be misled by the seemingly relaxed atmosphere:

“The process of communication has been a very painful one,” he said, in a voice that was only slightly larger than a whisper, “and the mathematicians are all pooped out. One of them was so tired that he had to go home to rest up.” Mr. Clemans then presented us to three of the participants in the pooping briefing, and we soon discovered that all three were harboring doubts about their day’s work, though each one was being gnawed by a different worry.¹

What accounted to this failure of communication? Why were the mathematicians so “pooped out”? And what made them doubt their day’s work?

¹ “Mathematicians,” *The New Yorker*, November 16, 1963.

The first mathematician, the *New Yorker* writer encountered was Joseph LaSalle. The question that troubled LaSalle, who had just retired as president of SIAM, was “How can mathematicians and non-mathematicians talk to each other?” Mathematics, LaSalle noted, is much more difficult to comprehend than other sciences. Rudolph E. Kalman, the second mathematician encountered, was bothered by a different question, namely, “How can mathematicians talk among themselves?”² According to Kalman, increased specialization made it practically impossible for any individual mathematician to understand more than a minor fraction of research in the field. Richard E. Bellman, the third mathematician with whom the *New Yorker* writer chatted was disturbed by yet another question, “Why should mathematicians *want* to talk to non-mathematicians?” Bellman, a RAND employee, felt that not only did most scientists conceive of mathematicians as magicians, but that mathematics had no real application to the real world. “Shaken by these gloomy assertions,” the writer concluded, “we turned instinctively to the bar, feeling that we, too, merited, a little de-pooing.”³

The appearance of such a portrayal of mathematicians on the pages of *The New Yorker* in 1963 was bad news for the mathematical community. The aim of the seminar at Columbia University was to demystify modern mathematics and make it more accessible to science writers and the general public, not to make it seem even more bewildering and disorderly than it already

² In 2009, Kalman was awarded the National Medal of a Science from President Barak Obama for his work on signal processing.

³ The article in fact closed on a more positive note when the *New Yorker* writer met Bernard Friedman of the University of California Berkeley. “Suddenly all barriers to communication between mathematicians and non-mathematicians melted away.” Friedman discussed with the group of journalist the distinction between pure and applied mathematics, and claimed that most work done in industry is not really considered mathematics. To convince the group, Friedman offered that most Madison Avenue artists do not consider their work art. The best work these days, Friedman further argued is done by pure mathematicians. “It’s the difference between Jackson Pollock and Norman Rockwell,” and most students in mathematics are more attracted to pure mathematics in the same way that contemporary artists find excitement in Abstract Expressionism. It was through its analogy to art rather than science that mathematics transcended communication. *Ibid.*

appeared to most non-specialists.⁴ Though the tone of the piece, which appeared in the Shouts and Murmurs section of the magazine, was humorous, its author, nonetheless, honed in on some of the main questions that troubled the mathematical community in the 1960s. How could mathematicians communicate their ideas among themselves, to other scientists, and to non-specialists?

Throughout the 1960s American mathematicians found themselves in a curious position. On the one hand, mathematical theories and methods become applicable in growing domains of fields, a process they christened the “mathematization of culture.”⁵ On the other hand, modern mathematics, which consisted of the lion’s share of the work produced in mathematics departments, was ever more abstract. Modern mathematics, it was asserted, developed according to its internal coherence independent from so-called real world concerns.⁶ To account for these two seemingly opposing tendencies that characterized the growth of the field, a new term of art – the mathematical sciences – gained credence. Mathematics, no longer a noun, became an adjective. Partly, this shift in terminology reflected mathematicians’ response to national policy demands. However, it also reflected a conscious effort by the mathematical community to come to terms with the field’s transformation in the two decades following World War II.

⁴ The seminar was also covered by John Osmundsen for *The New York Times*. Unlike the *New Yorker* writer, Osmundsen seemed to have engaged more seriously with the mathematical content covered during the seminar. The article he published provided a short introduction to the mathematical theory of sets. John Osmundsen, “2 Key Mathematics Questions Answered After Quarter Century: Proof Concerns Theory of Sets, Widely Used in Teaching Beginners -- Work Is Discussed at Seminar,” *The New York Times*, November 14, 1963.

⁵ The “mathematization of culture” is a repeated refrain in the official report of the Committee on Support of Research to the Mathematical Science, which I discuss at length in the second part of the chapter.

⁶ In its most extreme this image of modern mathematics was promoted by Marshall Stone. In “The Revolution in Mathematics,” which appeared in both the *Bulletin of the Association of American Colleges* and the *American Mathematical Monthly*, Stone wrote that the most important change in the modern conception of mathematics was “the discovery that mathematics is entirely independent of the physical world.” He then added, “mathematics is now seen to have no necessary connection with the physical world beyond the vague and mystifying one implicit in the statement that thinking takes place in the brain.” Marshall Stone, “The Revolution In Mathematics,” *American Mathematical Monthly* 68, no. 8 (1961), 716.

The new conceptualization of the field under the term the mathematical sciences provided a coherent and unified image and denoted the forging of new allegiances among members of the mathematical profession. Statistics, computing, management sciences, operation research, mathematical economics, mathematical biology, as well as abstract algebra, topology and number theory were subsumed under the “mathematical sciences.” The term, that is, introduced a more harmonious image of the field itself. However, as I show in this chapter, this new image of the field that aimed to provide a more integrated view of mathematics and application only had the counter-intuitive effect of further separating “pure” mathematics from applied mathematics and mathematical applications. The view of the mathematical sciences that came into prominence during the 1960s was predicated on a specific philosophy that conceived of the relation between mathematical and physical theories as bordering on the mysterious. This view, most famously made by physicist Eugene Wigner, only served to further cut the ties between mathematics and the rest of the sciences. The most notable effects were the cordoning off of (pure) mathematics as an academic pursuit and the neglect of classical applied mathematics.

Enabled by the sharp increase in the NSF budget in the aftermath of Sputnik and the passing of the National Defense Education Act of 1958, the mathematical community underwent a vast explosion in both scale and scope during the 1960s. In the process, “mathematics” was no longer an inclusive enough category to describe all the new areas of research that had emerged over the preceding two decades. Instead, the “mathematical sciences” began to take its place in national discussions. This change in terminology arose out of mathematicians’ desire to take an active role in national policy and to alter the public image of mathematics. The “mathematical sciences” was means by which to reflect the plurality of new areas of research that now laid claim to the mathematical terrain. Yet it also reflected mathematicians’ ongoing concerns about

the “proper” boundaries of the field. Despite the use of the term it was not necessarily clear what exactly were the mathematical sciences, or what united them together.

After outlining the rise of the mathematical sciences as a new term of art, the chapter takes its lead from the *New Yorker* writer and his frustrated mathematicians. In “How can mathematicians talk among themselves?” I examine the new conceptualization of the field mathematicians promoted under the rubric of the mathematical sciences. The section follows the work of the Committee on Support of Research to the Mathematical Sciences (COSRIMS), which was in charge of making policy recommendation to the Federal Government. Members of the Committee invested a lot of energy in defining the confines of the mathematical sciences. They even published a collection of essays written by leading mathematicians to accompany the official report. The coherent image of the mathematical sciences that the Committee advanced aimed to convey the importance of mathematics to the health of the nation, to present a view of modern mathematics to other scientists, and to help mathematicians come to terms with the transformations in the field.

“How can mathematicians and non-mathematicians talk to each other?” examines the impact of this new conception of the field on the development of (classical) applied mathematics. Training in applied mathematics continued to trail behind during the 1960s. The mathematical sciences offered a much more fragmented view of applied mathematics than existed in previous decades. In addition the vision of the mathematical sciences presented in the report was predicated on a philosophy that there was no way to predict which mathematical theories would have real-world applications. Mathematicians could not agree whether comprehensive training in the field was possible. Finally, “Why should mathematicians *want* to talk to non-mathematicians?” argues that the expansion encompassed by the mathematical sciences only

resulted in the further cordoning off of pure mathematics as an academic pursuit. If in the 1950s, applied mathematics was used as an umbrella category to encompass all the various fields of mathematical application that existed, by the mid-1960s many of these new areas of research were beginning to be established as independent fields. The growth of computer science, statistics, and operation research during the 1960s, all of which were subsumed under the mathematical sciences, was characterized by increasing autonomy. Thus, to a certain degree the successful growth of the mathematical sciences also brought their dissolution. As a consequence, “mathematics” could safely return to its prewar state.

Whereas the various sections of the chapter are organized around these independent questions of communications, they are in no way mutually exclusive. In trying to convey the nature of modern mathematics and the confines of the mathematical sciences to the public, mathematicians had to first agree and define for themselves the constitution of their field. Similarly, in establishing the scope of their field, mathematicians had to account for its relation to other scientific fields.

The Mathematical Sciences

In the decade following World War II, the American mathematical community was growing steadily. By the end of 1947, the number of annual PhD's reached its prewar height of about a hundred graduates per year, and continued to grow until it settled at an average of about 250 PhD's per year between 1955-1959. Yet this expansion was in no way comparable to the one experienced by the physical or the biological sciences during the decade and a half following the war. Sputnik changed it all. For the first time since the war, the mathematical community became fully integrated in the scientific enterprise. According to Roger Geiger, “the reaction to

Sputnik, at bottom, resulted in an endorsement of the case for disinterested academic research.”⁷

It was this philosophical change underlying the national support policy for science, and the passage of the National Defense Education Act, that had the greatest effect on the growth of mathematics in the following decade. Overnight the available funding for mathematics multiplied and a demand for mathematical manpower intensified.⁸ Within this new climate, the number of mathematics PhDs was suddenly singled out as being low not only in relative terms, but in absolute ones as well.

During the decade the number of annual PhDs per year quadrupled from 303 in 1960 to 1,204 in 1970. This increase was enabled by the establishment of new doctorate programs in mathematics. In the course of only four years starting in 1962, 41 new PhD-granting programs in mathematics were founded around the country.⁹ The expansion, of course, depended on increasing support from the federal government (see Table 1). In the course of four years, the NSF budget for mathematics tripled from \$3.8 million in 1960 to \$11.4 million in 1964 (see Figure 2). Mathematical publications similarly mushroomed over the decade. The *Mathematical Review*, the prominent reviewing journal in the field, experienced more than a 50% increase in size from 1960 to 1961 alone. Whereas the number of reviews published each year averaged around 7,500 from 1955 until 1960, by 1961 the number of reviews rose to 13,382.¹⁰ This increase in size was accompanied by a similar increase in scope. Computing, statistics, operation

⁷ Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities Since World War II* (New York, NY: Oxford University Press, 1993), 165.

⁸ As will be discussed in greater detail in the next chapter, the demand for mathematics PhDs was most forcefully stated in a report by President’s Science Advisory Committee published in 1962. The report called upon the mathematical community to increase the production of doctorate degrees confirmed each year, and set decade-long goals of more than tripling the annual number of PhDs. United States President’s Science Advisory Committee, *Meeting Manpower Needs in Science and Technology: A Report* (Superintendent of Documents, U.S. Govt. Print. Off., 1962).

⁹ John William Jewett et al., *Report: Aspects of Graduate Training in the Mathematical Sciences*, vol. II, Conference Board of the Mathematical Sciences: Report of the Survey Committee (Washington, D.C.: Conference Board of Mathematical Sciences, 1969).

¹⁰ “Annual Report – Mathematical Reviews,” AMSR, Box 16, Folder 71.

research, and a host of new fields such as mathematical economics and biostatistics grew in size and began to be acknowledged as independent fields of research. The mathematical terrain was undergoing a simultaneous explosion in both scale and scope. In response many members of the mathematical community believed that a more forceful representation of mathematics on the national level was required; one that would reflect more accurately the changing constitution of the field. In the process the term “the mathematical sciences” began to emerge as a convenient way of recognizing the ongoing transformation in the field.

Agency	1960		1962		1964		1966	
	Amount	%	Amount	%	Amount	%	Amount	\$
NSF	3.8	100	7.4	100	11.4	100	14.9	100
Army	2.2	81	2.6	47	2.9	35	2.4	28
Navy	4.8	70	4.6	28	6.7	26	5.9	22
Air Force	2.0	40	3.0	29	5.4	26	6.2	19
AEC	2.8	88	3.9	95	4.9	96	6.3	97
NASA	1.0	100	[0.7]	[4]	5.4	86	7.0	92
	16.6		21.5		36.7		42.7	

Table 1. Federal Support to Mathematical Sciences. The table lists the total funds (in millions) afforded to basic research in the mathematical sciences in the 1960s as well as the percentage it represented out of each agency’s total budget to the mathematical sciences. It is important to note that the funds were not all directed toward academic research, but rather to the agencies’ laboratories. Nonetheless, two points are evident from the table. First, the growing appropriation of funds to the mathematical sciences: not only did the NSF budget almost quadruple, but a growing number of agencies began supporting basic research in the mathematical sciences. NASA’s budget alone increased from \$1 million to \$7 million. Second, as the percentage columns indicate, the increase in overall funds was accompanied by a decrease in the percentage of the budget afforded to basic research. In 1960, more than 80% of the Navy’s budget went to basic research in the mathematical sciences, but by 1966 the percentage had dropped to 28%. The table is reproduced in part from: *The Mathematical Sciences: A Report*, 164.

The term “the mathematical sciences” had of course been used before. Yet, in this section, I point to the increased use of term in the 1960s as a way of reflecting the plurality of new fields that now made claim to be mathematical. Specifically, I wish to draw attention to the

rise of the term in response to concerns about national policy. It was in this domain, that “mathematics” was deemed insufficient to reflect the huge transformation the field had undergone in the previous two decades.

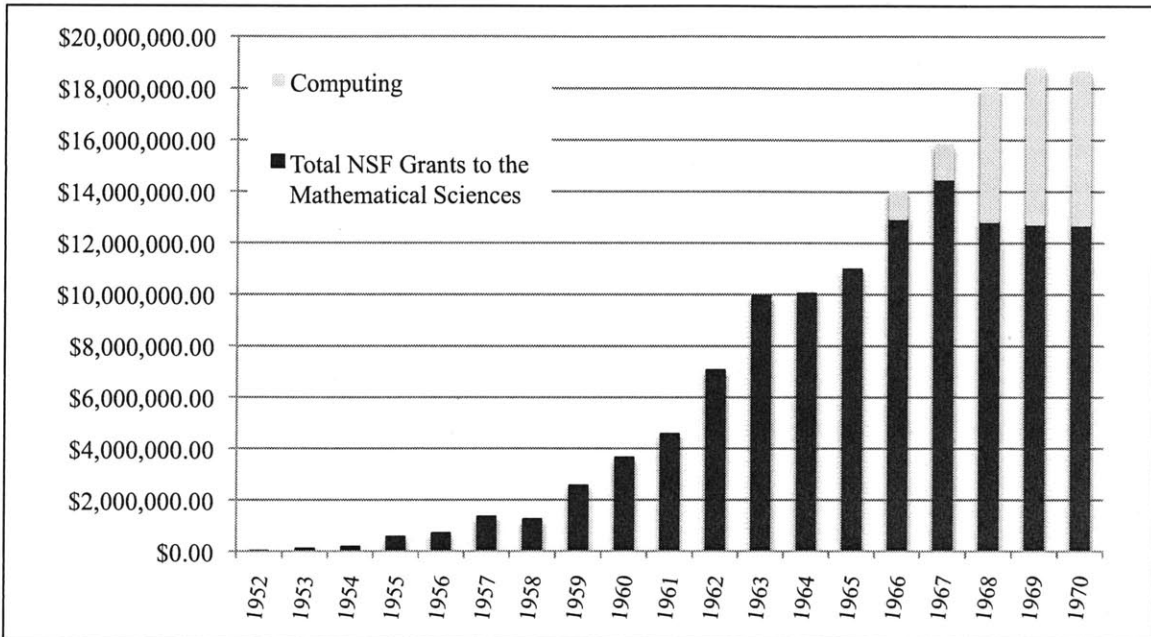


Figure 2. Total allocation of NSF funds to research grants to the Mathematical Sciences. The funds indicated in the graph above do not include other forms of support for fellowship, seminars, education initiatives, and publications, which also received funding by the NSF. As the total funds available for research increased so did the number of grants. For example, in 1957 the annual funds were distributed by 64 projects at an average of \$21,067 per grant. A decade later the number of unique projects funded increased by more than a factor of six, with 406 projects at an average of \$31,527. 1966 was the first year in which the funds for computing were indicated separately. Moreover, in the late 1960s a little less than one-third of the total funds were given to research grants in applied mathematics and statistics. More than two-thirds was allocated to other mathematical fields such as algebra, analysis, topology, etc. The information for the above table was compiled by the authors from the published NSF annual reports in the period.

At the end of World War II, the leading mathematical organizations in the country, established the Policy Committee for Mathematics. The Policy Committee was a direct continuation of the War Preparedness Committee and consisted of representatives from the Society, the Association, the Institute of Mathematical Statistics, and Association of Symbolic Logic. The Policy Committee was conceived of as a national body representing the interests of

the mathematics profession as whole, though its activities revolved for the most part around selecting and nominating mathematicians to various national organizations. It was widely acknowledged that it did not accomplish much in terms of policy. Frustrated with the ineffectiveness of the Policy Committee, in 1957, several mathematicians proposed that a new umbrella organization, the Institute for Mathematics, would be founded modeled on similar national professional organization by physicists and chemists. Whereas in the prewar era, the membership of the *Society* and the *Association* covered almost all professional mathematicians (either in teaching or research positions) in the United States, the 1950s and the 1960s saw the rise of new professional organizations dedicated to operation research, computing, industrial mathematics, management sciences, and high school teaching. The idea behind the proposal was to have a central organization that would provide leadership across the mathematical professions. The new organization was also seeking to incorporate the Society of Industrial and Applied Mathematics (SIAM), which had been established three years earlier in 1954. Yet, at least at first, not everyone in the mathematical community conceived of this move in positive terms.

In March, Marston Morse wrote to G. Baley Price, who was the president of the *Association* and was the main force behind the new proposal, protesting against the new organization. Morse had just finished reading the recent minutes of the Policy Committee for Mathematics, and he was disturbed by what he read. “It is stated,” he wrote to Price, “that ‘no organization is able to speak in the name of all mathematics and all mathematicians.’ This is probably true on most subjects and is as it should be.”¹¹ According to Morse, there was no reason that the “Industrial Mathematics Society” and the AMS would agree in their recommendation to the NSF “as to what is fundamental in mathematics.” Illustrating his point, Morse added, “one

¹¹ Marston Morse to G. Baley Price, 18 March 1957, MMP, Box 1, Folder A.

of my children recently brought home a book on the reasons for studying mathematics, written by the General Electric Company... the reasons given were those for being a General Electric engineer.”¹² This sort of publication, Morse reasoned, might be sponsored by the industrial mathematicians but not by the AMS.¹³

Saunders MacLane had similar doubts about the desirability of a new organization. MacLane represented the Society at the meeting of the Policy Committee in which the proposal to establish the new organization was discussed. He later wrote to Richard Brauer, who was serving as the president of the AMS, “I submit that this new organization is not only needless but bad, and that it is inspired by bureaucratic habits of thought.”¹⁴ MacLane reasoned that the new proposed organization would not add anything to the mathematical profession and would only increase the administrative duties of all involved. Like Morse before him MacLane was skeptical of the need to unify mathematics. Following his letter to Brauer, he began a correspondence with Albert Meder, a mathematician at Rutgers University, who was at the time the executive director of the Commission of Mathematics at the College Entrance Examination Board. Meder was more sympathetic to the idea of establishing a new organization. He wrote to MacLane, “it seems to me incontrovertible that we must emphasize increasingly the unity of mathematics, rather than the separate aspects of the American mathematical enterprise.” When it came to policy

¹² Ibid.

¹³ Two years later, Morse took a more active stand against such publications. Morse was sent a draft of a brochure on the nature of modern mathematics produced by the National Council of Teachers of Mathematics in collaboration with the NAS-NRS for examination. In a letter to Robert B. Garrabrant, who was working as part of the education project in the NSF, Morse explained that he could not approve of the brochure in its present form. According to Morse, the brochure provided a distorted picture of mathematics. “It devotes but *eleven* lines to basic mathematics,” and instead has a strong “materialistic tone.” Morse objected that all the brochure did was establish the fact that career options were available to young applied mathematicians, a reality, he noted, that could be demonstrated by simply reading the advertisements in daily newspapers. Morse was upset: “*The problem of presentation of mathematics is not a Madison Avenue problem of selling glamorous positions (already oversold) but of overcoming ignorance.*” (Emphasis his.) He recommended that the brochure should be revised and that a new author be commissioned to describe modern pure mathematics. See: Marston Morse to Robert B. Garrabrant, 12 June 1959, MMP (106.12), Box 1, Folder “1951-1960.”

¹⁴ Saunders MacLane to Richard Brauer, 12 August 1957, AMSR, Box 43, Folder 121.

declarations, Meder argued, it would be best if the mathematical community “speak with unified voice.” MacLane disagreed. “Why Unity?” he asked Meder. It is “more important to say the *right* things,” he wrote, “than to be *unified* on the wrong things.”¹⁵ Like Morse, MacLane was not convinced that the various groups who now laid claim to the mathematical landscape could agree on matters of national policy.

In his letter to Price, Morse stated that there were three fundamental groups in the country: “those concerned with basic mathematics,” “those concerned with teaching,” and “various societies which are concerned with applications.”¹⁶ Morse predicted that with time the AMS, which was still the largest professional organization for mathematicians, would actually become the smallest one. The other societies dedicated to the applications of mathematics, he explained, would eventually represent the majority of American mathematicians. “Unless the historic development of mathematics is to be inverted and the tail wag the dog, the first group [AMS], having great intelligence and ideals, must at least remain *independent*.”¹⁷ Both Morse and MacLane were concerned that pure mathematicians would lose their voice in light of the growing number of new mathematical applications. This turned out to be far from the case. The explosive growth of the profession in the 1960s was characterized by the growth of pure mathematics. Heeding Morse’s warning, mathematicians kept their independence. The tail, it appears, was wagging the dog.

Despite Morse’s and MacLane’s protests, by 1960, the different mathematical organization in the country incorporated under the Conference Board of the Mathematical

¹⁵ Albert Meder to Saunders MacLane, 10 September 1957; and Saunders MacLane to Albert Meder, 7 October 1957, AMSR, Box 43, Folder 121.

¹⁶ Marston Morse to G. Baley Price, 18 March 1957, MMP, Box 1, Folder A.

¹⁷ *Ibid.*

Sciences (CBMS).¹⁸ The launching of Sputnik I by the Russians and the resulting increase in federal funding afforded to the sciences weakened the resistance to such an organization, The Conference Board, chaired by G. Baley Price, opened its office in Washington and began its operations.

The move to Washington was significant. The goal of the Conference Board was to represent the mathematical profession on a national level and to ensure that mathematicians got to influence various science policies as they affected the development of the field. In 1961, for example, Price published an article in the *Notices of the AMS* informing mathematicians on the role of the Conference Board. Under the heading “Mathematics and the Washington Scene,” Price explained that “three types of activities in Washington concern mathematics.”¹⁹ These were the planning of programs in support of mathematics, the operation of these programs, and the collection of data and information about the profession. “Mathematics is in a transition period,” Price proclaimed. No longer restricted to teaching and university research, mathematics was now at the center of ongoing scientific and technological transformations. It was therefore important, according to Price, that “mathematicians examine and evaluate the information and data that concern mathematics.”²⁰ It was this task that the Conference Board was set to accomplish.

The Conference Board might have arisen out of policy considerations, but it also represented a new conception of the field – encapsulated in its use of the term the mathematical sciences in its title. From 1957, when the idea of a new organization was first suggested, until 1960 when it was officially established, the name of the organization changed from the Institute

¹⁸ The member organizations were AMS, the Mathematical Association of American, the Society for Industrial and Applied Mathematics, the Institute for Mathematical Statistics, the Association for Symbolic Logic, and the National Teachers Mathematics Association.

¹⁹ Baley G. Price, “News and Comments from the Conference Board of the Mathematical Sciences: Mathematics and the Washington Scene,” *Notices of the American Mathematical Society* 8, no. 1 (February 1961): 17–19.

²⁰ *Ibid*, 19.

for Mathematics to the Conference Board of Mathematical Sciences.²¹ The change in title is revealing. “Mathematics” was no longer deemed sufficient to describe all of the mathematical activities that were taking place around the country. In contrast, the mathematical sciences could encapsulate a wider arrange of fields under one organization. For example, in 1962, the Association for Computing Machinery joined the Conference Board. The term the “mathematical sciences” was indicative of the transformations that took place in the American mathematical profession in the aftermath of the war. It was a way of accounting for growth of the field in both scale and scope. From the standpoint of national policy, the change also points to mathematicians’ wish to align the field with the sciences. Following a meeting of the Conference Board in 1961, Tukey wrote to Price, “members felt that it was important that mathematics be regarded as a Science and not alone as a humanity.”²² The “mathematical sciences” became a way of fending off criticism and signaling to policy makers the role of mathematics within the scientific enterprise.

In his letter to Price, Tukey also mentioned that those in attendance “felt that Mathematics (with a capital M) should have a due share in the development of broad national policies for science.”²³ He reported that during the discussion that followed members discussed the possible strategies for producing responsible policy advisors. Tukey felt inclined to draw some attention to the possible implication of the increased involvement of mathematicians in

²¹ For a while the name for the proposed organization was Mathematics Conference Organization. Unfortunately, I am not able to date exactly when the new name was adopted, nor can I recreate the discussion surrounding the change in name. Nonetheless, the shift is revealing, especially, when considering that the other major body that was in charge of representing mathematicians of the national level was the Division of Mathematics at the NAS-NRC. Representation at Division of Mathematics was proportional to organizational size. Thus, members of the AMS always represented the majority of the Division. Moreover, disputes about inadequate representation for applied mathematicians were a constant refrain in the two decades following the war.

²² John Tukey to G. Baley Price. 23 February 1961, JWTP, Series I. Correspondence. Box 26, Folder “Price, G. Baley.”

²³ Ibid. Tukey’s “Mathematics with a capital M” also points to the changing conception of the field.

matter of national policy. If mathematics was to gain influence and become more integrated with the rest of the sciences, Tukey reasoned, it was inevitable that some policy decisions regarding the field would be made without the direct input of mathematicians. It was therefore “urgent,” Tukey concluded, that mathematicians “educate enough of the outstanding scientists in all fields about the nature of mathematics.” Only such “education” would ensure that scientists would make adequate policy decisions when it came to mathematics.

The so-called need to “educate” non-mathematicians about the nature of modern mathematics was also acknowledged by members of the Conference Board who spent part of their meeting discussing what they conceived of as “the public relations problem of mathematics.”²⁴ A few days after he received Tukey’s letter, Price replied. He was especially interested in Tukey’s views regarding a recent proposal by the Conference Board to establish a public relations office for mathematics. Price wanted to know if the proposed office would possibly provide a solution to the sort of concerns raised by Tukey.²⁵

But what was the “public relation problem of mathematics”? In his letter, Price mentioned an article by Warren Weaver that had recently appeared in *Goals for America*, a 1960 report of the President’s Commission on National Goals. The article, entitled “A Great Age for Science,” can be read as Weaver’s defense of the ideals of basic research. In it Weaver promotes a view of science as both an intellectual pursuit and a source for social and technological change. Weaver discusses science writ large, but in a section on education, he singles out mathematics. On one extreme, he writes, “there exists a school of thoughts that takes altogether too precious an attitude towards research, belittling applied science as though it were stupid and inelegant.”

²⁴ Ibid.

²⁵ G. Baley Price to John Tukey, 28 February 1961, JWTP, Series I. Correspondence, Box 26, Folder “Price, G. Baley.”

Weaver then adds, “these are persons who still insist that ‘applied mathematics’ is necessarily several cuts below ‘pure mathematics’.”²⁶ Despite the growing number of applications and the increased use of mathematical techniques in an increasing domain of fields, mathematicians were unable to shake the image of detachment. Weaver’s criticism of a large section of the mathematical community appeared in a national report next to essays by some of the leading public intellectuals of the period.²⁷ For Price, such a portrayal of sectors of the mathematical community was emblematic of mathematics’ public relation problem.

Mathematicians were right to worry. In October 1962, Tukey circulated a “think piece” among members of the President’s Science Advisory Committee’s Panel on Scientific Manpower. The piece, entitled “If the Government is to Influence the Distribution of Graduate Students Among Fields, What Means Should It Use?”, considered the ways by which the government could successfully influence the distribution of students across scientific fields. During the second half of the 1950s and into the early 1960s, the number of bachelors degrees in mathematics was rising faster than in any other field.²⁸ While this was not the case when it came to the number of doctorate degrees in the field, which were rising but at a slower rate than in other scientific fields, according to Tukey, some people were troubled by these changes. “Some may be concerned,” he wrote, “about the rapid increase in the fraction of MPE (mathematics, physical sciences, engineering) bachelor’s degree taken in mathematics and, particularly, about a

²⁶ Warren Weaver, “A Great Age for Science,” in *Goals for Americans: Programs for Action in the Sixties*, ed. United States President’s Commission on National Goals (Prentice-Hall, 1960), 118.

²⁷ *Goals for America* included essays by John McCloy, the former president of the World Bank; Clark Kerr, the president of Berkeley University; and Thomas J. Watson Jr., president of IBM, as well as many other public intellectuals of the period. The essays in the volume were on a broad array of topics from the democratic process to US foreign economic policy.

²⁸ From 1955 until 1960, the number of BAs awarded in mathematics and statistics increased from 4,034 annually to 11,437. For comparison, BAs in physics increased from 10,516 to 16,057 during the same period. By 1966, mathematics BAs surpassed those in physics.

possible extension of this phenomena to the doctor's degree."²⁹ According to Tukey, this disquiet was not due to a fear that students who in the past pursued a career in physics would now pursue one in mathematics. Nor was it a question of distribution, a worry that the increase number of mathematics bachelors would alter individual careers. Rather, Tukey argued, it was due "to a feeling that modern mathematics has gone away from closely physical-science related fields of classical mathematics, that it has lost contact with science, and became an 'art form' pursued for its own sake."³⁰ Despite the obvious significance of mathematics to the physical sciences and engineering, the concern was that students who chose to advance training in mathematics would be pushed away from the sciences, not toward them.

The public image of mathematics was not just a matter of cartoonish portrayals of mathematicians in popular magazines such as *The New Yorker*. National policy was at stake. In making the case for federal support, mathematicians had first to overcome the prevailing suspicion not only of the general public but of the scientific community as well. In the early 1960s, C. P. Snow's two cultures became a common a trope in discussion concerning science and national policy. The idea that scientists and humanists were growing apart and found it increasingly difficult to communicate was well acknowledged. Mathematicians' problem was even worse. Not only were they unable to explain their theories to humanists, they also found it difficult to communicate with other scientists. The emphasis on abstraction and generalization that characterized the growth of modern mathematics, so the argument went, made recent mathematical discoveries inaccessible to non-mathematicians.³¹ Mathematical communication

²⁹ John Tukey, "If the Government is to Influence the Distribution of Graduate Students Among Fields, What Means Should It Use?", 5 October 1962, JWTP, Series II. Works By Tukey, Subseries A. Papers, Folder "If the Government is to Influence the Distribution of Graduate Students Among Fields, What Means Should It Use?"

³⁰ Ibid.

³¹ The growing division between mathematics and physics was well recognized by mathematicians, and at least some in the mathematical community tried to amend this situation. In 1959, for example, the Committee on Applied

failed not only between mathematicians and the public, but also between mathematicians and the scientific community.

In his “think piece,” Tukey suggested that “the general feeling of disquiet which a nontrivial number of scientists and engineers have about modern mathematics,” was due to the fact that mathematics did not cultivate any “branch which stands in somewhat the same relation to ‘pure’ or ‘monastic’ mathematics that the experimental branch of a science stands to the corresponding theoretical branch.”³² Not only was modern mathematics characterized by an increasing tendency towards abstractness, but a parallel development in physical mathematics did not exist. Tukey added that differences in opinions existed as to what the nature of such a branch would be, or for that matter “whether one can exist at all.” This was the crux of the matter. The growth of computing, statistics, and operations research had undoubtedly demonstrated the forcefulness of mathematical techniques and methods. Yet the fact that during the preceding decades applied mathematics had not developed into a field with a strong disciplinary identity and an experimental tradition implied that the image of “mathematics” was dominated by theoretical or pure mathematics. It was partly this state of affairs that the shift from mathematics to the mathematical sciences thought to amend. Yet it was not necessarily clear what were the mathematical sciences and what tied them together.

Mathematics at the AMS proposed that the Society support a summer institute on “Modern Physical Theories and Associated Mathematical Developments” that would bring together some leading theoretical physicists and mathematicians to discuss developments in each field. In justifying the topic for the institute, the committee wrote: “over the years there has been an increasing wall separating mathematics from modern physical theories. This wall is regrettable from two points of view. These physical theories have been largely isolated from the newer advances in mathematics, and mathematics itself has lacked contact with one of the most stimulating intellectual developments of our times. The proposed Seminar is an effort to supply this contact.” See: “Proposed 1959 Summer Seminar in Applied Mathematics of the American Mathematical Society,” 1 July 1958, AMSR, Box 44, Folder 106.
³² John Tukey, “If the Government is to Influence the Distribution of Graduate Students Among Fields, What Means Should It Use?”

It took Tukey almost four months to respond to Price's inquiry regarding the proposed public relations office. When he finally did his optimism seemed to have declined. "Frankly," he wrote, "I think it would be necessary for mathematicians, individually and as a body, to become more clear and more explicit about the nature and aims of their own discipline before they could tell even the physicists what it is that they are doing."³³ A few years later, mathematicians attempted to do just that.

How Can Mathematicians Talk Among Themselves?

In February 1963, the National Academy of Science established the standing Committee on Science and Public Policy (COSPUP) as a mediating body between the Academy and Congress. The committee included representatives from all sectional disciplines of the Academy, and was charged with providing information for long-term planning support of science by the federal government. COSPUP published several influential reports during its first few years, the first of which, *The Growth of World Population*, warned against the problem of uncontrolled world population growth. In 1966, Kenneth Kofmehl described the standing committee as "the apex of the Academy's power structure." In an article in the *Journal of Politics*, Kofmehl explained that COSPUP "is rapidly becoming the principal arm of the Academy in dealing with broad policy issues in the science-government area."³⁴ In 1964, COSPUP began publishing a group of studies on individual fields, such as astronomy, physics, and computing that provided the government with specific recommendations and identified promising research directions. Harvey Brooks,

³³ John Tukey to G. Baley Price, 23 June 1961, JWTP, Series I. Correspondence, Box 26, Folder "Price, G. Baley."

³⁴ Kenneth Kofmehl, "COSPUP, Congress, and Scientific Advice," *Journal of Politics* 28, no. 1 (1966), 105.

who served as the second chairman of COSPUP, claimed that “the disciplinary reviews, or planning reports, have constituted the most important single activity of COSPUP.”³⁵

Official preparations for a COSPUP report on the mathematical sciences began in October 8, 1965, when members of the Committee on Support of Research in the Mathematical Sciences (COSRIMS) convened for the first time. Besides Hendrik Bode of Bell Labs and Joachim Weyl, the eleven other members of the committee consisted of academic representatives from various research fields. Tukey (Princeton University) and Theodore Anderson (Columbia University) were the representative statisticians on the committee, Mark Kac (Rockefeller University) was an expert on probability theory, George Forsythe (Stanford University) was a computer scientist, and C. C. Lin (MIT) was an applied mathematician. R. H. Bing (University of Texas, Austin) and Hassler Whitney (IAS) were pure mathematicians, both of whom specialized in topology. Finally, the committee also included the renowned physicist Chen Ning Yang (Stony Brook). The membership of the committee reflected the various disciplines that formed the mathematical sciences. In January 1966, Mark Kac, the chairman of the Division of Mathematics at the NRC, wrote a letter to his predecessor at the job, Adrian Albert. In his letter, Kac explained that the goal of COSRIMS was to produce a statement “intelligible to those responsible for Federal science policy in the Congress and the Executive Branch of the government, of the extent of research and of the supporting strata of education in the mathematical sciences at and above the undergraduate level.”³⁶ COSRIMS was charged with making policy recommendations regarding the mathematical sciences to the government.

³⁵ Quoted in Rexmond C. Cochrane, *The National Academy of Sciences: The First Hundred Years, 1863-1963* (Washington, DC: National Academy of Sciences, 1978), 578.

³⁶ Mark Kac to Adrian Albert, 14 January 1966, AMSR, Box 16, Folder 99.

Therefore, it represented one of the earliest opportunities of the mathematical community to present a coherent image and define the boundaries of the mathematical sciences.³⁷

COSRIMS was a massive undertaking by the mathematical community. In order to base the final recommendations on accurate and current statistics, the committee collaborated with the Conference Board to produce a series of surveys on the state of undergraduate education, graduate education, and employment opportunities. The surveys were quite thorough. They included basic information such as enrollment and graduation numbers as well as detailed analysis of national changes in the mathematical curriculum.³⁸ In addition to these national surveys, which were published independently, the committee established several individual panels to study and produce reports on specific topics. Besides the eleven members of the standing committee, approximately forty-five additional highly respected mathematicians took part in the work of these panels.³⁹ The final report of the committee was published in 1968. It included a list of recommendations pertaining to undergraduate education, graduate training, and research. It also included a lengthy discussion about the nature of the mathematical sciences.⁴⁰

³⁷ In 1957, a decade earlier, a Survey of Research and Training Potential in the Mathematical Sciences was published by the University of Chicago. Like COSRIMS, the Survey provided statistics about the research and graduate training in the field, but it was a much smaller and concentrated undertaking. The work was colloquially referred to as the Albert Survey as it was chaired and organized by Adrian Albert at the University of Chicago. While the title referred to the “mathematical sciences,” there was no real attempt at defining the confines of the field in the survey itself. The analysis provided follows much closer the 1950s division between pure and applied mathematics.

³⁸ The Survey found, for example, that the number of institutions that offered courses in plane geometry for undergraduates decreased by half between 1960-1961 and 1965-1966. “Solid geometry,” the Survey observed, “is practically extinct, the total national enrollment is less than 500... The fine old subject of theoretical mechanics now has a national enrollment of less than 500 as a mathematical science offering.” The main finding that the Survey on undergraduate training discovered and emphasized in the report was the increase enrollment in mathematical courses was felt most strongly in advanced courses in modern mathematics. John Jewett and Clarence B. Lindquist, Conference Board of the Mathematical Sciences: Report of the Survey Committee *Aspects of Undergraduate Training In the Mathematical Science*, 29.

³⁹ Among others, the mathematicians who took part in the work of the Committee included Mina Rees, William Prager, Allen Newell, Harold Grad, Joseph LaSalle, Andrew Gleason and Adrian Albert.

⁴⁰ National Research Council, *The Mathematical Sciences: A Report* (Washington, DC: The National Academies Press, 1968).

From the beginning the work of committee was couched in terms of communication. In his letter to Albert, Kac explicitly stated that the goal of the committee was to increase communication both within the mathematical community and between the community and other scientists. “It is, perhaps, not an exaggeration to say,” he added, “that the leading mathematicians in the United States have been struggling with two distinct but related developments. On the one hand their research has raised pure mathematics in this country to a pre-eminent world position. On the other hand the applications of mathematics have burst the traditional boundaries set by classical physics and have used recent mathematical results in a sophisticated way.”⁴¹ By speaking for the mathematical sciences as a coherent entity, COSRIMS had to come to terms with these two opposing tendencies. The work of the committee can be seen as a careful balancing act between the two. A policy document, the report represented a conscious attempt to portray a coherent image of the field to non-specialists. It sought to emphasize the pervasiveness of mathematics and its significance to national policy. Yet, in many ways, the committee’s work can also be viewed as mathematicians’ effort to answer Tukey’s plea “to become more clear and more explicit about the nature and aims of their own discipline.” It wasn’t just policy makers, but also other mathematicians that the report sought to address.

When he assumed chairmanship of COSRIMS, Lipman Bers had just moved to Columbia University after spending fifteen years as a professor of mathematics at the Courant Institute. Bers began studying mathematics at the University of Riga, where he had also been an outspoken critic of the regime and a political activist.⁴² In 1934, with a warrant issue for his arrest, Bers moved to Prague where he continued his studies and obtained a PhD in mathematics.

⁴¹ Mark Kac to Adrian Albert, 14 January 1966, AMSR, Box 16, Folder 99.

⁴² See: Donald J. Albers and Constance Reid, “An Interview with Lipman Bers,” *The College Mathematics Journal* 18, no. 4 (1987): 266–290; Bill Abikoff et al., “Remembering Lipman Bers,” *Notices of the American Mathematical Society* 42, no. 1 (1995): 8–25.

In 1940, after spending two years in France, Bers obtained a visa to the United States as a political refugee. He settled in New York with his wife, where they lived among other unemployed refugees. Bers got his first break in 1942, when he was invited by Richardson to serve as an instructor at the Brown summer school in applied mechanics. During his time at Brown, Bers began research on fluid flows which was later applicable to aircraft wings' design. At NYU Bers served as the chair of the graduate program and supervised the work of twenty-two students. His research over the years spanned both pure and applied mathematics, though in an interview later in life Bers noted that he always felt "emotional attraction to applied mathematics." Throughout his life Bers was an avid human rights activist.

Mathematics, the report begins, had long played a role in scientific and technological developments. Yet such an assertion "hardly begins to convey or account for the current explosive penetration of mathematical methods into other disciplines, amounting to a virtual 'mathematization of culture.'"⁴³ Mathematization, or the penetration of mathematical techniques to a growing sphere of knowledge, became the rallying cry for the mathematical community.⁴⁴ The mathematical sciences were all-encompassing. The term, that is, was premised on the assertion that there was both a quantitative and a qualitative difference in the appropriation of mathematical techniques and methods in the aftermath of World War II. The mathematical sciences, the report asserts were not only fundamental to the physical sciences and engineering sciences, as they have been for years, but also to the biological, social and behavioral sciences. Anthropology, sociology, political science, and psychology were all singled out as fields in

⁴³ National Research Council, *The Mathematical Sciences*, 3.

⁴⁴ I follow the report's analysis of this process of "mathematization," since it was written in collaboration with many leaders in the mathematical community and do not necessarily represent the views of a single mathematician. Yet this sentiment is expressed by many writings from the period. Stone, for example, wrote that "it is becoming clearer and clearer every day that mathematics has to be regarded as the corner-stone of all scientific thinking and hence of the intricately articulated technological society we are busily engaged in building." Stone, "The Revolution In Mathematics," 716.

which statistical techniques were becoming increasingly important. Moreover, the use of mathematical techniques was not limited to academic domains. Government, business, and industry, the report continues, progressively depend on mathematics and computers to solve problems of resource and time allocation. The message was clear: mathematics was everywhere.

This mathematization of culture the report sought to establish was manifestly not militarized. The authors proclaim, “it is no exaggeration to say, therefore, that the fundamental problems of national life depend now, more than ever before, upon the existence and the further growth of the mathematical sciences and upon the continuing activated of able people skilled in their use.”⁴⁵ Almost every aspect of daily life was affected by mathematics. As examples, the authors note that mathematics was an “absolute necessary condition” for developments in electronics, and that the growth of information theory, network synthesis and feedback theory was “unthinkable” without mathematics.⁴⁶ They add that telephone and radio communication also depend on mathematics, as well as the transmission of pictures, and the “collection, classification, and transmission of data in general” requires mathematics. If that was not enough, transportation both on the ground and in the air is demonstrated to require mathematics in consideration of traffic control problems. Mathematics, the authors prove, was implicated in the life of the nation.⁴⁷

This ubiquitous image of mathematics, needless to say, stood in direct opposition to *The New Yorker*’s portrayal of mathematics and mathematicians that began the chapter. The report,

⁴⁵ National Research Council, *The Mathematical Sciences*, 47.

⁴⁶ *Ibid*, 46.

⁴⁷ The most pressing message the report aims to convey is the need to strengthen the mathematical literacy of the nation. The demand, according to the authors of the report, is both for professional mathematicians, more mathematically trained scientists, and a mathematically informed public. This of course was the era of the “New Math” movement that saw for the first time the involvement of research mathematicians in elementary and high-school mathematical education. Chris Phillips has demonstrated how debates about the New Math curriculum were debates about the “necessary” intellectual discipline of American citizenship. Christopher Phillips, “The American Subject: The New Math and the Making of a Citizen” (Dissertation, Harvard University, 2011).

that is, represents a conscious effort to counter the image of mathematics as abstract and disconnected from the world. Nothing could be further from the truth. Not only were the mathematical sciences crucial to the development of science and technology, they were also fundamental to the fabric of everyday life. No other science could make a similar claim. A period of abundance, the 1960s also saw the growing concern about the promises and perils of basic research. Books such as Daniel Greenberg's *The Politics of Pure Science* sought to draw attention to the political underpinning of the scientific enterprise. Mathematics was not always mentioned in these discussions, but when it did it was often considered as the furthest extreme, the purest of pure science.⁴⁸ Thus, in making their case for funding and their rightful place among the sciences, what mathematicians did was to completely invert this view.⁴⁹ Instead of placing mathematics in the extreme, they placed it in the center. Mathematics was not only at the heart of the scientific project, but also central to the daily life of the nation. The “mathematical sciences” became part of mathematicians’ public campaign to control the image of mathematics and mathematicians. It was a way of reaffirming the authority and dominance of the field.

A chapter entitled, “examples of mathematics in use,” further illustrates this point. The chapter begins with a discussion of the crucial relation between mathematics and physics, stressing the reliance of physical breakthroughs and insights on mathematical theories. However, this is not the emphasis of the chapter. The general reader might have had a more positive view of theoretical physics than of mathematics, but it was still too far removed from one’s daily life. The next section instead moves to describe the uses of the mathematical sciences in engineering.

⁴⁸ In making a distinction between Big Science and Little Science, Greenberg, for example writes that “Little science, on the other hand, involves fewer people per project and far less costly equipment, the extreme in this category being the lone theoretical mathematicians whose tools are papers and pencil.” Daniel S. Greenberg, *The Politics of Pure Science* (University of Chicago Press, 1999), 11.

⁴⁹ Mathematics did not depend on expensive equipment and hence was less open to direct criticism, but the field did lay claim to the scientific budget of the nation.

From civil and mechanical engineering to nuclear technology and electronics, increasingly sophisticated mathematics was being used. “It is found, as expected, that the more advanced the technology, the more sophisticated are the basic concepts involved, and the more they depend on mathematics.”⁵⁰ Faster airplanes, longer-ranging rockets, better designed gas turbines and radio antennas, and more accurate prediction of satellite orbits are all celebrated as examples of mathematically based technologies. Developing new mathematical techniques and tools was crucial, the report made clear, for maintaining technological progress. From technological advancements, the chapter moves to the environmental sciences and to weather prediction. The successful operation of the National Meteorological Center, the report asserts, depended on increasing computational power of digital computers and improvements in numerical weather modeling: two activities that were fundamentally mathematical.

Finally, the chapter provides additional examples from the use of the mathematics in economics, finance, insurance, and management and operations. “Key economists,” the report proclaims, “know as much of the details of modern control theory and what is known about the stability of nonlinear systems...as do all but the most specialized mathematicians.”⁵¹ The actuarial profession is asserted to be “a thoroughly mathematical technology,” and the “mathematical approach” is claimed to be “steadily penetrating the practice of management and operation.” In many of the examples provided in the report, the emphasis was on the use of statistical methods and computing. This was part of the ingenuity behind the use of the term the mathematical sciences. Combining the various mathematical fields together and emphasizing their interdependence enabled the authors to present an all-pervasive image of mathematics. The

⁵⁰ National Research Council, *The Mathematical Sciences*, 105.

⁵¹ If that was not enough, the report adds, “we recall also that J. M. Keynes, the father of modern economics, had been trained as a mathematician.” *Ibid.*, 111.

mathematical sciences did not cohere around a subject matter, but around a methodology. To say that a discipline or a technology was *mathematical* was to claim that it could be analyzed using mathematical reasoning and techniques.⁵²

Paul Erickson, Judy Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm and Michael Gordin have recently argued that, within the political and cultural climate of the Cold War, a new conception of rationality came into existence.⁵³ This “Cold War rationality,” they argue, was a characteristic feature of human sciences and was implicated in everything from the scientific method to the highest political decision-making during the period.⁵⁴ As an ideal type, they write, Cold War rationality was to be “formal, and therefore largely independent of personality and context, and frequently took the forms of algorithms.”⁵⁵ It sought to break complex situations into simple step-wise tasks, and elevated “across-the-board generalities” over specific historical or cultural context. Finally, it promoted mechanized reasoning. To a certain degree these features of Cold War rationality to which the authors draw attention can be described as three of the main characteristics of modern mathematics: abstraction, generalization, and sequential (and symbolic) reasoning. It was not so much mathematics as mathematical thinking that grew in prominence during the period. Cold War rationality, it can be argued, was predicated on the mathematization of culture described in the report.

This new articulation of the field and its components was not merely a reaction to national demands. It also represented a conscious effort by mathematicians to come to terms with the tremendous transformations that the field had undergone in the preceding two decades. Early

⁵² The authors of the report note, “mathematical thinking penetrated the sciences and the development and organization of technology at an ever-accelerating rate.” *Ibid.*, 46

⁵³ Paul Erickson et al., *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality* (University Of Chicago Press, forthcoming).

⁵⁴ Under the human sciences, the authors broadly include political science, economics, sociology, psychology, and anthropology.

⁵⁵ *Ibid.*

on in the committee's work, it had been decided that in addition to the formal report, the committee would also publish an edited volume consisting of individual essays on aspects of the mathematical sciences.

The Mathematical Sciences: A Collection of Essays came out in 1969, a year after the official report was published.⁵⁶ In the forward to the collection of essays, Bers notes that from the beginning it was clear to the members that the report would be somewhat different than the ones produced for other fields. Members of the Committee, he explained, felt that additional background information about the mathematical sciences was necessary. Even though mathematics "provides the common language for all sciences," Bers wrote, scientists (as well as non-scientists) might feel that they are not informed about modern mathematical research. "Even professional mathematicians, or scientists who customarily use mathematics in their work," Bers then added, "may be unaware of the manifold applications of mathematics in various sciences and technologies, especially the new applications influenced by the computer revolution."⁵⁷ The goal of the edited volume was to provide a glimpse into both well-established areas of research and developments at the forefront of the mathematical sciences.

The twenty-two essays in the collection were commissioned from experts in various mathematical fields specifically for the publication.⁵⁸ Stanislaw Ulam opened the collection with a general discussion about "The Applicability of Mathematics," while Jack Schwartz, who founded the Department of Computer Science at New York University in 1964, closed it with an examination of the "Prospect of Computer Science." The collection also included essays on point-set topology by R. H. Bing, non-Euclidean geometry by H. S. M. Coxeter, vector spaces by

⁵⁶ National Research Council, Committee on Support of Research in the Mathematical, *The Mathematical Sciences: A Collection of Essays* (Cambridge, MA: M.I.T. Press, 1969).

⁵⁷ *Ibid.*, vi.

⁵⁸ Three of the essays were reprints, though only Freeman Dyson's essays "Mathematics in the Physical Sciences" was not edited by its author to fit the publication in the collection of essays.

Edward McShane, and the algebraization of mathematics by Samuel Eilenberg. In addition, the collection included essays about mathematics and the social sciences, mathematics and the physical sciences, the role of mathematics in economics, mathematical linguistics, and the biomedical sciences and mathematics. It is hard to characterize the essays in the collection. Bers writes in the forward that the essays are intended for the non-mathematical scientists as well as the “scientifically oriented layman.” Yet each author seems to assume a different background from his reader. Differential equations, for example, are taken as given in one essay while being carefully defined in another. Probably the main message the *Collection of Essays* advances is the multifaceted nature of the mathematical sciences.

Almost a third of the entire report (which was approximately 250 pages) was devoted to an overview of the mathematical sciences. The decision to publish an additional collection of essays, many of which would not necessarily be comprehensible to the uninitiated, indicates that members of the committee and the mathematical community more broadly were eager to present a comprehensive image of the field. Reading the report as just a cynical presentation of the field, which seeks to optimize support, would be to miss all the hard work that went into assembling and presenting a unified theory of mathematics. The membership of the committee reflected the changing terrain of the mathematical community itself. This was not a committee of the AMS or one that was dominated by its members.⁵⁹ The various mathematicians who took part in the numerous panels of COSRIMS came from a wide area of specialization. A consensus document by definition, the report can be read as a real attempt by the mathematical community to come to terms with the many changes the field had undergone in the aftermath of World War II.

⁵⁹ Here again a comparison with the 1957 national Survey is revealing since, unlike COSRIMS, it was initiated and conducted through the AMS and its leadership.

Shiing-Shen Chern, David Blackwell, and George Forsythe were the three members of COSRIMS' subpanel on "Support of Various Areas of the Mathematical Sciences." The three were in charge of making recommendations as to how to disperse support across the mathematical sciences. Yet the draft of their report did not start with a list of policy recommendations, but rather with a series of "observations." "The underlying unity of the mathematical sciences," the first read, "seems to involve the use of long chains of formal manipulation of abstract symbols often called 'symbolic reasoning'. Such chains may occur in the mind, on paper, or inside a digital computer."⁶⁰ The second observation asserted that in pure mathematics the chain of symbolic reasoning is "judged to be successful mainly according to aesthetic and intellectual standards of beauty, universality, economy, etc." In applied mathematics the same criteria hold, the third observation noted, but "another criterion is often important and sometimes decisive: the useful prediction or control of events in the real world of objects and men."⁶¹ Only after providing the following analysis did the three men state their funding recommendations.⁶² Folded into the definition of the mathematical sciences was a theory

⁶⁰ "Report to Graduate Panel of COSRIMS by Subpanel Concerned with Support of Various Areas of the Mathematical Sciences," JWTP, Series I, Box 23, Folder "National Research Council (U. S.) - Committee on Support of Research in the Mathematical Sciences."

⁶¹ Ibid.

⁶² After he read a draft of their report, Tukey wrote to Bers commenting that the panel's ideas had made him consider once again where he stood on the question of unity. Noting that his own answer was influenced by the report, Tukey wrote:

the underlying unity of the mathematical sciences lies in the assembly and study of chains of steps, sometimes short but often long. In pure mathematics, these steps typically involve formal manipulation of abstract symbol according to agreed-upon logically based rules, a process of called "symbolic reasoning." At the outer fringes, by contrast, these steps often involve symbolically describable processes, whose properties may not be well understood, and the adequacy of whose interconnection may have to be tested by trial in some real situation. In the inner citadel, both chains of symbolic reasoning and the results reached by these chains are mainly judged by aesthetic and intellectual standards of beauty, universality, economy, etc. (though, as the rise and fall of projective differential geometry shows, hopes of eventual application do exert their influence.) At the outer fringes, although beauty, universality and economy are still valued, progress toward the empirically verifiable, especially toward the prediction or control of events in the real world of objects and men, becomes the prime criterion. Of course, all transitional stages can be found." John Tukey to Lipman Bers, 23 March 1964, JWTP, Series I. Correspondence, Box 23, Folder "National Research Council (U.S.) – Committee on Support of Research in the Mathematical Sciences."

of knowledge, one that truly sought to place the transformations in mathematics into a coherent framework. These discussions presented a meta-theory about the nature of mathematics and its increasing source of utility.

Chern was a celebrated differential geometer. In his history of the “Geometry Renaissance” in America, Osserman writes that Chern’s move to the US from China in the 1940s was the “single most decisive factor contributing to the rebirth of geometry in America.”⁶³ Forsythe was central in establishing the Department of Computer Science at Stanford. Donald Knuth has written that “one might almost regard him [Forsythe] as the Martin Luther of the computer reformation!”⁶⁴ Finally, Blackwell, a statistician, was the first African American to be elected to the National Academy of Science as well as the first to obtain tenure at the University of California at Berkeley.⁶⁵ Seeking the source of unity of the field, the three men settled on the act of symbolic reasoning. Their assertion that manipulation can take place on paper or inside a digital computer already begins to point to the re-conceptualization of the field. Their joint work was indicative of the new types of alliances that were being formed across the so-called mathematical sciences.⁶⁶ By the end of the decade the various constitutive fields only grew

⁶³ Robert Osserman, “The Geometry Renaissance in America: 1938-1988,” in *A Century of Mathematics in America. Part II.*, ed. Peter L. Duren, Richard Askey, and Uta C. Merzbach (Providence, RI: American Mathematical Society, 1989): 513–526.

⁶⁴ Donald E. Knuth, “George Forsythe and the Development of Computer Science,” *Communications of the ACM* 15, no. 8 (1972): 721–726.

⁶⁵ Donald J. Albers, “David Blackwell,” in *Mathematical People: Profiles and Interviews*, ed. Gerald L. Alexanderson and Donald J. Albers (Boston: Birkhäuser, 2008): 18–32; George G. Roussas, “A Tribute to David Blackwell,” *Notices of the American Mathematical Society* 58, no. 7 (August 2011): 912–928.

⁶⁶ In December 1966, Anthony Oettinger, who was at the time the President of the Association for Computing Machinery (ACM), published a letter to the membership of the organization in the *Communication of the ACM* informing them of the COSRIMS study. Many members, Oettinger suggested, would likely believe that “computer science and engineering will necessarily be inadequately represented in a body dominated by mathematicians, pure and applied, whose outlook on computing, if at all understanding, is from the point of view of the computer user.” Despite this warning, Oettinger added, “I might emphasize that, at this time, COSRIMS is our only major official point of contact with the powerful policy-recommending machinery.” At the time, computer science was still in its infancy and was only starting to break its ties from mathematics. However, Oettinger wanted to reassure the ACM membership that the COSRIMS effort was not going to disregard the needs of computer scientists. When the committee was established, he added, it was agreed that it would represent the common opinions of the whole

further apart, though in the mid-1960s the contours of the mathematical sciences were in constant flux. In describing the mathematical sciences, the report sought to establish both the commonalities and the differences between the various fields that were subsumed under it.

To accomplish this goal, the first thing the report did was to dismiss altogether the division between pure and applied mathematics or, more accurately, the notion of pure mathematics. Instead the authors suggest the use of the term “core mathematics” to refer to traditional disciplines such as logic, number theory, algebra, geometry and analysis.⁶⁷ This seemingly simple shift in terminology served several goals at once. First, it challenged the image of the detached mathematician. If “pure mathematics” did not exist, then neither did “pure mathematicians.” This was explicitly stated in the report. “The name ‘pure mathematics’ is unfortunate since it implies a monastic aloofness from the world at large and an isolation from its scientific, technological, and social concerns. Such aloofness may be characteristic of some mathematicians. It is certainly not characteristic of mathematics as a collective intellectual endeavor.”⁶⁸ To be sure some mathematicians could be characterized as aloof, but this was in no way descriptive of the mathematical community. The mathematization of culture required mathematicians who were deeply enmeshed in the world around them.

Second, the use of the term “core” served a descriptive purpose. As the report makes clear, those disciplines characterized as belonging to the “core” of mathematics were

mathematical community. To that end, the committee began soliciting ideas and suggestions from the broader community. “I urge members of the ACM concerned with the future of computer science,” Oettinger concluded, “to avail themselves of this channel of communication.” Indeed, when the final report was published it stressed the need for support in computer science, claiming it was a matter of real urgency. Anthony G. Oettinger, “President’s Letter to the ACM Membership,” *Communication of the ACM* 9, no. 12 (December 1966): 838–839.

⁶⁷ In his brief history of American mathematics from 1888 – 1988, William Duren noted that given the inaccurate prediction of COSRIMS, the report’s “small, but lasting, contribution was the phrase, ‘core mathematics.’” William L. Duren, “Mathematics in American Society, 1888-1988: An Historical Commentary,” in *A Century of Mathematics in America*, ed. Peter L. Duren, Richard Askey, and Uta C. Merzbach, vol. 2 (Providence, RI: American Mathematical Society, 1989), 455.

⁶⁸ National Research Council, *The Mathematical Sciences*, 49.

fundamental to past and future theoretical developments in the field. Instead of an antagonistic image of the field in which pure mathematicians pulled away from the world while applied mathematicians fight to ground them, the “mathematical sciences” presented a much more harmonious view of the field with “core mathematics” serving as a nucleus for further developments. The term, as the report succinctly puts it, “reminds us of the central position of so-called ‘pure’ mathematics with respect to all mathematical sciences.”⁶⁹ In many ways, the language of the report can be read as mathematicians’ conscious resolution not to repeat the debates of the 1950s. The increase in the funding afforded to mathematics post-Sputnik, especially through the NSF, implied that pure mathematicians no longer worried, as they had in the previous decade, that they would be left out of the federal funding machine. Also, while applied mathematics did not grow as a coherent field of study, areas of mathematical applications did continue to develop and gain independence over the decade.

In addition to “core mathematics” the mathematical sciences were composed of the “applied mathematical sciences,” which comprised of four major areas of research: computer science, operation research, statistics, and physical mathematics (classical applied mathematics). This four-part division of the applied mathematical sciences further points to the reconfiguration of the field. If in the 1950s, applied mathematics was used as an umbrella category to encompass all the various field of mathematical applications, by the mid-1960s many of these new areas of research were already beginning to be established as independent fields. Therefore, applied mathematics could no longer account for the various developments in the field. The report clearly indicates that the use of the term “applied mathematics” is “accurate” only if it is understood to include both the “traditional” areas of research and the newer ones. In fact the

⁶⁹ Ibid.

report makes a further division between applied mathematical sciences, and “partly mathematical sciences.” Whereas computer science and statistics belonged to the latter, physical mathematics belonged to the former. The partly mathematical sciences were deemed as such because they had “dual sources of identity and intellectual force, only one of which is mathematical.” Mathematics was an integral component of computer science, but the hardware required electrical engineering and other domains of expertise outside of mathematics.

The “mathematization of culture” was contingent to a certain degree on this rearticulating of what constituted applied mathematics. After all, it was not mathematical physics that was deemed to be fundamental to the everyday life of the nation. In defining the applied mathematical sciences the report places different areas of research in a spectrum ranging from fields centered around a specific subject matter to ones characterized by their breadth of application. Thus, on the one side were computer science and statistics, which the report noted, “already apply to almost as wide a variety of activities as does mathematics itself.” On the other “extreme” were fields such as mathematical economics, mathematical psychology, and mathematical linguistics, which dealt “with mathematical aspects of rather specific areas.”⁷⁰ Physical mathematics, or classical applied mathematics, occupied an “intermediate level” between the two extremes. These fields were derived from a specific area of application but had “developed far enough to have a mathematical character of their own.” Prior to World War II, applied mathematics research was almost completely characterized by this middle ground. Mathematical theories, such as fluid dynamics and elasticity, arising either from engineering considerations or from physical experimentations, *were* applied mathematics. It is the

⁷⁰ National Research Council, *The Mathematical Sciences*, 46.

development of fields on the two extremes that characterized the growth of the field in the postwar period, and which necessitated the shift from mathematics to the mathematical sciences.

The final report was widely publicized in the mathematical community. The National Research Council set up two committees to study how the various recommendations made in the report could be implemented, and articles about the committee's work appeared both in the *Notices* and the *Mathematical Monthly*.⁷¹ The community for the most part received the report positively. However, probably the most lasting impact of the report was due to the fact that it was published at exactly the time when the incredible growth that underlined its inception came to a halt. Thus, the report, as will be discussed in greater detail in the next chapter, became a sort of a benchmark point in discussions during the early 1970s regarding the transformation the mathematical community was undergoing. When discussing either the overproduction of PhD mathematicians or the changes in the federal government support structure to the sciences, mathematicians repeatedly refer to the report as singling a turning point. At a time when the emphasis on applied research with clear societal benefits was on the rise, the presentation of mathematics in the report was celebrated as opportune.

How Can Mathematicians and Non-Mathematicians Talk to Each Other?

In 1970 Robert Hermann published an article in the *Monthly* summarizing his reaction to the report. The report, Hermann began, “displays much more wisdom and sensitivity” than expected and its authors deserved the appreciation of the entire mathematical community. Hermann's only

⁷¹ R. P. Boas, “The Cosrims Reports,” *The American Mathematical Monthly* 77, no. 6 (June 1, 1970): 623–625; Preston C. Hammer, “The Mathematical Sciences: A Collection of Essays (COSRIMS),” *SIAM Review* 12, no. 3 (July 1970): 474–476; Peter Hilton, “The COSRIMS Reports,” *The American Mathematical Monthly* 77, no. 5 (1970): 515–517; Arnold Ross, “COSRIMS Reports--Retrospect and Prospect,” *The American Mathematical Monthly* 77, no. 5 (May 1, 1970): 514–515.

objection was the report's treatment of the "the old bugaboo of a problem – 'applied mathematics.'" Hermann acknowledged that for the most part the report gave a pretty good portrayal of the field, but he insisted that the majority of mathematicians still have a "distorted picture" of the field. He was specifically disappointed with the treatment of mathematical physics, which, he noted, had languished in the previous decades. MIT applied mathematicians C. C. Lin, who was a member of COSRIMS and was active in building a program in applied mathematics at MIT, replied to Hermann's article. In his letter, Lin argued that the only way to ensure the growth of applied mathematics was to establish it more firmly within higher education. "The support of universities, giving *tenure* appointment to scientists of *high caliber*, is the only stable way to support any academic subject."⁷² What Lin pointed out was that the new fragmented vision of the applied mathematical sciences articulated in the report called into question the development of applied mathematics as an academic pursuit.

Indeed, the number of applied mathematicians in academic position was still low in comparison with other mathematics specialization. For example, a survey in 1961 found that out of 95 institutions that offered graduate training in mathematics, only 49 offered a specialization in applied mathematics.⁷³ In comparison, 86 institutions offered a specialization in analysis, 77 in algebra, and 67 in topology. Four years later, in 1965 there were only four departments across the country dedicated specifically to training in applied mathematics. At the same time, as will be detailed in the next section, independent departments in statistics, computing, and operation or management research were growing in prominence. In order to survey the contemporary state of training in the field the Society of Industrial and Applied Mathematics organized a conference

⁷² C. C. Lin to Robert Hermann, 18 March 1969, AMSR, Box 16, Folder 150.

⁷³ A department offered a specialization in a given field when its faculty included a professor who was able to direct a dissertation in the specific area of research as well as offer courses.

on education in applied mathematics in May 1966. Twelve years had passed since the last national conference in training in the field, and while applied mathematics continued to grow and develop it still represented a relatively small percentage of training in mathematics. Similar to the conference that preceded it twelve years earlier at Columbia University, the conference brought together educators from around the country to consider how to improve training in applied mathematics.⁷⁴ The conference was held at the Aspen Institute of Humanistic Studies in Colorado over four days and included sessions about research programs in applied mathematics, the curriculum in mathematics and the post-doctoral experience in the field.

Chia-Chiao Lin gave the opening speech at the conference on the objectives of applied mathematics education. Lin, who had received his PhD under the supervision of von Kármán at Caltech, was troubled by the fact that there did not exist any program that provided a comprehensive training in applied mathematics, as a coherent entity and not as a collection of sub-specialties. At MIT, Lin together with several other mathematicians tried to implement such training. Lin used his opening remarks to lay down some of his philosophical conviction. He began:

I shall present the subject of applied mathematics as a science (not technology), the applied mathematician as a scholar (not a technical expert), and an education in applied mathematics as a liberal education (not a technological education) – which has, as its *primary goal*, the knowledge and the proper understanding of the *total* picture of the

⁷⁴ Several members of COSRIMS were invited to the conference and used the discussions to inform their presentation of applied mathematics in the final report.

interaction between pure mathematics and the sciences, and the development of an ability to do creative work to further this partnership.⁷⁵

Lin believed that applied mathematics needed to be taught from a comprehensive perspective, which would not be centered around specific applications, but rather seek to establish a middle ground between pure mathematics and theoretical sciences.

The Comprehensive Applied Mathematician, Lin promoted, was to “have a knowledge of *all* the (dramatically) successful interactions of mathematics with the basic aspects of the sciences.”⁷⁶ Lin maintained that there was a way to teach applied mathematics from a comprehensive perspective that was not defined by specific theories but rather by an attitude. It was the “desire and ability to cut across traditional scientific disciplines, through the medium of mathematics” that, according to Lin, should be the main characteristic of this new applied mathematician.⁷⁷ This new brand of mathematician would develop mathematics that was directly motivated by scientific problems, but would be distinguished from the theoretical scientist. Lin believed that applied mathematics should denote the applicability of mathematical techniques and methods to physical phenomena regardless of the specific subject matter.⁷⁸

Lin’s description of training in applied mathematics is reminiscent of the 1950s discussions described in the previous chapter. Applied mathematics was still being defined as a matter of an attitude rather than a specific body of techniques or theories. However, whereas a

⁷⁵ H. J. Greenberg, ed., “Proceedings of a Conference on Education in Applied Mathematics,” *SIAM Review* 9, no. 2 (April 1967), 293.

⁷⁶ *Ibid.*, 294.

⁷⁷ *Ibid.*, 298.

⁷⁸ In response to a letter from Lin, in which he protested the portrayal of applied mathematics in the first part of the report, Bers explained, “The issues discussed in part I are primary and must be understood before one comes to such subtler questions as the distinction between the applied mathematical sciences as a collection of disciplines and a comprehensive applied mathematics.” Lin felt that in trying to come up with a consensus as to the nature of the mathematical sciences, the Committee neglected academic applied mathematics. Lipman Bers to C. C. Lin, September 28 1967, JWTP, Series I, Box 23, Folder “National Research Council (U. S.) - Committee on Support of Research in the Mathematical Sciences.”

decade earlier much of the discussion concerned the construction of the applied mathematician as a new professional identity in light of the growing demand for industrial workers, this was not the case in the 1960s. Lin made it clear in his opening remark when he emphasized that he was interested in the training of the “applied mathematician as a scholar.” It was not the industrial worker, or the consultant that conference attendees discussed, but the research scholar. In part this was due exactly to the sort of differentiation of the applied mathematical sciences described in the report. As computer science and statistics began to separate from mathematics, there was once again room to question the place of physical applied mathematics as an academic pursuit. Subsequently, the discussions during the conference did not center on the personality characteristics of the applied mathematicians, but rather on the applicability of abstract mathematical theories in the physical world. In turn mathematicians shifted from talking about psychology to talking about philosophy and history.

Commenting on Lin’s presentation, George F. Carrier conceded that “it is a very nontrivial question to ask whether applied mathematics is a sufficiently deep and coherent and broad activity that it stands in its own right as an independent discipline, or whether applied mathematicians are a rather disjoint collection of people who are nonconformist enough not to fit naturally into any other category and are grouped under this kind of a term without any great coherence.”⁷⁹ Having raised the question Carrier resolved that the answer was indeed yes. However, echoing the 1950s discussions, Carrier defined the field through its practitioners, who tended to deal with problems that arose outside of mathematics proper.⁸⁰ Tukey announced that he accepted Lin’s “view that there can be an independent discipline in the area of applied

⁷⁹ Greenberg, “Education in Applied Mathematics,” 305.

⁸⁰ Carrier stated: “I would say that applied mathematics was that discipline whose devotees, by and large, direct their attention to attempts to answer questions that arise largely outside of mathematics using mathematics as a unifying element and a principal source of tools.” Ibid.

mathematics.” However, he remained somewhat doubtful that there did exist, as Lin suggested, but one applied mathematics. More than relabeling “pure” mathematics as “core” mathematics, COSRIMS points to the changing conceptual understanding of applied mathematics in the 1960s. Confronted with what appeared to be the appropriation of mathematics into an ever-increasing number of distinct domains of knowledge, mathematicians were left asking what the source of this prosperity. Were there essential unified principles that accounted for the applicability of mathematics and hence could be identified and transmitted to students? Or should each field of application be studied, and taught, separately?

On the third day of the conference during a discussion following a session on the “science curriculum,” R. J. Duffin of Carnegie Institute of Technology interjected, “most of the discussion at this meeting has concerned the philosophy of applied mathematics.”⁸¹ Duffin insisted that a more focused discussion of concrete educational plans was in order. By the fourth day, the tendency to default into philosophical discussions was so overwhelming that when George Pólya took to the stage to open the session on “training in applied mathematics research,” he announced that the group of presenters had met the previous night for a preliminary meeting. The group had discussed philosophy and reached an agreement, Pólya reported, as to the definition of a philosopher. “A philosopher is a man who knows everything, but nothing else.”⁸² A slight variation of this definition, according to Pólya, was that “a philosopher is a man who specializes in generalities.” Pólya retorted that he had nothing against philosophy. Having already published *How To Solve It*, he could not be accused of not being concerned about the role of heuristics in mathematics. Rather, he wanted to draw attention to the group’s decision to “get down to brass tacks” and talk about “concrete things.” Avoiding philosophical discussions,

⁸¹ Ibid., 335.

⁸² Ibid., 347.

however, was not an easy task. Even the most concrete proposals as to how applied mathematicians should be trained gave rise to theoretical deliberations regarding the relation between mathematics and science and the source of mathematical applicability.

The discussion during the second day of the conference was dedicated to the topic of the mathematics curriculum. Attendees were asked to consider what sort of mathematical training an applied mathematician should receive. After a brief set of introductory comments, Bers, who was one of the official presenters during the session, declared, “the problem is, how do we prepare the future card-carrying applied mathematicians, the future mathematicians who will apply mathematics, the future mathematical technologist?” More specifically, he asked, “what mathematical equipment do we give them before sending them out to do their job?”⁸³ Yet, stating the problem so clearly, Bers did not proceed by providing concrete examples of courses, techniques, or theories applied mathematicians should be taught. He did not try to specify the “mathematical equipment” students would require. Instead, Bers made a complete reversal. “I would like to make a plea,” he told audience members:

Let us stimulate research in the history of modern mathematics...let us try to find “what did really happen.” I think if we do we will find that the traditional picture of problems coming from the outside into mathematics, being solved there and then going back, is exceedingly oversimplified. I think, too, that the future applied mathematician should be told about things which were invented by the purest of pure mathematicians, out of pure curiosity, and which later turned out to be the bread and butter of applied mathematics.⁸⁴

How did Bers move from talking about the tools necessary for applied mathematicians to making a plea for an investigation of the history of mathematics? How could episodes from the history of

⁸³ Ibid., 315.

⁸⁴ Ibid.

mathematics instruct mathematicians as to how to best train the applied mathematicians of the future?

What accounted for the applicability of mathematics in the natural world was a burning question during the 1960s. World War II marked a watershed in the development of mathematics both quantitatively and qualitatively. That the great expansion in mathematics came at a time when “core mathematics” was turning towards increased abstractions only served to heighten the mystery. In calling for a history of mathematics that would uncover “what did really happen,” Bers was merely voicing what was a contemporary preoccupation with the development of mathematical ideas.⁸⁵ Understanding how mathematical theories that emerged out of one context transformed into another, so the argument went, would illuminate how to best train applied mathematicians for the future. Yet, in the absence of a definitive historical assessment about the relation between mathematics and science, the reigning philosophy, of which Bers himself was an avid supporter, held that the source of mathematical applicability is mysterious and cannot be predicted. This view animated COSRIMS’s final report.

Articulating the division between core mathematics and the applied mathematical sciences was only the first step. In presenting a unified image of the mathematical sciences, the

⁸⁵ It is almost impossible to pick up an expository article from the period about some aspect of mathematics which does not include some episodes from this history of mathematics. A prime example of this phenomenon is a special issue on “Mathematics in the Modern World,” which appeared in *Scientific American* in September. The issue included articles by leading mathematicians such as Richard Courant, Phillip Davis, Stanislaw Ulam, and Mark Kac, on topics such as “number,” “probability,” “geometry,” and “computers.” Many articles included lessons in the history of mathematics from Greek geometry and Babylonian algebra to Newton’s calculus and Hilbert’s millennium problems. Moreover, despite the title “mathematics in the *modern* world” among the images included in the issue were images of an ancient Egyptian papyrus about the measurement of land and a stone tablet portraying a dot-and-bar notation recording dating from the Olmec in Mexico. The emphasis on the history of mathematics was not just an artifact of the popularization of mathematics. The period was also marked by the publication of several histories/philosophies of mathematics written by mathematicians. Salomon Bochner published *Why Mathematics Grows* in 1965 and *The Role of Mathematics in the Rise of Science* in 1966. Alfre Renyi published *Dialogue on Mathematics* in 1967, and Durik Struik and Morris Kline each published several historical surveys of mathematics. This occupation with the history of mathematics fully manifested itself in 1970 in a workshop on the evolution of modern mathematics in the 19th and 20th century organized by leading mathematicians in collaboration with several historians of mathematics.

report also had to account for the way the two interact. The answer, at least as far as the authors of the report were concerned, was simply serendipity. The report is furnished with examples of mathematical theories, which were developed independently of physical reality but turned out to be applicable in varying circumstances. The emphasis throughout, however, is on the fact that there is no way to foresee which mathematical theories will have practical uses and which will not. “Remarkably enough,” the report proclaims, “it is impossible to predict which parts of mathematics will turn out to be important in other fields.”⁸⁶ A page later the authors add, “we stress once more the totally unpredictable nature of such applications.”⁸⁷ These two assertions, that seemingly abstract developments in core mathematics turn out to have diverse applications and that there is no way to predict this process, were central to the unified image of the mathematical sciences the report portrayed. Providing an overview of current research in core mathematics, the report somewhat remarkably managed to demonstrate the applicability of almost every mathematical concept it introduced.

For example, a short introduction to mathematical logic covers everything from Whitehead and Russell’s *Principia* to Georg Cantor’s set theory to Kurt Gödel’s incompleteness theorem. The main idea of each logician is quickly presented and contemporary developments are briefly mentioned. Yet, before concluding, the discussion the report proclaims:

What has been said up to now may convey the impression that mathematical logic is a highly abstract subject, related to philosophy and much too esoteric for the taste of most mathematicians. This is indeed the case. Yet this most austere of all mathematical disciplines had made a contribution to America’s fastest-growing industry.⁸⁸

⁸⁶ National Research Council, *The Mathematical Sciences*, 8.

⁸⁷ *Ibid.*, 9.

⁸⁸ *Ibid.*, 59,

That mathematical logic was fundamental to the development of computing is taken as the ultimate proof of the relations between abstract mathematics and real world uses. Combinatorial analysis, group theory, functional analysis, differential geometry, and lie groups are all demonstrated to have applications outside the hermetic confines of pure mathematics.

Stanislaw Ulam, who is most famous for his collaboration with Edward Teller on the design of the H-bomb, chaired COSRIMS's Panel of Applied Mathematics. He was also invited to write a contribution to the *Collection of Essays*, which he titled "The Applicability of Mathematics." The article to a certain degree summarizes the philosophical attitude that prevailed in the report regarding the relation between mathematics and the real world.⁸⁹ "Current research in mathematics," Ulam begins, "tends toward ever-more-varied abstraction. Yet, the most far-reaching excursion into mathematical theory may lead to application not only within mathematics itself but also in physics and the natural sciences in general."⁹⁰ According to Ulam, while most current publications in mathematics are highly specialized and detailed, they should be conceived of as "patrols" sent to unexplored lands. Some of these "patrols," Ulam explained, would "encounter new areas of interests" and result in better understanding of the natural world. As long as the "great sphere of knowledge is increasing at an important rate," Ulam argued, "the applicability of mathematics to problems suggested by new discoveries seems to know no boundaries."⁹¹ Ulam furnished his essay with several historical examples, starting from Greek geometry moving to infinitesimal calculus and up to group theory. Mathematics, Ulam

⁸⁹ The fact that the editors chose to include Ulam's article as the first essay in the collection also served as a guide for the reader as to how the collection should be read. There is no obvious connection between the essays in the collection, which range from complex analysis to mathematical economics. Ulam's essay, in trying to articulate a more general description of the relation between mathematics and application, can therefore be read as an introductory essay for the entire collection. Stanislaw M. Ulam, "The Applicability of Mathematics," in *The Mathematical Sciences: A Collection of Essays*, ed. National Research Council (Cambridge, MA: M.I.T. Press, 1969), 1-7.

⁹⁰ *Ibid.*, 1.

⁹¹ *Ibid.*

suggested, was unique among the “activities of the human mind” in that it was as much “an art for art’s sake” and a source for tangible applications.

Philosophical and historical accounts came to dominate the discussion surrounding the applicability of mathematics because mathematicians recognized that pure mathematics differed in a very fundamental way from the other sciences. It was one thing to suggest that basic research in biology on the structure of DNA might have unforeseen benefits to medicine and positive impact on citizen’s lives. But it was a completely different thing to argue that a classification of all finite simple groups would have some real-world application. Research in biology and physics regardless of scientists’ motivation was still rooted in the world. How could seemingly abstract exploration lead to concrete application? The gap, both temporally and procedurally, was just too wide. Especially at the time that many pure mathematicians insisted that the field must develop according to its own internal coherence in separation from the physical world, it was necessary to nonetheless establish the applicability of mathematics. History and Philosophy were called upon to breach the gap.

The view that mathematical theories developed in one context turn out to be applicable in unexpected places was (and still is) popularized by the mid-1960s through Eugene Wigner’s article “The Unreasonableness Effectiveness of Mathematics in the Natural Sciences.”⁹² The article was based on a talk Wigner gave at the inaugural Courant Lecture series in 1959 at New York University, in which he famously declared that the “enormous usefulness of mathematics in the natural sciences is something bordering on mysterious and that there is no rational

⁹² Eugene P. Wigner, “The Unreasonable Effectiveness of Mathematics in the Natural Sciences,” *Communications on Pure and Applied Mathematics* 13, no. 1 (1960): 1–14.

explanation for it.”⁹³ The oppositional growth of the mathematical sciences in the period extended Wigner’s thesis beyond the physical sciences. The inexplicable and unpredictable nature by which mathematical theories turn out to have real-world application permeates the entire report and the several of the contributions to the *Collection of Essays*. Wigner’s thesis was based on the assumption “that the laws of nature must be already formulated in the language of mathematics to be an object for the use of applied mathematics.”⁹⁴ The “miracle” that Wigner discussed was that mathematics, a creation of the human mind, was so effective in describing the natural world. According to this view, physicists were discovering the workings of the natural world, not constructing it. However, the “mysterious” quality that Wigner described did not really pertain to many of the recent advancements made in the report.

That Boolean algebra was crucial to the design of digital computers was not really equivalent to the applicability of matrix algebra in quantum mechanics. There was nothing bordering on the mysterious since mathematicians like von Neumann who were designing modern computer architecture were both utilizing existing mathematical theories and developing new ones. The computer did not exist independent of its construction. The applied mathematical sciences that grew out of the war were dependent on concerted efforts by mathematicians and scientists. Still the language of surprise and wonderment that was expressed by mathematicians indicates that more was at play than just the sort of rhetorical divide between basic and applied

⁹³ There is something ironic about the fact that the talk was given as the first Courant Lecture, since the attitude it fostered among some mathematicians toward applied mathematics went directly against Courant’s philosophy. In his article in *Scientific American* about “Mathematics in the Modern World,” Courant noted, “that mathematics an emanation of the human mind, should serve so effectively for the description and understanding of the physical world is challenging fact that has rightly attracted the concern of philosophers.” Yet, Courant ends his article by stating that it is the task of the “mathematicians who works with engineers and natural scientists” to “handle the translation of reality into the abstract models of mathematics.” At least for Courant, the recognition that there was a “mysterious” element in the applicability of mathematics to the natural world did not imply that the work of translating from one domain to another could not be trained and cultivated. Richard Courant, “Mathematics in the Modern World,” *Scientific American* 211 (1964), 49.

⁹⁴ Wigner, “The Unreasonable Effectiveness of Mathematics in the Natural Sciences,” 6.

research.⁹⁵ Rather, given the emphasis placed on abstraction in modern mathematical research, what was being constantly questioned by mathematicians and scientists alike was the proper relation between abstractness and concreteness in scientific work.

A section of the report entitled “the mathematical community,” gives the following description: “in a simplified way mathematics consists of abstractions of real situations, abstractions of abstractions of real situations, and so on. It is *surprising* but true that these abstractions of abstractions often turn out to further our knowledge and control of the world in which we live.”⁹⁶ The growing pervasiveness of mathematical theories and methods in what seemed to be ever-increasing domains imbued mathematicians with a sense of awe. It was a different “miracle” than the one originally expressed by Wigner, but it nonetheless seemed inexplicable. It arose not from the efficacy of mathematics in describing the laws of nature, but from its ubiquity. What mathematicians saw were disembodied mathematical ideas traveling from one domain to another. What they ignored was the incredible machinery that was put into place during the war and its aftermath to ensure that those ideas would get developed.

In 1965, Richard Hamming, a mathematician working at Bell Laboratories, published an article entitled “Numerical Analysis vs. Mathematics” in *Science*. Hamming, who spent the war working on the Manhattan Project, wished to draw attention to what he conceived of as the difference between the approach of modern mathematics and that of numerical analysis.

⁹⁵ This is not to say that the authors of the report were not concerned with the distinction between basic and applied research. In fact, in several places in the report they comment on the importance of developing basic applied mathematics. The report also includes an analysis of the distribution of federal funds between basic and applied research in the mathematical sciences. Rather, the point here is that more was at stake. Unlike in the 1950s when pure mathematicians were worried about the source of funding for the field, in the 1960s the NSF budget was continuously growing as well as the funding afforded to the field from other federal agencies. The source of confrontation was not between different groups of the mathematical community vying for the same source of funding. Rather, the criticism mathematicians were trying to fend off came from the outside. Convincing policy-makers that mathematics deserved to be funded was important, but so was establishing the prominent role of mathematics in the scientific enterprise.

⁹⁶ National Research Council, *The Mathematical Sciences*, 46.

Mathematics, Hamming wrote, “has tended to be precise in its statements of results and its rigor, while numerical analysis, and computing generally, tends to put great emphasis on the clear statement of the processes used.”⁹⁷ Numerical analysis’s emphasis on methodology, according to Hamming, implied that in some respects “the goals and objectives of computing are more in tune with the rest of our scientific culture than are those of traditional mathematics.”⁹⁸

In drawing the distinction between numerical analysis and mathematics, Hamming had two main objectives. First, he wanted to establish numerical analysis as an autonomous field with its own standards independent of those of pure mathematics. Second, he believed that numerical analysis should be developed as a scientific rather than a mathematical field. At four points in his article Hamming notes that his aim is not to criticize modern mathematics but only to point to some of the differences between the two fields. Yet this premeditated defense was of no help. In response to his article, five leading mathematicians at the Department of Mathematics at the University of Chicago wrote a letter to the editors of *Science* protesting Hamming’s “attack” on mathematical numerical analysis.⁹⁹

The authors of the letter took specific issue with Hamming’s characterization of mathematics, which they believed “seriously misconceived the nature of mathematics and its role in the scientific enterprise.”¹⁰⁰ Hamming’s main claim was that unlike in the past most of the postulates of modern mathematics, being too abstract, could not be verified via observations. The Chicago mathematicians offered a forceful defense of mathematical abstractions. After describing the work of mathematicians as an increasing process of abstraction, they concluded “it is a paradox which lies at the heart of what Wigner has called the ‘unreasonable effectiveness

⁹⁷ Richard W. Hamming, “Numerical Analysis V. Mathematics,” *Science* 148, no. 3669 (April 23, 1965): 473–475.

⁹⁸ *Ibid.*, 474

⁹⁹ The letter was signed by Adrian Albert, Felix Browder, Israel Hersten, Irving Kaplansky, and Saunders MacLane.

¹⁰⁰ A. Adrian Albert et al., “Mathematics Vs. Numerical Analysis,” *Science* 149, no. 3681 (July 16, 1965): 243–245.

of mathematics in the natural sciences' that it is these successive acts of mathematical abstraction piled upon abstraction, urged on by the force of the autonomous development of the mathematical structure, that have made mathematics a significant tool and a dynamic force in the development the physical sciences."¹⁰¹ Wigner's philosophical musing here became a defense for the development of modern mathematics as a chain of ever-increasing abstraction, or rather "abstraction piled upon abstraction."¹⁰²

In December 1970 the NSF Director William D. McElroy sent a letter to Oscar Zariski, who was at the time the president of the Society asking him for assistance in "identifying examples of how basic research has contributed to the solution of problems facing society, how such research has produced the knowledge by which man has improved his condition."¹⁰³ Zariski assured McElroy that he had plenty of such examples, only to add, "any attempt on my part of citing and discussing such examples would not measure up to what has already been done in that direction in the excellent Cosrims report."¹⁰⁴ Instead of providing McElroy with specific examples as he requested, Zariski went on to affirm the philosophical underpinning of the report, asserting that it is often the most abstract and non-directive investigations that in the long run have some benefit to society. "All this brings me to what I consider the main point to be stressed." Basic research, Zariski continued, "must stand on its own feet when it faces society, and for me, as a mathematician, the problem of justifying basic research *on utilitarian grounds*

¹⁰¹ Ibid., 244.

¹⁰² In the letter the Chicago mathematicians write: "this is not to denigrate more explicit forms of relationship between physical problem and mathematical discoveries, but rather to point up the crucial fact that the latter sort of relation is only one aspect of a deeper interconnection." Ibid., 244.

¹⁰³ William D. McElroy to Oscar Zariski, 10 December 1970, AMSR, Box 64, Folder 21.

¹⁰⁴ Oscar Zarizki to William D. McElroy, 22 December 1970, AMSR, Box 64, Folder 21.

does not exist.”¹⁰⁵ Neither during the war nor in the 1970s were some mathematicians willing to admit a utilitarian conception of mathematics.

* * *

Yet this philosophical conception that held that the relation between mathematical abstraction and real world application was verging on the mysterious had an actual affect on the training of applied mathematicians. At least in part, this view was rooted in a disembodied view of knowledge, in which ideas traveled from one context to the next, not through people. The fact that group theory found its way into modern physics is repeatedly mentioned. What is never mentioned is how these ideas traveled from one domain to another. This view was predicated on a view of the history of mathematics as a progression of disembodied theories. If there was “no rational explanation” for the applicability of core mathematics, than there was no wonder that there did not exist, as Tukey noted in 1960, a branch of mathematics analogous to the experimental divisions of other sciences. Specific areas of research, such as statistics and mathematical economics could be identified and studied but a comprehensive field whose goal was to close the gap between mathematics and natural phenomena would by definition be unattainable.

After Bers made his plea for a history of mathematics, Werner Rheinboldt, a research professor at the Institute for Fluid Dynamics and Applied Mechanics at the University of Maryland, took to the stage. Rheinboldt heard Bers’s plea. His talk focused on the transformations in applied mathematics brought on by computer science, specifically the

¹⁰⁵ Zariski goes on to ask, “how, then, do I expect public funds to support our basic research?” To which he gives the following indirect answer: “The Italian patrons of arts and sciences of the Renaissance did not ask, said the algebraist Cardano, to explain to them how his solution of the cubic equation would help their society, or, say their commerce with the East. I hope that the government leaders with whom you and I have to deal will prove to be as enlightened as those Italian Princes of the Middle Ages or the Renaissance; they have been so, to my knowledge, after World War II, until only a few years ago.”

increased use of mathematical modeling. However, his emphasis was on already existing mathematical theories that had become crucial in this endeavor. Automata theory, the theory of formal languages, and graph theory and combinatorics, he explained to the audience, were decisive in contemporary study of nonanalytic computer models. After briefly introducing each of these fields, he concluded, “not one of the three discussed here was originally thought of as having a direct connection with applied mathematics, and their development before the advent of computers took scarce notice of applications.”¹⁰⁶ It was not that mathematicians did not acknowledge that new mathematical theories often developed by considering specific physical circumstances. Yet, many mathematicians in the period chose to emphasize the opposite assertion, repeatedly pointing out how various ideas developed in one context had made their way into unexpected domains.

Not all mathematicians were pleased with this growing tendency. During the discussion that followed, Peter Lax, who was also a member of the Courant Institute, interjected:

I disagree with you in this business of how applications interact with mathematics. Maybe I am misinterpreting your remarks; they seemed to say this is best left to randomness. But as C. C. Lin remarked, there is a certain filter which influences the probabilities. I emphatically do not think it should be left to the random subconscious of the mathematical mind, but it is something that has to be fostered most deliberately.¹⁰⁷

Lax perfectly encapsulated what was at stake. If the relation between mathematical theories and the natural world were, as Bers and Reinholdt suggested, unpredictable, than nothing really could be done to train applied mathematicians. After all, it was exactly the work of the applied

¹⁰⁶ Greenberg, “Education in Applied Mathematics,” 320.

¹⁰⁷ Ibid., 322.

mathematicians to draw connections between abstract mathematics and the physical world. And the goal of training was to teach them how to do just that.

This was a real point of contention. Bers replied to Lax, “I would like to first emphasize a point which we seem to disagree. This is this matter of surprises, the cases of unexpected influence of mathematical discoveries in other fields.” He then added, “I do not believe that we should try to program education for surprises. A surprise is, by definition, something which cannot be programmed for.”¹⁰⁸ This is not to say that Bers believed that there should not be a training program in applied mathematics, but how applied mathematicians should be trained and for what goal remained debatable.¹⁰⁹ In his opening remark Lin explained that the principle theme underlying applied mathematics as an independent discipline was “the interdependence of mathematics and the sciences.” Most attendees in the conference agreed with this underlying statement, though what was the best way to implement it was far from clear. Relabeling mathematics as mathematical sciences did not in itself solve the question as to how mathematicians should interact with scientists from different fields. Since applied mathematics was, at least in principle, to serve as a sort of mediator between core mathematics and science, discussions surrounding training in the field often attempted to define the necessary or desired communication between the two.

¹⁰⁸ Ibid.

¹⁰⁹ Here again it is important to emphasize that Bers’s defense of abstract mathematics was not merely a cynical means of ensuring support to the field. More likely, it was truly a philosophical conviction. For one thing, Bers himself was an applied mathematician. However, more than that, Bers was well aware of the need to develop and teach mathematics for practical purposes. Before he began reading his prepared remarks, Bers chided some of the mathematicians in attendance:

I am afraid that during yesterday’s discussion there appeared a certain attitude of snobbishness toward technologists, toward people who merely apply mathematics, rather than create new models and methods. I think this is a wrong attitude. Let us not fool ourselves. Society holds mathematics in high regard, not perhaps as it should because of the intrinsic beauty of the subject, but because mathematics is a useful art which society needs very badly. Our responsibility is not only to train the future leaders of thought in applied mathematics, but also to teach all people who will do the work of society.

Barkley Rosser, who was the director of the Army Mathematics Research Center at the University of Wisconsin-Madison, gave the main address during the session on the research program in applied mathematics. Rosser was a logician but had spent the war years working on ballistics. Based on his experience, Rosser made an impassioned case for the active pursuit of applied mathematics. Unlike Bers, Rosser maintained that mathematical application would be developed only if mathematicians would pursue them. “You do not sit in your office with a sign on your door saying ‘Problems solved here’ and sit back and wait for people to come in with problems,” he told members of the audience. He then added, “we have somehow got to get out in the trenches and run into some good problems.”¹¹⁰ To bolster his argument, Rosser gave as an example Mark Kac, who according to him, used to go around from one Cornell department to the next asking researchers what they were doing. “He would interest himself in their problems...they would tell him, he would get all excited about this; pretty soon he was solving problems, too.”¹¹¹ The sort of activist approach Rosser advocated might have existed among some active mathematicians but it was in no way representative of most researchers.

A much more accurate description of the majority of mathematicians was probably provided by Ulam, who during one of the conference’s discussions called attention to the “catastrophic lack of awareness among most pure and applied mathematicians of what is going on in various other sciences.” In order to illustrate his point, Ulam added, “I have a feeling if you ask a random mathematician to describe in two sentences, in his own words, the difference between the neutron and electron, in the vast majority of cases you would get a complete blank, hardly any answer, or any interest, as a matter of fact at all.”¹¹² The job of the applied

¹¹⁰ Ibid., 372.

¹¹¹ Ibid.

¹¹² Ibid., 323.

mathematician was supposed to be to bridge the gap between modern mathematics and science, but as long as mathematicians could not agree what was the proper domain of the field or what was the most appropriate course of training in the field there is no wonder that it only grew wider.

Why Should Mathematicians *Want* to Talk to Non-Mathematicians?

In the same year that COSRIMS published its *Collection of Essays*, another edited volume on the nature of mathematics intended for general readership came out. *The Spirit and the Uses of the Mathematical Sciences* was published by the Conference Board, and consisted of fourteen essays about aspects of the mathematical sciences.¹¹³ The book was edited by Joachim Weyl and Thomas Saaty. Saaty, who had received his PhD from Yale University in 1953, was an expert on operation research, queuing theory, and decision-making.¹¹⁴ Unlike the *Collection of Essays*, the essays in Weyl and Saaty's edited volume were divided into three sections. The first, "A Basic Form of Creative Thought," included among others essays by Raymond Wilder and Marston Morse. Wigner's "unreasonableness" lecture was reprinted in the second section, "A Medium for Understanding Nature." Finally, "A Challenge of Living Structures" was composed of essays about mathematical methods in biology, economics, and the social sciences. In the introduction to the volume, Weyl and Saaty explain that the motivation behind the current publication was the feeling that despite the spread of mathematical methods and forms of reasoning to all aspects of

¹¹³ Thomas L. Saaty and Fritz Joachim Weyl, *The Spirit and the Uses of the Mathematical Sciences* (McGraw-Hill, 1969).

¹¹⁴ Saaty was a prolific writer. In the decade and half following his graduation, he published seven books on various aspects of mathematics. For example, he published *Modern Nonlinear Equation* in 1965 and *Mathematical Models of Arms Control and Disarmament* in 1968.

modern life, the public's understanding of mathematics was highly inadequate. According to Weyl and Saaty, the book was intended not only for the literate layman, but also for "administrators in government agencies who must make decisions about supporting mathematical scientists, high school and college students who are considering careers in mathematical fields, executives who have been advised to strengthen the analytical capabilities of their staff, and journalists from all news media who need some background to understand the contemporary preoccupation with mathematics." Similar to the COSRIMS's publication, *The Spirit and the Uses of the Mathematical Sciences* was driven by a strong desire to influence the public perception of mathematics as a means of affecting real change on a national scale.

Yet the organization of the book into three separate sections does point to one more implication of the re-conceptualization of the field under the mathematical sciences. The dual nature of mathematics as a creative intellectual activity and a scientific tool had been well acknowledged for centuries, but expanding the confines of the field only served to cordon off the domain of pure or core mathematics. Put somewhat differently, as the "mathematical sciences" turned increasingly outward, "mathematics" could safely turn inwards. This was not the 1950s oppositional image of the field. It was the abundance of mathematical applications and computing that empowered core mathematicians to pursue their independent research.

Throughout the 1960s, the *Mathematical Reviews* (MR), the leading reviewing journal of the mathematical community, ballooned. An ad hoc committee established in 1962 to study the coverage of the *Mathematical Reviews* put things succinctly when they began their report stating "the increased rate of mathematical production causes headaches for MR." The problem, though, was not just the increase in size. "Of greater impact," the committee continued, "is the change of

character of various fields.”¹¹⁵ Symbolic logic, computing, and operation research, the committee noted, expanded the breadth of the *Mathematical Reviews*. Whereas in the past, the tendency was to be as inclusive as possible, the committee suggested that in the future “only mathematical papers should be reviewed and reviewing should be from a mathematical point of view. The importance of a paper of applications or teaching...is irrelevant for MR.”¹¹⁶ To ensure prompt coverage and to better serve the mathematical community, coverage, the committee suggested, had to be more selective.

Four years later as the production of mathematical research continued to grow, the selection for inclusion became even stricter. Starting in 1967, the *Mathematical Review* followed a coverage policy that specifically sought to exclude “routine applications of known mathematics.”¹¹⁷ The effects of this new policy were immediately noticeable. Whereas in 1965 and 1966 the percentage of reviews in the “applied” sections was about 33%, in the last volume of 1967 it dropped to 14%.¹¹⁸ Noting the effect of this new policy, the editorial committee admitted that in some cases making the distinction between pure and applied was quite difficult. Section 93 (Control Theory), they noted, “always contained quite a few paper in ‘pure’ mathematics.” In conclusion they observed, “none of the dividing lines are sharp, but the net effect is discernible in the general trend of the figures: a larger total number of reviews, a smaller proportion of applied mathematics, very little of what could be called physics.”¹¹⁹

Acknowledging that any attempt to distinguish pure from applied was by definition artificial, mathematicians were nonetheless drawing new boundaries around their discipline. In the case of

¹¹⁵ “Report on Mathematical Reviews Coverage,” 22 March 1962, AMSR, Box 16, Folder 65.

¹¹⁶ Ibid.

¹¹⁷ “Editorial Committee For Mathematical Reviews,” AMSR, Box 16, Folder 128.

¹¹⁸ In the *Mathematical Reviews*, the “pure” sections are those numbered from 00-65, while the “applied” sections are those from 68-94.

¹¹⁹ Ibid.

the *Mathematical Reviews* this was done in part due to the unprecedented expansion in the production of research papers, but these boundaries were also being marked into the field's institutional formation.

At the same time that the image of the mathematical sciences was being promoted on a national level, the four major areas of the applied mathematical sciences identified in the COSRIMS report (i.e., statistics, computer science, operation research, and physical applied mathematics) were beginning to be increasingly separated on an institutional level. The separation of statistics, operation research, and computer science from mathematics was conceived of in terms of increased autonomy. As these fields became independent they followed their own sets of standards, which were not dictated by those of pure mathematics. For example, mathematical statistics in the United States began to emerge as a separate professional identity already in the 1930s.¹²⁰ Yet it was during the 1950s and the 1960s that departments of statistics began to appear in universities around the country. By 1967 there were about 31 independent departments of statistics (at the time there were 126 mathematics departments) in the United States, and the number of PhDs granted in the field doubled from 67 in 1961 to 132 in 1968.¹²¹

As far as computer science was concerned, institutional transformations in the 1960s were pivotal. The first Department of Computer Science was established at Purdue University in 1962. By 1967, when the Survey of the Conference Board was collecting statistics, they identified 24 departments of computer science across the country.¹²² The development of each of

¹²⁰ During the war, the field had undergone a considerable expansion through mathematicians' work on several defense-related research projects. See: Hunter, Patti. "Drawing the Boundaries: Mathematical Statistics in 20th-Century America" *Historia Mathematica* 23 (1996): 7-30.

¹²¹ On the growth of statistics departments in the U.S. see: Alan Agresti and Xiao-Li Meng, *Strength in Numbers: The Rising of Academic Statistics Departments in the U. S.: The Rising of Academic Statistics Departments in the U.S.* (New York, NY: Springer, 2012).

¹²² There were earlier programs in several universities under different names before then. For example, the University of Michigan had a Communication Science Program, and the University of Pennsylvania has a Computer

these departments depended very much on local circumstances, but throughout the 1960s these programs gained further independence and autonomy from the mathematics departments. For example, George Forsythe, who was a member of COSRIMS, was the driving force behind the establishment of the Department of Computer Science at Stanford University in 1965. A Computer Science Division was established already in 1961, but was subsumed under the Department of Mathematics. When in 1964, Forsythe began recruiting faculty to Stanford, he described the status of the division, saying: “The role of the Computer Science Division is likely to be increasingly divergent from that of Mathematics. It is important to acquire people with strong mathematics background, who are nevertheless prepared to follow Computer Science into its new directions.”¹²³ As Computer Science grew independent over the 1960s, its ties to mathematics were becoming looser.

In 1968, Forsythe published an article entitled “What to Do Till the Computer Scientist Comes,” in the *American Mathematical Monthly*. The goal of the article was to make the case for computer science to mathematicians. Reminiscent of contemporary writing about mathematics, Forsythe begins his article by asking the simple question, “What is computer science anyway?”¹²⁴ After describing the three main areas of research--design of hardware, design of languages and software, and methodology of problem solving with computer--Forsythe noted that considerable discussion was dedicated to the question of whether computer science was part of mathematics. “In a purely intellectual sense such jurisdictional questions are sterile and a waste of time,” Forsyth proclaims. Yet having dismissed these debates as a waste of time, he goes on to say, “on

and Information Sciences Program. For more about the early institutional development of computer science, see: Mahoney, Michael. “The Histories of Computing (s).” *Interdisciplinary Science Reviews* 30, no. 2 (2005): 119–135.

¹²³ Quoted in Knuth, “George Forsythe and the Development of Computer Science,” 723.

¹²⁴ George E. Forsythe, “What to Do Till the Computer Scientist Comes,” *The American Mathematical Monthly* 75, no. 5 (May 1, 1968), 454.

the other hand, they have great importance within the framework of institutionalized science.”¹²⁵

Three years after he helped spur the establishment of an independent department at Stanford, Forsythe felt confident to declare that it was inevitable that computer science would develop under a separate organizational structure independent of both mathematics and engineering.

What Forsythe recognized was that the institutional and the intellectual formation of the field are not separate. It might be difficult to clearly distinguish mathematics from computer science intellectually, but it does not mean that the two could not be developed as two autonomous disciplines advancing differing agendas.

In writing for a mathematical audience, Forsythe wished to highlight some of the way by which computing intersected with mathematics. “Applied mathematics,” he explained, “is no longer the same subject, now that you have a magnificent experimental tool at hand.” He consequentially concluded that mathematicians should not only learn how to program in some language, but they should also try to encourage computer scientists to join their universities. However, having encouraged mathematicians to welcome computer scientists to their campuses, he immediately added, “above, all please don’t judge him as a mathematician, for he isn’t one and isn’t supposed to be one – his values are different.”¹²⁶ Forsythe does not go into great detail describing in what way the computer scientists’ values are different than the ones of the mathematician, but what becomes clear once again is the entanglement of epistemological commitments, institutional formations, and intellectual distinctions.¹²⁷ The mathematical sciences were growing further apart.

¹²⁵ Ibid., 455.

¹²⁶ Ibid., 459.

¹²⁷ Forsythe does reference here Hamming’s *Science* paper.

At the same time, most departments of mathematics continued to graduate students whose research was in core mathematics. During the 1960s as the production of doctorates in mathematics was rapidly expanding, approximately 65% of all annual mathematics PhDs were in areas of core mathematics. Algebra, number theory, geometry, topology and analysis represented the lion's share of research produced during the decade. Several of the attendees at the 1966 conference suggested that the best way to ensure the growth of the field was to establish schools of mathematical sciences. George Dantzig, for example, argued during his talk that the expanding scope of mathematics made it clear that "mathematical activities within a university must be reorganized to reflect these developments."¹²⁸ Such a School of Mathematical Sciences, Dantzig maintained, would increase communication between pure and applied fields and ensure the healthy growth of the field. Chern, Forsythe, and Blackwell advanced a similar view in their report calling for the establishment of a school of mathematical sciences, separated from the humanities and the sciences. Such schools, however, did not become the norm. Instead, as statistics, operation research, communication, biostatistics, mathematical economics and computing gained autonomy, the independence of traditional departments of mathematics was only further established. To a certain degree the successful growth of the mathematical sciences also brought about their dissolution.

By the end of the decade, these fissures were also felt at the level of national organizations for the mathematical sciences. By 1969, both the *Society* and the *Association* established committees to investigate the relation between their respective organization and the Conference Board. The driving force behind the work of both committees was the feeling that in its decade of operation the Conference Board did not register any meaningful achievement in

¹²⁸ Greenberg, "Education in Applied Mathematics," 353

terms of national policy for mathematics. When Gail Young, who was the president of the *Association*, discovered that the *Society* was also considering its membership at the Conference Board, he wrote to Murray Protter, who chaired the AMS's committee, suggesting that the two societies collaborate in their deliberations. Young noted that while he personally was a "supporter of the idea of a super-organization," there was no question in his mind that so far the Conference Board had not functioned.¹²⁹

When Saunders MacLane got his hands on Young's letter, he replied to Young proclaiming, "it is not enough to observe that the CBMS has been fragile. One must ask *why* it was fragile? I suspect that in part this was because CBMS on occasion set out to do things which part of its constituency – notably the AMS – did not want done."¹³⁰ More than ten years had passed since MacLane first voiced his suspicion of an umbrella organization for mathematics and the experience of the Conference Board in the decade that passed seemed to have justified his initial suspicion. However, in the time that passed MacLane also changed his mind. He now believed that a national organization responsible for representing the interests of the mathematical community on a national level was necessary. His main qualms were with the Conference Board, which he believed was organizationally weak and tended to deal with peripheral matters instead of actual policy concerns. In fact, a couple of months after he sent his letter to Young, MacLane circulated an informal memorandum entitled "A Global Organization for American Mathematics."¹³¹ In the memorandum, which he circulated among a small number of members of both organizations, MacLane contended that the national representation of mathematics had been hampered for years due to existence of multiple separate organizations. "I

¹²⁹ Gail S. Young to Murray Protter, 17 July 1969, AMSR, Box 60, Folder 68.

¹³⁰ Saunders MacLane to Gail Young, undated, AMSR, Box 60, Folder 68.

¹³¹ Saunders MacLane, "Global Organization for American Mathematics: An Informal Examination of a Possible Development," 18 February 1970, AMSR, Box 62, Folder 83.

submit that there is a real need for a strong mathematical posture on questions of national science policy and that this need will grow greater with the shortage of funds and the inevitable wider application of mathematical methods.” MacLane then added, “there seem to me two essential conditions for such an organization: First, it is concerned with mathematics (which has existed for millennia) and not with that novelty ‘mathematical sciences.’ Second, it has direct individual membership.” By 1970, the ties that had kept the mathematical sciences together were beginning to come apart.

As long as the field was expanding during the 1960s, coalitions between the mathematical sciences could be maintained. Different constitutions might have had conflicting agendas at the time, but as long as the money was pouring in and growth was assured, these factions were able to work together. By the end of the decade, the successful growth of the various fields only served to separate them. The looming financial troubles brought these changes to the front. In his memorandum, MacLane suggested that another more “heroic” solution would be to unite the *Society*, the *Association* and SIAM. Notably, this new proposed organization not only did not include the national teachers association but also excluded the statisticians, computer scientists, and operation researchers. What was left was mathematics, “which has existed for millennia.”

Chapter 5.

Q.E.D.:

Mathematics and the Job Market Crisis

“Why then do I ask, ‘Is there a crisis in science?’” President Johnson’s Science Advisor Donald Hornig was frustrated. During a speech at Carnegie Mellon University on May 10, 1968, he assured the audience that the answer was not the tightening of universities’ budgets. “The question I would like to ask,” he explained, “is whether there is something deeper?” He meant something inherent in contemporary scientific enterprise that could explain its present predicament. Recently, he observed, Congressmen as well as the public had begun to question whether the “violent expansion” in science over the previous two decades was desirable or even beneficial. What Congressmen see when they survey the current state of science, he continued, is a community “which, insisting on its purity, will not deign to communicate with the public and justify itself, but prefers to believe that its virtues are so self-evident that a right-minded society must necessarily support it on its own terms.” Even though the scientific community, Hornig noted, consumes approximately a quarter of the disposable federal budget, it was selfishly more concerned with the needs of science than with those of the nation. He added:

This blithe spirit leads mathematicians to seriously propose that the common man who pays the taxes ought to feel that mathematical creation should be supported with public funds on the beaches of Rio de Janeiro or in the Aegean Islands. I don't doubt for a minute that mathematical creation is possible under those circumstances; it may even be improved. But the public which pays the bill is not in tune with such colossal intellectual conceit.¹

¹ “A Point of View,” *Science* 161 No. 3838 (19 July 1968), 248.

When *Science* published excerpts from the speech, it sent the mathematical community into a frenzy. “Many mathematicians were dismayed and shocked,” announced the President of the AMS Charles Morrey, in a letter to the editors of *Science*. Such an attack on mathematics and mathematicians, he asserted, “was completely uncalled for.”²

Why did Hornig single out *mathematicians* in his speech as the emblematic manifestation of this spirit of purity that affected the scientific community? In part, the answer, as almost all contemporaries at the time recognized, was hidden in Hornig’s reference to the beaches of Rio de Janeiro. As Morrey noted in his response, implicit in Hornig’s remark “was a thinly veiled attack on Dr. Stephen Smale.”³ Two years earlier, Smale had been involved in a highly publicized controversy between the National Science Foundation, Congressman Richard L. Roudebush (R-Ind.), and the University of California, Berkeley. A celebrated topologist and an outspoken activist against the Vietnam War, Smale’s NSF research funds were (temporarily) revoked when upon receiving the prestigious Fields Medal at the International Congress of Mathematics in 1966, he publically denounced the United States involvement in Vietnam. To be fair, in his brief remarks to the press on the steps of Moscow University, Smale also criticized the Russian government for its human rights violations and its military involvement in Hungary a decade earlier. Yet this did not protect him from the wrath of Congressmen Roudebush, a member of the House Committee on Science and Astronautics, who made it his personal goal to ensure that

² Charles B. Morrey, “Mathematics: Pro Bono Publico,” *Science* 162, No. 3853 (November 1, 1968): 514–515. Before Morrey sent this official letter to the editors of *Science*, Felix Browder, S. S. Chern, Richard S. Palais, and Protter drafted a longer reply, which they submitted to the council of the AMS for approval. Their letter presented a more forceful attack on Hornig and federal science administrators more broadly. “Instead of supporting the basic needs of scientific development before the public, this class of administrators seems to be happiest in devoting their energies to attacking one of the most cherished principles of the scientific ethos: the autonomy of the individual scientist in determining for himself how he works and what he works upon.” The letter writers further accuse Hornig for disregarding the “major problems” that face the scientific establishment, such as the current draft policy and the suspension of fellowship programs for graduate students. “To the editors of *Science*,” undated, AMSR, Box 16, Folder 140.

³ *Ibid.*

Smale would not be entitled to receive any federal funds. The affair lasted for about a year and was exhaustively covered in *Science* by Daniel Greenberg.⁴

When he returned from Europe, Smale was asked to provide an account of his work during the summer. Since he used funds from his NSF grant to cover his travel expenses, Smale was required to demonstrate that he had worked for at least two full months during his summer travels. This was an uncommon practice, but Smale quickly complied. In a letter to Berkeley's Vice Chancellor Connick, Smale gave a detailed account of his summer travels. He listed the universities and conferences he had attended, clearly indicating that he was officially working for a period of at least eight weeks. Yet, in his letter to Connick, Smale made sure to point out that he was in fact "doing mathematics" throughout the summer in campgrounds and hotel rooms. Even during his boat trip on the S. S. France from Le Havre to New York, he wrote to Connick, he was discussing problems with a fellow mathematician and was working at the boat's lounge. To prove his point, he added as an aside, "(My best-known work was done on the beaches of Rio de Janeiro, 1960)."⁵ Hence, a mathematical legend was born.⁶

By 1968 the controversy was considered by most participants to be over. Hornig's allusion to it in his speech, however, makes sense considering that it included many of the components that were now contributing to the "crisis" in science. Campus unrest over the

⁴ Daniel S. Greenberg, "The Smale Case: NSF and Berkeley Pass Through A Case of Jitters," *Science* 154, no. 3745 (1966): 130–133; Daniel S. Greenberg, "Smale and NSF: A New Dispute Erupts," *Science* 157, no. 3794 (1967): 1285–1285; Daniel S. Greenberg, "The Smale Case: Tracing the Path That Led to NSF's Decision.," *Science* 157, no. 3796 (1967): 1536; Daniel S. Greenberg, "Smale: NSF Shifts Position," *Science* 158 (1967): 98; Daniel S. Greenberg, "Smale: NSF's Records Do Not Support the Charges," *Science* 158, no. 3801 (1967): 618–619; .

⁵ "The Case of Stephen Smale," *Notices of the AMS* 14 (1967): 778–782; "The Case of Stephen Smale: Conclusion," *Notices of the AMS* 15 (1968): 49–52. For more on Smale, see: Steve Batterson, *Stephen Smale: The Mathematician Who Broke the Dimension Barrier* (American Mathematical Society, 2000).

⁶ For Smale's generation the story became a legend. It was covered in *Science* and the *Notices of the AMS* at the time, and Smale's celebrity status within the community (being a Fields Medalist) only helped raise its profile. Smale himself promulgated the story. See for example, Steve Smale, "The Story of the Higher Dimensional Poincaré Conjecture (What Actually Happened on the Beaches of Rio)," in *From Topology to Computation: Proceedings of the Smalefest*, 1993, 27–40. Most recently, the story was briefly mentioned by mathematician Gregory Buck in the *New Yorker* Out Loud Podcast. The *New Yorker*, *Out Loud: Thought Crimes*, January 7, 2013.

Vietnam War and concerns over the accountability of federally funded scientists began signaling the end of the golden age for science that had dominated American higher education in the previous two decades. The post World War II era was coming to an end, and in what felt like overnight, scientists began experiencing what Daniel Keveles described as “an employment squeeze reminiscent of the 1930s depression.”⁷ Historians have noted that several factors contributed to this transformation. A rise in expenditures forced universities around the nation to tighten their budgets, while at the same time these universities were losing the endorsement of the general public. Research for its own sake was no longer deemed a defensible cause. Instead, an emphasis on applicability and on public service was placed front and center.⁸ As far as scientific research was concerned, however, it was the overall decrease in federal support for research and development and the changing emphasis on applied as opposed to basic research that had the greatest affect on the scientific community.⁹ The deeply entrenched mathematics profession, needless to say, was profoundly affected by these transformations.

Smale’s case, albeit the most famous, was just one of several public controversies that shook the mathematical community in the late 1960s.¹⁰ When universities began cutting down

⁷ Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, MA: Harvard University Press, 1995), 421.

⁸ Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities Since World War II* (New York, NY: Oxford University Press, 1993); David Kaiser, “Cold War Requisitions, Scientific Manpower, and The Production of American Physicists After World War II,” *Historical Studies in the Physical and Biological Sciences* 33, no. 1 (2002): 131–159; David Kaiser, *American Physics and The Cold War Bubble* (Chicago, IL: University Of Chicago Press, forthcoming); Kevles, *The Physicists*.

⁹ In 1969, for example, when President Nixon came into office while he increased the NSF budget, after inflation the total money that actually went into R&D had actually decreased from the previous year. Moreover, within the budget itself stress was placed on funding applied research that would benefit social causes. Kevles, *The Physicists*, 412.

¹⁰ In the late 1960s, the American mathematical community was consumed by one scandal after another. Debates over the role of mathematicians in DOD military funded research, Vietnam War dissenters, and Civil Rights issues consumed the membership of the profession. This was a turbulent time for the Society, whose members were fiercely divided on whether the organization should get involved in these social and political affairs or limit its operations to issues pertaining solely to mathematical research. The pages of the *Notices* became a battleground for the different factions within the Society, and despite some members’ hopes all attempts to isolate mathematical research *per se* failed. For example, in 1969 the Society was scheduled to hold its April meeting in Chicago. Yet,

their faculties and the NSF decreased its funds to mathematical research and education, the mathematical community together with the rest of the sciences clearly received the brunt. Unemployment began to rise and anxiety regarding the future development of the field became an unavoidable reality. Consequentially, mathematicians began asking what had gone wrong, and pondering what could be done. In seeking an answer to both questions, mathematicians turned their attention to doctoral training in the field. The discussions that followed brought to light many of the tensions that dominated the growth of the field in the aftermath of World War II. The increase in the number of mathematical doctorates was necessitated by a demand for college mathematics teachers as well as a presumed need for industrial mathematicians. Yet the mathematical training most students received during the 1950s and 1960s did not prepare them adequately for either of these jobs. Their tendency to be highly specialized made them unsuitable for undergraduate teaching, and their inclination to focus on core mathematics implied they were, for the most part, also unqualified for non-academic research. Doctoral programs in mathematics were focused on producing academic research mathematicians. As long as new departments were established and existing ones were expanding, mathematics PhDs were able to find “suitable” jobs. However, once these jobs began to dry up, calls for reform were heard from every direction.

following the demonstration and violent response of the Chicago Police Department during the Democratic Convention the previous August, members of the Society felt that the location should be changed. In January 1969, during a business meeting of the Society it was decided to move the April meeting to Cincinnati. After the announcement appeared in the *Notices*, members inundated the offices of the editors with letters either praising the council for its action or criticizing it for its political involvement. “That the AMS should take this forthright step makes me proud to be among its members,” wrote one mathematician. Whereas, another offered, “mathematicians object when politics enters mathematics so they should see that mathematics keeps out of politics also.” By February, approximately 360 members wrote in expressing their opinions. “April Meeting in the West: Some Reactions of the Membership to the change in Its Location,” *Notices of the AMS* 16 (1969): 485-487. On the Chicago Democratic convention see: David Farber, *Chicago '68* (University of Chicago Press, 1994).

The employment crisis placed front and center the paradox at the heart of the field. Mathematics had enjoyed the fiscal benefit of the boom years like the rest of the sciences, yet in many ways the field continued to resemble a humanistic pursuit. The rate of growth of the field, its expansion in scope, and its funding structure resembled the physical sciences. Yet the nature of graduate training in mathematics, the emphasis on individual work, and the patterns of graduate employment were closer to that of humanistic fields. The flexibility inherent in the field had enabled mathematicians to track between the two throughout the postwar period. When the employment crisis hit, it was not quite clear how or whether the two could be reconciled.

Defining Demand - Diagnosing the Problem

In his letters to the editors of *Science*, Morrey criticized Hornig for claiming that the scientific community had not made any attempt to communicate its ideas to the general public. Morrey claimed that on the contrary many branches of science prepared extensive reports in hopes of doing just that. “In particular,” he wrote, “the mathematical community through the medium of the COSRIMS committee has just completed a comprehensive report, designed for the public and Congress, on the current problems of mathematical research and their relations to the national goals.”¹¹ The so-called mathematization of society, which the COSRIMS report described in great detail, was after all the mathematical community’s attempt to justify itself to the public.¹² In making its case for increase in federal support, the report aimed to show the relevance of mathematics to ever-growing domains of knowledge. However, the report’s main

¹¹ Morrey, “Mathematics.”

¹² Morrey was not the only one to point to COSRIMS in response to Hornig’s speech. In a letter to Columbia University’s Vice President, David Truman, mathematician Serge Lang also argued that the mathematical community tried to communicate its work to the public. “Professor Lipman Bers and many other mathematicians worked very hard for two and a half years on the COSRIMS report.” Serge Lang to David Truman, 25 July 1968, AMSR, Box 16, Folder 140.

recommendations were deemed invalid before the ink had even dried. The report, which was published in 1968, called for continuing the massive expansion of graduate education and warned against a looming shortage of college mathematics teachers. Its authors cautioned that even if the federal government continued its support of mathematics at the current rate “the mathematical community will not grow fast enough to meet national needs.”¹³ By 1969, only a year later, it had become clear that far from growing too slowly, mathematics was growing too fast.

In calling for continued expansion in higher education, the report based its estimates on what was at the time an ongoing shortage of mathematical college teachers. During the 1960s, undergraduate enrollment in mathematics rose faster than in any other scientific field.¹⁴ Since the rate of growth of mathematics doctorates was in no way comparable to that of bachelors, there was a noticeable shortage of qualified mathematics teachers around the country. In 1966, the Conference Board Survey found, for example, that only 48% of all mathematics college faculty held a PhD, by far one of the lowest percentages among all other academic fields.¹⁵ This demand for mathematics college teachers implied that, at least at first, the mathematical community was shielded from experiencing the full ramifications of the new economic reality. In 1969 and 1970 when young physicists and engineers were desperately searching for jobs, mathematicians were still able to obtain teaching positions and unemployment was not yet a pressing problem.

¹³ National Research Council, *The Mathematical Sciences: A Report* (Washington, DC: The National Academies Press, 1968), 14.

¹⁴ In the decade from 1955-1965 the annual number of BAs in mathematics and statistics increased by a factor of 4.9, compared to factor of 1.6 for engineering, 1.7 for the physical sciences, and 2.8 for the biological sciences. *Ibid.*, 122.

¹⁵ The comparable numbers for other scientific fields was 78% for the biological sciences, 80% for chemistry, and 69% for physics. The report further noted that “of all the standard liberal arts fields mathematics had in 1962-1963 a smaller percent of doctorates on the faculty of any fields except for English and the fine arts. Only when vocational and professional fields such as agriculture, business, and law are included does the percentage of doctor in mathematics approach the average.” John William Jewett and Clarence Bernhart Lindquist, *Report: Aspects of Undergraduate Training in the Mathematical Sciences* (Conference Board of Mathematical Sciences, 1970).

However, it quickly became apparent that this delay would not last for long and that unemployment would soon catch up with the profession. The question that troubled mathematicians was how warnings about an insatiable shortage of mathematics PhDs had turned overnight to alarms about oversupply.

While many contemporaries were quick to note COSRIMS's erroneous projections, the report was not considered to be responsible for the current overproduction of mathematicians.¹⁶ After all, by the time the committee began its work, the expansion in higher education was already well underway. COSRIMS's predictions might have been completely inaccurate, but they did not, in and of themselves, explain why there were now too many PhDs struggling to find jobs. Instead, mathematicians pointed to the President's Science Advisory Committee's 1962 "Gilliland report."¹⁷ The report, which called for an increase in the production of science and engineering doctorates, had been instrumental in initiating the major increase in mathematics PhDs during the 1960s. For example, in 1966-1967 the government supported a third of all graduate students in mathematics. Through a host of federal programs, higher education in mathematics mushroomed during the 1960s. When the Gilliland report was published in 1962, the annual number of PhD's in mathematics was 396; by 1972 the number had more than tripled reaching an all time high of 1,281. The expansion was nothing if not rapid. However, what the effects of this growth were for the mathematical community was contested.

¹⁶ In January 1970, during a winter meeting of the Association in San Antonio, a discussion panel entitled "COSRIMS Reports –Retrospect and Prospect," was organized to consider how to implement some of the recommendations made in the report. Yet, when the panel's chair Arnold Ross open the discussion he immediately noted that "much has happened since the writing of the report which put to a severe test the comfortable assurance of our mathematical community." New realities, Ross continued now faced the mathematical community and required "critical reappraisal" of academic responsibilities. "COSRIMS Reports--Retrospect and Prospect," *The American Mathematical Monthly* 77, no. 5 (May 1, 1970).

¹⁷ The report's official title was Meeting Manpower Needs in Science and Technology. United States President's Science Advisory Committee, *Meeting Manpower Needs in Science and Technology: A Report* (1962).

For example, as part of a correspondence in 1971 concerning how the AMS should rethink the organization's relation to the government, Felix Browder, a mathematician at the University of Chicago, suggested in a letter to Lowell Paige, "we might even take the revolutionary step of repudiating the disastrous goals set for mathematics ten years ago in the Gilliland report of ever-larger number of Ph.D.'s in Mathematics."^{18,19} William Duren, the Dean of the College of Arts and Sciences at the University of Virginia, was more direct. "Manpower studies such as the Gilliland report of President Kennedy's Science Advisory Committee," he wrote in a discussion paper in 1972, "were all framed in terms of a predicted shortage of an important commodity (scientific research and know-how) that should be produced and stockpiled like nerve gas in Nevada."²⁰ It was irrelevant to those in charge, Duren blasted, that those produced goods were in fact "our best young people" who spent the best years of their lives under false promises.²¹ From a federal perspective, "Ph.D.'s are just like potatoes. When there is a shortage, production is subsidized, but when the shortage eases, good business sense requires

¹⁸ Felix Browder to Lowell Paige, 29 November 1971, SMP, Box 6.4/AAM 86-10/4, Folder "AMS: Government Relations, Committee On – Part One."

¹⁹ Paige was at the time Dean of the Division of Physical Sciences in the College of Letters and Science at the University of California, LA. In 1973, President Nixon appointed Paige to be Assistant Director of the NSF.

²⁰ William L. Duren, "Employment and Educational Policy in Mathematics: A discussion paper for the April 27, 1972, meeting of the AMS Committee on Employment and Educational Policy (CEEP)," SMP, Box 6.4/AAM 86-10/4, Folder "AMS: Employment and Educational Policy, Committee On (1973), April-May 1972."

²¹ As David Kaiser showed, this analogy between the number of PhDs and stockpiles of weapons was exactly the one physicists and policy makers used when they called for increase in scientific manpower in the 1950s. Of course, following the public unrest over the Vietnam War, making such a comparison between chemical weapons and graduate students was much more controversial. Yet Duren's charge also pointed to some key differences between the mathematical and the physical science community. In the aftermath of the war, mathematicians maintained a complicated relationship with the scientific establishment. The philosophical divide that pitted mathematicians and physicists against one another did not disappear at the end of the war. During the 1950s mathematics grew at a slower rate compared to physics and the mathematical elite, as evident through its ongoing fights with applied mathematicians, continued to fight against a utilitarian conception of the field. The discourse surrounding manpower was predicated after all on the conception of scientific personnel as future weapon producers and for their potential support of military operations. Calls for expansion in doctorates were conceived of as coming from outside the profession and were therefore treated with more scrutiny by the mathematical profession. It was one thing for Henry DeWolf Smyth to argue that more physicists must be trained to ensure the United States military might, but it was a different thing for him to call for the production of more mathematicians. Mathematicians were well aware of this situation, and Duren's anger can be seen as directed exactly against scientists in high policy positions. Kaiser, "Cold War Requisitions, Scientific Manpower, and The Production of American Physicists After World War II."

that production be curtailed. The insensitive and inhumane aspect of this when applied to young people gets lost in the model and the budgetary figures.”²² When Saunders MacLane read a copy of the memorandum, he wrote to Duren that while his historical discussion of the growth of the system was interesting, it did not sufficiently highlight the fact that the mathematical community was not consulted in these policy discussions. “[F]or example, it had hardly any input in the formulation of the *notorious* Gilliland report (my adjective and my italics),” he wrote to Duren.²³

While he was highly critical of the model of supply and demand that dominated manpower decision-making at the federal level, Duren was less accusatory when it came to the overall growth of the field. In the long run, he wrote to MacLane, this growth would serve the mathematical community well. “The bad thing about the federal policies resulting from the Gilliland report was that expansion was too rapid for maintenance of quality and then the cut off was too sudden for decent treatment of young people, and universities too.”²⁴ As long as the field was expanding, he explained, there were positions to be filled.

MacLane was not convinced. In his reply to Duren, he repeated his charge that the mathematical community had not been adequately consulted and emphasized that the expansion was indeed too fast to maintain quality. “I think these two things are enough to make this report a very serious mistake.” He then added, “mathematicians should say so publically.”²⁵ In pointing to the Gilliland report and in arguing against its “unreasonable” goals for expansion, mathematicians were in a sense shifting the responsibility from the community to the federal government and to policy makers. The problem, according to them, was that the government’s

²² Ibid.

²³ Saunders MacLane to William L. Duren, 17 April 1972, SMP, Box 6.4/AAM 86-10/4, Folder “AMS: Employment and Educational Policy, Committee On (1973), April-May 1972.”

²⁴ William L. Duren to Saunders MacLane, 8 May 1972, SMP, Box 6.4/AAM 86-10/4, Folder “AMS: Employment and Educational Policy, Committee On (1973), April-May 1972.”

²⁵ Saunders MacLane to William L. Duren, 17 May 1972, SMP, Box 6.4/AAM 86-10/4, Folder “AMS: Employment and Educational Policy, Committee On (1973), April-May 1972.”

demand for an ever-increasing number of PhDs was not sufficiently discussed with members of the mathematical profession.

The concern, however, was not so much the quality of research, but rather the uneasy tension between mathematicians and the scientific establishment. Despite MacLane's and Browder's criticism, mathematicians hailed the growth of the field during the postwar period as a golden age for American mathematics. The new generation of American mathematicians was praised as far superior to the one that preceded it. New results and mathematical theories were discovered and American mathematics, it was asserted, was leading the international mathematical community.²⁶ Homological algebra, algebraic topology, differential geometry, and finite group theory were just a small number of fields recognized to be at the forefront of academic research. The period was one of prosperity. What the discussion concerning the Gilliland report highlights was the belief that mathematics and mathematicians were not fully integrated into the national scientific establishment. The tension between the mathematical elite of the country and the newly formed civilian scientific authority did simply dissipate at the end of the war. Through the 1950s and 1960s, mathematicians' influence on decision making in Washington grew, but to a certain degree mathematicians continued to feel, as they did during the war, like outsiders to the scientific establishment.²⁷ Hornig's attack on mathematicians in his

²⁶ An example of a typical assessment of the field can be found at the report to the council of the AMS in 1974. "During the past two decades... American mathematics has led the international mathematical community in a burst of mathematical development which has made this one of the golden periods of the long history of mathematics. Many of the great classical problems of nineteenth and early twentieth century mathematics have been solved... The technical power displayed in the solution of these problems has corresponded to a remarkable conceptual development which has seen the total transformation of many classical fields of both pure and applied mathematics and the development of new and unexpected links between these fields which are only beginning to be fruitfully explored. "A Report to the Council of the AMS," *Notices of the AMS* 1974, 84.

²⁷ The COSRIMS report gave some indication into this state of affairs when it acknowledged under the section "Criticisms and Tensions" the friction between the mathematical and scientific community. As described in the previous chapter, mathematicians were often criticized for growing further apart from the rest of the sciences.

1968 speech was only a further proof of the unavoidable divide between mathematicians and scientists.

In 1967, MacLane gave a speech about the future role of the federal government in mathematics during the fiftieth anniversary celebrations of the Association. Noting the growing class of scientists involved in policy considerations, MacLane declared, “If I define a mathematician to be one who has contributed to our science at least one idea, process, or theorem that others have used, then I know of exactly two mathematicians who are part of the establishment.”²⁸ MacLane’s arithmetic is difficult to recreate, but his overall assessment seems to be correct.²⁹ For example, besides Tukey, who served on the President Science Advisory Committee between 1960 and 1963, the group did not include any other mathematicians. Considering that Tukey was in fact a statistician, then it is fair to say that PSAC never included a mathematician among its members. While mathematicians might not have had a strong influence in matters of national science policy, they nonetheless maintained surprising autonomy over the constitution of their own discipline. This was accomplished, as will become evident below, through graduate training.

In all fairness, already when the Gilliland report was published in 1962 mathematicians met it with reservations. In 1963 the Conference Board convened a special conference to discuss the implications of the report’s recommendations for the mathematical community.³⁰ Those in attendance were skeptical that the figures specified in the report could be reached, and expressed

²⁸ Saunders MacLane, “The Future Role of the Federal Government in Mathematics,” *The American Mathematical Monthly* 74, no. 1 (1967): 95.

²⁹ MacLane himself claimed that this outsider status of mathematics could, at least in part, be tracked back to World War II. “The scientific establishment was first forged during the Second World War under the fervor of work on important classified applied problems. When the groups to carry out this work were set up, about 1941, those who were responsible simply didn’t know who the mathematicians were and even had sadly mistaken notions as to who the good mathematicians were.” Ibid.

³⁰ Conference on Manpower Problem in the Training of Mathematics, CBMS, Washington DC, 1963.

concern that the desired expansion would lower the standards of doctorates in the field. Tukey, who was one of the PSAC members behind the report, was also in attendance and assumed the role of easing attendees' worries. Tukey asserted that there was not any compelling explanation for why such a small percentage of mathematics BAs continued on to pursue a PhD. "As an ex-chemist, with a master degree in chemistry, who has spent many years in a mathematics department, I am not convinced that mathematics undergraduates are dumber than chemists."³¹ For Tukey the math was simple. If each research faculty would supervise one successful student per year, the goals for mathematics described in the report would be easily obtained.

Tukey was in a unique position among those in attendance. As a member of PSAC, he was well acquainted with the report and the national policy considerations that prompted it, and as an active member of Princeton's Department of Mathematics, he was also closely familiar with the mathematical establishment. During his remarks, Tukey acknowledged that mathematicians might have to admit more flexibility into their doctoral program, but, he assured his listeners, "we will *not* have to lower general standards, and we will *not* have to change our standards for Ph.D.'s in the usual branches of 'pure' mathematics."³² Still, he added that mathematicians would have to reconceive the mathematical doctorate in one meaningful way. "We will need to realize," he implored those in attendance, "that mathematics Ph.D.'s are not today being trained exclusively to make people who can train more mathematics Ph.D.'s. The 'reflex ratio' for mathematics Ph.D.'s -- the fraction returning to higher education, is already down about 60%, and it will go lower."³³ In the late 1950s the "reflex ratio" was indeed beginning to decrease, and Tukey's remarks make it clear that it was assumed that this trend

³¹ John Tukey to Leon Cohen, 24 June 1963, JWTP, Box 5, Folder "Brown." Tukey attached a copy of his remarks during the conference to the letter.

³² Ibid.

³³ Ibid.

would continue.³⁴ However, the increase in federal funding to mathematics through the sort of recommendations set forth in the Gilliland report had exactly the opposite effect. Instead of further decreasing the reflex ratio, it actually increased it to approximately 80%. The overall expansion in higher education implied that the number of mathematicians who sought non-academic employment actually *declined* during the decade. Yet, compared with the situation in other sciences, the question still remained why did the overwhelming majority of mathematicians chose an academic career? What can account for this trend? In the early 1970s, mathematicians began asking these same questions.

In 1971, Gail Young, who just completed his term as president of the Association, published an article entitled, “The Problem of Employment in Mathematical Sciences,” in the *Notices*. Young had extensive knowledge of the mathematical community. Between 1965 and 1967 he served as the chairman of the Conference Board’s Survey Committee, which provided the major statistical data for COSRIMS. Therefore, most of his article gave an account for the mistaken predictions that appeared in the 1968 report. Yet, in tracing the causes for the gloomy employment prospects, Young reached to the early 1960s. “One source of our troubles,” Young wrote, was the push for more PhDs as set in the Gilliland report. “Re-reading the report now,” he added, “I wonder why the statements of need seemed so convincing at the time. Perhaps it was because they fit into a post-Sputnik mood of a new era for science, signaled by the founding of

³⁴ As part of his remarks, Tukey noted, “for my own part, I firmly believe that there are going to be more kinds of channels to the Ph.D. for people in mathematics. Whether or not these are called ‘mathematics’ is unimportant. Those whose main concern is with the growth of modern pure mathematics -- practical for its own sake -- will, I am convinced, come to the same conclusion, once they understand the consequences of having, or not having, such channels.” In a sense, Tukey was exactly right. During the 1960s there were more channels of PhDs in mathematics through the establishment of computer science and statistics departments. Yet, as he partially predicted, these tracks were not called “mathematics.” This implied that departments of “mathematics,” in which much of the expansion took place, were still dominated by pure mathematics.

NSF.”³⁵ According to Young, the increase in federal support had the unfortunate effect of inducing many universities to expand their graduate programs in the hopes of gaining federal funds, without the necessary long-term planning behind them.

Going back to the Gilliland report, Young conceded that its “estimates of need for mathematicians was a prediction of greater industrial and governmental employment of mathematicians.” However, he quickly noted that while there was an increase in absolute numbers, the percentage of PhDs who obtained industrial jobs was very low during the previous decade, at approximately 15%.³⁶ “Ph.D. programs in the mathematical sciences,” he explained, “were and are focused on the academic market.”³⁷ Graduate education in mathematics, that is, was by design aimed at academic employment and students who joined a doctoral program in the field were rarely encouraged to search for non-academic jobs. As long as existing departments of mathematics continued to expand and new ones were being established the number of available academic positions continued to grow.³⁸ Throughout the 1960s, the AMS published the annual salary survey in the *Notices*. They included the average salary across various ranks and different types of institutions, as well as information about the employment placement of recent PhDs. For example, the 1966 survey reported, “the academic life attracted the largest proportion of new Ph.D.’s in mathematics, 83% of the total reporting.” Noting the small percentage of graduates who went into industry, the report added, “even with its significant higher salaries, it [industry]

³⁵ Gail S. Young, “The Problem of Employment in Mathematical Sciences,” *Notices of the AMS* 18 (1971): 720.

³⁶ Reflecting on the low percentage of mathematics PhDs who went into industry, Young added, “whether a deliberate effort to train Ph.D.’s for industrial positions would have been proper or not, no large effort was made.” It is not clear if Young was completely unaware of the various attempts by applied mathematicians and SIAM to do just that, or whether he just did not consider these efforts effective since they were not made by the Society or the Association. In any case, his statement is indicative of the gap between academic and industrial mathematicians.

³⁷ *Ibid.*, 721.

³⁸ John William Jewett et al., *Report: Aspects of Graduate Training in the Mathematical Sciences*, vol. II, Conference Board of the Mathematical Sciences: Report of the Survey Committee (Washington, D.C.: Conference Board of Mathematical Sciences, 1969).

managed to attract 8.4% of those reporting.”³⁹ The fact that most doctorates chose to continue in academia despite the seductive salaries provided in industry was presented as a point for pride. This was not, after all, the goal of mathematical training.

Richard Anderson, a mathematician at Louisiana State University, provided a similar assessment of the situation. Together with Duren and Young, Anderson served on the AMS’s Committee on Employment and Educational Policy. Throughout the early 1970s, he published a series of articles providing up-to-date detailed statistical analysis on employment trends in the field, according to area of specialization and employment. For most mathematicians, Anderson’s articles that appeared twice a year in the *Notices* provided the best and most accurate assessment of the employment situation in the field. The first of such studies entitled, “Are There Too Many Ph.D.’s?” appeared in 1970.⁴⁰ In addition to statistical information it also included some possible explanations:

Over the last 25 years, at least, the principle thrust of graduate training in the mathematical sciences has been that of training in core mathematics, chiefly pure mathematics. We have trained Ph.D.’s in our own image, to regard research as the principle purpose of mathematicians and the primary (and almost only) route to real status in the profession. In a sense we have been fabulously successful.⁴¹

It was the production of academic researcher mathematicians, not industrial or governmental employees, that defined training in the field. Anderson’s confession, “We have trained Ph.D.’s in our own image,” also points to the mechanism by which this trend persisted. Despite Tukey’s remark in 1962 that mathematicians must recognize that today’s doctorates are not “being trained

³⁹ “The Annual Salary Survey,” *Notices of the AMS* 13 No. 6 (October 1966), 700.

⁴⁰ Anderson published his first statistical survey in the *American Mathematical Monthly*, since it arose out of a talk he gave during a meeting of the Association. After that he began publishing in the *Notices*.

⁴¹ Richard D. Anderson, “Are There too Many Ph.D.’s?,” *The American Mathematical Monthly* 77, no. 6 (1970): 628.

exclusively to make people who can train more mathematics Ph.D.'s," this was exactly what mathematicians did. The presumed prescription for a young mathematics PhD was simple – obtain an academic position, publish research papers, gain status, and supervise new doctoral students.

Yet Anderson's remark highlights the fact that it was not just the emphasis on academic research that characterized the growth of the field, but more specifically research in core mathematics. While the Gilliland report calling for expansion in mathematics PhDs was based on a specific vision of economic and technological growth, the training most mathematicians received placed them in a uniquely unfavorable position to take part in these developments. The demand was clearly for doctorates in the applied mathematical sciences, or at least for core mathematicians with a flexibility of interests, but by and large mathematics departments around the country continued to produce core or pure mathematicians. Anderson, in fact, began his article, echoing COSRIMS, noting that mathematics in the article "or more properly, the mathematical sciences, will be classified using four components: *core mathematics*, *applied mathematics*, *computer science* and *statistics*." However, he then quickly added, "by and large, core mathematics is that mathematics done in mathematics departments *per se*. It has been principally pure mathematics but has had some overlap with the other components, particularly applied mathematics."⁴² The emphasis on training in core mathematics was so predominant that by 1970 Anderson was able to define core mathematics not as an umbrella term including algebra, geometry and number theory, but as that mathematics which is pursued in mathematics departments *per se*.

⁴² Ibid., 627.

In 1971, R. Creighton Buck circulated a discussion paper among members of the newly appointed AMS committee in charge of studying the mathematical community's relation to the government. In trying to analyze the possible arguments mathematicians could make when asking for federal support, Buck began by examining how the case had been made in the past. "The argument for mathematics that is based on *applicability* is a very strong argument...One may stress the philosophical viewpoint of Wigner, and one may cite the historical examples – Maxwell, Turing, Hamilton, etc."⁴³ This, Buck noted, was the sort of argument presented in COSRIMS. Such reasoning, Buck added, has "many pitfalls." A critic, for example, may say that until all existing theories in pure mathematics are shown to have application, research in the field should be halted. More realistically, a critic can argue that the small percentage of mathematicians who devote their research to application does not justify the current funding for the field. Finally, Buck asserted, "it seems hypocritical" to sell mathematics via its applicability "unless we are will[ing] to give the matter full justice in training programs for mathematicians." Of course, that was what the mathematical community was doing all along. The hypocrisy was not that mathematics was not applicable; rather it was that on the whole this fact was not part of mathematical training. Only when the employment crisis deepened was applied mathematics finally placed at the center of mathematical education.

In 1970 demand for mathematicians was no longer defined via an appeal to economic and technological progress as it had been in the Gilliland report. Instead a new model for demand based on the academic job market came to replace it. It turned out that it too raised concerns as far as academic training was concerned. The standard reference point for mathematicians became an article entitled, "Scientific Manpower for 1970-1985," by economist and chancellor of New

⁴³ R. C. Buck, "Relations With the Government," 5 December 1971, SMP, Box 6.4/AAM 86-10/4, Folder "AMS: Government Relations, Committee On – Part Two."

York University Allan M. Cartter.⁴⁴ In his article, which appeared in *Science* in 1971, Cartter argued that the current employment problems could not be attributed completely to cuts in federal funds. The current financial crisis, Cartter claimed, only expedited the process but did not entirely cause it. An analysis based on birth rate, the expansion of higher education, and college enrollment, he argued, gave a more accurate estimate for scientific demand. According to Cartter, universities were producing more PhDs than they would be able to employ. Cartter noted that any analysis is prone to failure since even slight changes in curricula or unpredicted intellectual trends can have major effects on the prediction of aggregated numbers. More revealing, as far as mathematicians were concerned, was Cartter's assertion that "many of the science fields are also more complicated than, say, English, history, or anthropology, for in the latter, over 90 percent of the doctoral degree recipients enter college teaching directly upon completing their degree work."⁴⁵ Since employment trends in mathematics, unlike in physics, were closer to the ones in history, Cartter's analysis was especially relevant to mathematicians.

The AMS's Committee on Employment and Educational Policy studied previous models such as the one by Cartter only to find that "they are rather more optimistic for mathematics than we are."⁴⁶ Up until 1972, there was still a backlog of unfilled college teaching positions, but many of these jobs were in four-year colleges that did not have doctoral programs in mathematics. These were positions with a clear emphasis on undergraduate teaching, not only for majors but also as service courses for other departments. Mathematical research was not really a necessary component of the job description. As it became clear that most academic positions would be based on teaching rather than on research, new concerns about the PhD surfaced.

⁴⁴ Allan M. Cartter, "Scientific Manpower for 1970-1985," *Science* 172, no. 3979 (1971): 132-140. Also, see: Dael Wolfle and Charles V. Kidd, "The Future Market for Ph. D.'s," *Science* 173, no. 3999 (1971): 784-793.

⁴⁵ Cartter, "Scientific Manpower for 1970-1985," 134.

⁴⁶ William L. Duren, "Employment and Educational Policy in Mathematics," 4.

One of the earliest mathematicians to call the mathematical community's attention to the problem with the PhD was Chicago algebraist Israel N. Herstein. "It is time," Herstein wrote in an article in 1969, "that we question the objectives of our Ph.D. in mathematics and that we try to attenuate the intensity of this 'research only' spirit which permeates it."⁴⁷ Herstein explained that while only 25% of doctoral students continue to do research in mathematics, the remaining 75% receive the same training, which is "oriented to the production of research scholars," even though they would spend most of their career teaching. According to Herstein, the emphasis on research that students received during their graduate studies meant that they lacked a broad overview of the field that was necessary for teaching. "[T]oo often they have narrowed or have been narrowed in order to learn about some small slice of mathematics so as to be able to write something original."⁴⁸ When these students begin to teach, Herstein argued, they often know little more than their students about certain topics. The emphasis on academic research in core mathematics was not only detrimental to mathematicians' effectiveness in non-academic domains but also in the much more available teaching positions.

What else becomes evident in Herstein's remark is the emphasis on individual work that characterized almost all training in pure mathematics. Unlike in physics, chemistry, or biology, training in the field was not centered around a shared project, laboratory, or experimental apparatus. Most students were assigned their thesis problem by their advisor and pursued it in isolation. The research was not done in collaboration and there was not necessarily any reason that two students of the same advisor would be working on similar topic. In that restricted sense, graduate work in mathematics was still closer to one in history and English than to molecular biology. At the end of the day, what a student required to complete his or her research was a

⁴⁷ Israel N Herstein, "On the Ph. D. in Mathematics," *The American Mathematical Monthly* 76, no. 7 (1969), 821.

⁴⁸ *Ibid*, 819.

library. This is further emphasized in Herstein's proclamation that training in the field was not directed toward the production of "researchers" or "research scientists," but rather "research scholars."

Herstein's critique, therefore, had another component. Even worse than the lack of breadth, he argued, was that after being repeatedly told that being "a mathematician is synonymous with being a research mathematician" and that "anything less is failure," these young mathematicians are now left with a sense of guilt.⁴⁹ Pursuing a teaching career, he noted, was deemed equivalent to failing in one's chosen profession.⁵⁰ Adding a personal note, Herstein remarked, "I am proud of my own students who have gone on to become first-rate research mathematicians. But I also take a deep sense of pride in my other students who have gone on to become excellent teachers and worthy members of their departments and faculties."⁵¹ In articulating the social pressure placed on young PhDs, Herstein pointed to one of the truisms of the discipline; namely that to be a mathematician was to be an academic research mathematician (and preferably in a top-tier university).⁵²

⁴⁹ Ibid, 820.

⁵⁰ In his 1973 discussion paper to the Committee on Employment and Educational Policy, Duren also talks about the inability to obtain or maintain an academic position in terms of failure. Duren notes that due to budget constraints, many universities will undoubtedly terminate the appointment of "unproductive assistant professors," who will therefore "go far down the scale of mathematical working conditions to find any employment." This will not be to detrimental to the mathematical research, but will rather be more of a "human problem." What the AMS could do in these cases, Duren suggested, "was to ease the *sting of failure* and send them out proudly, still mathematicians and still members of the Society." Not only was the move from academia into industry seen as a failure, but the individual involved would also get stripped of his/her "mathematician" rank.

⁵¹ Herstein, "On the Ph. D. in Mathematics," 821.

⁵² This sentiment was openly acknowledged. In February 1969, Alex Rosenberg, a Cornell mathematician, wrote a worried letter to Oskar Zariski, who at the time was the president of the AMS:

It is my personal impression, and one that I think is shared by many people, that the recent Ph.D.'s in Mathematics will find it very difficult to get positions that they regard as suitable. On the other hand I have a fairly thick folder of letters from very undistinguished institutions looking for mathematics teachers with Ph.D.'s. It seems to me, therefore, that what is bound to happen is that many recent Ph.D.'s will end up at institutions like Slippery Rock State Teachers College or the College of St. Mary of the Woods. Surely in many cases this will be very beneficial to the state of mathematics in this country. On the other hand it will almost certainly mean, unless there is a drastic change in present day conditions, that a good deal of mathematical talent for research will be wasted.

This sentiment was not merely a sort of view from above, but was in fact part of the lived experience of many young mathematicians during the period. In October 1973, George Poynor wrote an angry letter to Henry Alder, the secretary of the Association. In his letter, Poynor informed Alder that given its current policies he could no longer continue with clear conscience his membership in the Association. At a time of much political upheaval, Poynor's disenchantment with the Association did not result from its political or social policies (such letters were common in the Society and the Association); rather Poynor's uneasiness was due to the Association's promotion of research as the ultimate goal of all mathematical education.

I received my graduate mathematical training and indoctrination during the latest Golden Age of mathematics (1950-1964). I dutifully absorbed and believed the cant which was my daily provender during that time. And I went on to teach it at a Midwestern university.

With each ensuing year my dissatisfaction grew: with our concentration on mathematical esoterica; with our overweening elitist pride; and with our attempt to turn the meanest college mathematical department into a hotbed of research. So finally I left academe.⁵³

Poynor reported that he went on to secure a governmental position where he found more personal satisfaction. Poynor, it is worth noting, was not educated in one of the top mathematical departments in the country. Rather, he obtained his PhD from Louisiana State University in 1964. Poynor was not a celebrated Cornell algebraist. The research-only attitude, it seems, prevailed among the entire community.

Poynor's decision to withdraw his membership from the Association, he explained, was due to his feeling that nothing was being done on the professional organizational level to try to

Four years later even the teaching position in these "undistinguished" institutions would be hard to come by. Still, Rosenberg's concern emphasizes that the doctorate degree in mathematics was considered above all a research degree. Alex Rosenberg to Oskar Zariski, 5 February 1969, AMSR, Box 60, Folder 36.

⁵³ George Poynor to Henry Alder, 10 October 1973, SMP, Box 86-10, Folder "AMS: Employment and Educational Policy, Committee on – General 1973-1975."

fight this sentiment. “I keep hoping to see enlightened leadership,” Poynor concluded his letter, “but all I see are theorems!!”⁵⁴ Five days after he received Poynor’s letter, Alder forward it to Anderson. “There are several such letters which I have recently received,” he informed Anderson.⁵⁵

The perceived class division that both Herstein and Poynor describe between academic research mathematicians and mathematical researchers in other domains was of course not a new phenomenon. As described in the previous chapters, it repeatedly came up during the 1950s and 1960s in arguments between applied and pure mathematicians about the relative merit of each research field. Rather, what the employment crisis did was to place this elitist conception of the field at the center of public discussion. As they began to debate about how to modify mathematical training, these tensions became especially clear. For at heart was what it meant *to be* a mathematician. Was an individual with a PhD in mathematics, working in a small liberal arts college teaching calculus to freshmen, who had not published a single mathematical paper beyond his/her dissertation still entitled to the title of a mathematician? What about one who was working in a big industrial laboratory or at an investment bank?

At play was not only the professional identity of the mathematician but also the mathematical persona. This elitist conception of the field was predicated on the persona of the mathematician as a creative scholar. When Warren Weaver and Marshal Stone were debating during World War II about the most effective way to incorporate mathematicians into war-related research, Weaver argued that many mathematicians did not possess the necessary personality characteristics to take part in such work. The creative mathematician, according to

⁵⁴ Ibid.

⁵⁵ Henry L. Alder to Richard D. Anderson, 15 October 1973, SMP, Box 86-10, Folder “AMS: Employment and Educational Policy, Committee on – General 1973-1975.”

Weaver, was an eccentric genius consumed more by the world he creates than the one he inhabits. Despite the tremendous growth of the field in the preceding three decades, elements of this characterization were still present. In 1972 *The New Yorker* published an article about mathematics and mathematicians entitled “Reflections: Mathematics and Creativity” by mathematician Alfred Adler. Adler’s goal was to give the readers of the magazine some insight into the nature of a mathematical work. However, many parts of the piece read more like an idealistic portrayal of mathematicians than of mathematics. Mathematics, Adler proclaimed, is “an art—the most intellectual and classical of the arts. And almost no one is capable of doing significant mathematics.”⁵⁶ Adler added that each generation sees only a small number of great mathematicians and that the work of the majority of mathematicians is of no importance. “There is never any doubt who is and who is not a creative mathematician,”⁵⁷ he asserted. Mathematics in 1972, as far as Adler was concerned, was still a field developed by geniuses.

Mathematicians, therefore, were always young, inherently skeptical, creative, and imaginative. They tended to have radical political positions (for the most part on the left), but were not interested in political power, material reward, or even recognition. They were the perfect explorers motivated by nothing than sheer curiosity. Yet in celebrating mathematicians, Adler announced, “mathematicians are often expected to manage brilliantly in the fields of business and finance. Of course, they do nothing of the kind. Their non-mathematical efforts are, on the whole, pitifully inept.” For Adler there was something inherent in mathematical work that just did not translate into other domains. “The qualities embedded in the mind of the mathematician by the discipline of mathematics fail to extend beyond the boundaries of

⁵⁶ Alfred Adler, “Reflections: Mathematics and Creativity,” *The New Yorker*, February 19, 1972, 39.

⁵⁷ *Ibid.*

mathematics.”⁵⁸ Mathematics was a scholarly activity and mathematicians were intellectuals. At the time when many young mathematicians were eagerly searching for a job, such a portrayal in the pages of *The New Yorker* was not enthusiastically welcomed.

Gordon Walker, the Executive Director of the AMS, pleaded with Nathan Jacobson, the president of the Society, to send a letter to the editors of the magazine rebutting some of the assertions made by Adler. Walker was worried that the article might only increase the hardship of young unemployed mathematicians. Jacobson, however, did not see fit to write an official reply to the article, and suggested that such an act would only draw further attention to the problematic elements of the essay.⁵⁹ John Jewett, the head of the Department of Mathematics at Oklahoma State University, who was copied on the correspondence, agreed with Jacobson that an official reply was not desirable, but decided to take it upon himself to send a letter to *The New Yorker* editors. Jewett asserted that Adler’s claim that “the mathematician is great or he is nothing” and that his non-mathematical efforts are for the most part inept, “are really expressions of an adolescent romanticism about mathematics.” He then added, “these romanticisms...are more typical of the generation of mathematicians raised during the adolescence of American mathematics in the thirties and forties than they are of the younger generation who have come to maturity more recently.”⁶⁰ Possibly, one of the most surprising aspects of the development of the field in the postwar period was the maintenance, at least to some degree, of this romantic vision of the field. Adler’s description might have read like a caricature but to say that elements of his

⁵⁸ Ibid, 41.

⁵⁹ Jacobson concluded his letter to Walker, noting, “I should add that while I had and still have quite a few reservations on the content of the article, after re-reading it and talking to some non-mathematicians who read the article, I have come to the conclusion that in spite its faults, its effect on the general intelligent public will not be wholly bad. Its glamorization of mathematical research and of the satisfaction which comes from achievement in this activity may in fact enhance the standing of mathematicians generally.” Nathan Jacobson to Gordon Walker, 15 March 1972, AMSR, Box 67, Folder 12.

⁶⁰ John Jewett to Editors The New Yorker Magazine, 21 March 1972, AMSR, Box 66, Folder 70.

portrayal completely vanished with the generation of the thirties and the forties would be a mistake. After all this was exactly the “research only” sentiment that prevailed in most training programs in mathematics, and who was it if not the prewar generation that designed these programs.

Throughout the postwar period the applied and pure mathematician were defined in opposition to one another. There was not really one image of the mathematician. There were two. Henry Alder also wrote to Walker with regard to Adler’s article. “I believe,” he wrote, “that by and large it is a surprisingly accurate description of *some* of the aspects of a mathematician.” On the whole, he explained, he in fact enjoyed the article quite a lot and thought that mathematicians should read it. What bothered him was something else. Namely, “that the positive contributions of mathematicians are never described to the public; only those which tend to give the rather one-sided image of the mathematician, as is presented in the *New Yorker* article.”⁶¹

Adler’s article can be viewed as marking the point of change. Jewett argued that Adler’s description was a more accurate portrayal of the prewar generation than the one that came after it. However, his assessment was only partially true. The postwar period did not witness the disappearance of the mathematician-as-scholar; rather it saw the coming into existence of a competing image of the applied mathematician, who was still separate and distinct. As Alder wrote to Walker, this is a “surprisingly accurate description of *some* of the aspects of a mathematician.” Only after the 1970s did these two personas begin to merge. The employment crisis played a crucial role in that respect.⁶²

⁶¹ Henry Alder to Gordon Walker, 13 March 1972, AMSR, Box 68, Folder 23.

⁶² This of course is not to suggest that a romantic conception of the mathematician is not still with us today. Especially in popular culture, the mathematical “genius” is often presented as somewhat detached from the world. The most recent example of this was the extensive coverage of Grigori Perelman, who in 2003 proved the famous Poincaré conjecture. That Perelman refused the one million dollar prize that was attached to the problem and declined to accept the Fields Medal only reaffirmed this romantic vision of the mathematician.

Finding Solutions – Rethinking Training

In 1971, the Committee on Employment and Educational Policy published a statement on the short-term and long-term employment prospects of mathematics doctorates in the *Notices*. The mathematical profession, the report announced, was going through a depression. “Many of our young people have excess expectations because they have never known anything but fantastic boom. The sooner that we all face the reality that the boom has ended, the sooner that we will recover from its temporary psychological effects and get on with our lives.”⁶³ The committee repeatedly emphasized that the mathematical community must become aware of this new reality. It added, “other things now occupy the TV spotlight and will get the dollars and the attention. We had our turn.”⁶⁴ In stressing the matter, the committee hoped to convey that the situation was not likely to change any time soon and that no major change in policy was expected or likely to improve the employment prospects of young mathematicians. Imploring mathematicians to come to terms with these transformations was a call to arms. If the federal government was not going to swoop in and offer a solution to the looming employment problem, then it was up to mathematicians to do it. The question was -- what could be done?

⁶³ “Statement on Employment of Ph.D.’s in Mathematics,” *Notices of the AMS* 18 (1971), 487.

⁶⁴ In fact only a few month earlier, the *Notices* published a short article informing the mathematical community about the proposed NSF budget for 1972, where it became clear that the appropriation to mathematics from the budget has been steadily decreasing. Whereas the projected increase in the NSF budget stood at 23%, the budget for mathematics was to increase only by 18.7%. Mathematics, the article noted was the only field for which the percentage increase was in fact lower than the overall budget increase. “Of concern particularly to mathematicians is the statement of the Administration that the most pressing need now ‘is the application of what we already now.’ As mathematicians are more concerned with what is not known, but should be, than with what is already known, this philosophy could be extremely detrimental to the future of mathematical research.” “National Science Foundation Budget for 1972” *Notices of the AMS* 18 (1971), 352-353.

It was clear that academic research positions were drying up. In a perfect mathematical fashion, Joseph LaSalle provided a complete analytic model of the effect of tenure and promotion on the job prospects of young mathematicians in the mathematics department. The model, published in 1972 in the *Notices*, included several variables, numerous equations, and a number of diagrams (see Figure 1). LaSalle wished to demonstrate that current promotion and hiring practices were especially disadvantageous for young mathematicians. Continuing with current practices, LaSalle maintained, would jeopardize the crucial balance of young and old faculty in mathematics departments. Anderson also began providing a year-to-year analysis of job prospects, which he presented in neat flow diagrams. Anderson's studies determined the effects of retirements, promotions, and deaths, on the prospect of young PhDs (see Figure 2). But, if academic positions did not exist at present and were unlikely to become available in the future, than what types of positions were mathematics PhDs supposed to obtain?

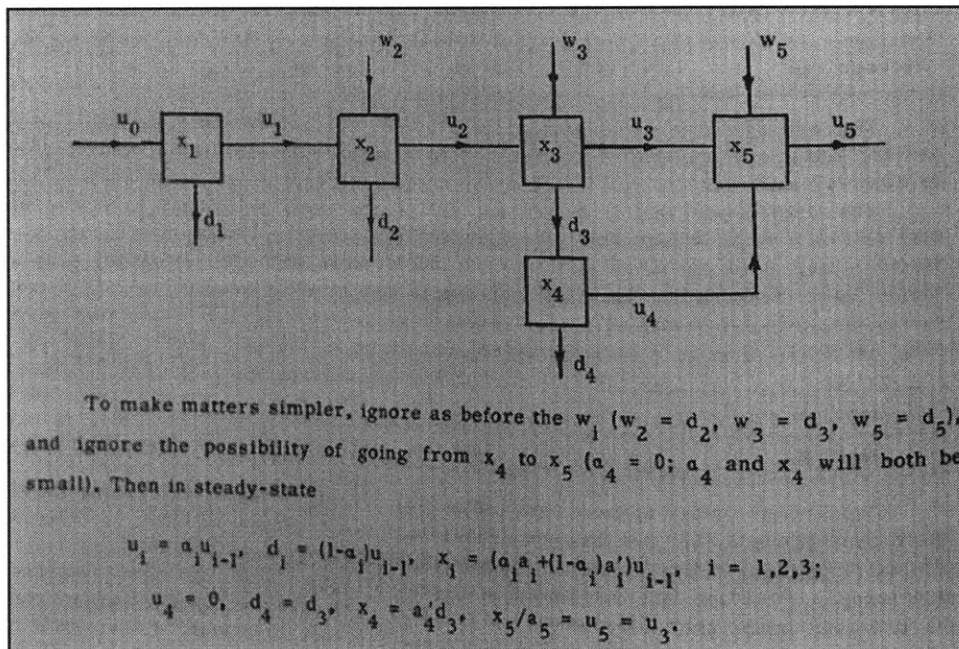


Figure 1. A diagram from Joseph P. LaSalle's "Appointments, Promotions, and Tenure Under Steady-State Staffing." The variables correspond to various aspects of a standard tenure procedure. For example, x_1 denotes the number of appointments of assistant professors and other untenured faculty, and u_1 denoted the average number of promotion of this type. LaSalle constructed a model showing how changes in promotion and hiring practices would affect the job prospects of new faculty. *Notices of the AMS* 19 (1972), 69-73.

In diagnosing the problem of overproduction, mathematicians were quick to point to what they recognized as the research-only sentiment that dominated mathematical education. Thus, in considering how to improve the current job prospects of mathematics PhDs, they inevitably turned their attention to the doctorate programs. The question of what could be done to improve the job prospects of mathematicians was quickly translated into how educational policies should be changed in response to the job market. Over the next few years, national meetings included panel discussions on topics such as “Graduate Education in Mathematics in the Coming Decade,” “Non-Academic Employment of Ph.D.’s,” “The Role of the Dissertation in the Ph.D. Program,” and “The Role of the Ph.D. in Two-Year College Teaching.” Mathematical publications were inundated with letters to the editors as well as long form proposals outlining how doctorate-training programs in mathematics should be reconfigured. As is evident from the titles above, mathematicians focused on industrial employment and teaching positions.

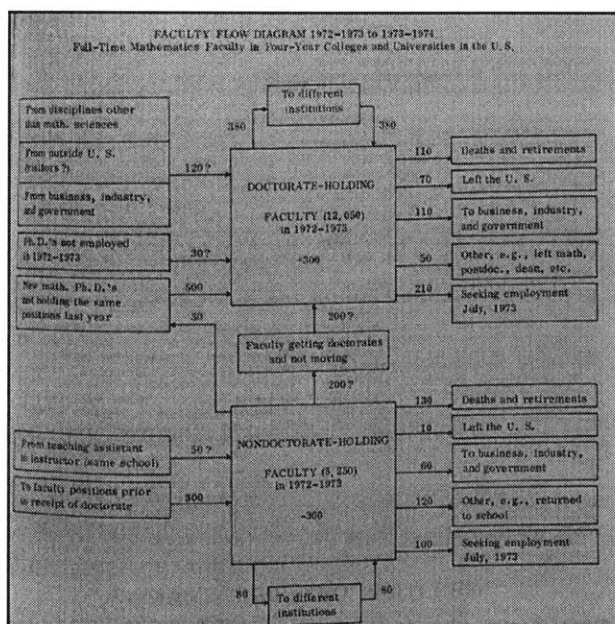


Figure 2. Anderson’s flow chart 1962-1973 to 1973-1974 of full-time faculty in four-year colleges and universities in the United States. The chart indicates the number of new positions created by retirement, death, change of employment, as well as the number of incoming faculty. Anderson began publishing this flow chart in his yearly of statistics about the profession, which also included a breakdown of PhDs’ first employment by specialization. *Notices of the AMS* 20 (1973), 351.

One of the most common suggestions was that graduate students would supplement their courses in core mathematics with ones in applied mathematics or some other scientific field. Almost all mathematicians agreed that this should be an available option for students. What was more controversial was whether it should become a *required* component of the PhD degree. Edwin Moise, a Harvard mathematician and an expert on mathematical education, published a long essay in the *Notices* entitled “Jobs, Training, and Education for Mathematicians.” Noting that a growing complaint about graduate programs in mathematics was that they neglected application, Moise suggested that the requirement for a PhD. “should include a one-year course dealing with some sort of application of mathematics... Such a course would contribute toward competence in industrial mathematics. And it will contribute a dimension to the student’s intellectual sophistication.”⁶⁵ Many contemporaries repeated this sentiment with varying degrees of emphasis.⁶⁶ Requiring graduate students to take courses in applied mathematics was seen as way of broadening students’ employment prospects, since it was well acknowledged that doctorates in statistics, computing, and applied mathematics were having an easier time in the job market. Finally after three decades, training in applied mathematics was considered an advantage over pure mathematics education.

A year before Moise’s article appeared in print, the requirement for students to take courses in applied mathematics came up during a panel discussion on the future of graduate

⁶⁵ Moise made it clear that such course should be required only if their scholarly level was as high as those in core mathematics. Edwin Moise, “Jobs, Training, and Education For Mathematicians,” *Notices of the American Mathematical Society* 20 (August 1973): 217–221.

⁶⁶ For example, in a 1971 letter to the editors of the *Notices*, Robert Hermann of Rockefeller University declared that the present situation made it clear that it was “not wise to encourage the complete separation between mathematics and its applications in the sciences.” Hermann argued that one way to amend the situation was to make sure that students in pure mathematics were familiar with other fields of science. These were to be “cultural courses that will introduce students to the uses of modern mathematics in various scientific fields. A more radical approach was suggested a year later by Dale W. Lick in a manifesto entitled “New Direction and Commitments for Mathematics.” Lick argued that traditional departments of mathematics needed to be closed and new departments centered on the application of mathematics be opened instead. New fields of application needed to be developed and new alliances with scientists formed.

education in mathematics. Peter Duren, a professor of mathematics from the University of Michigan, was invited to the panel to discuss a recent study conducted by his department. By surveying the past performance of graduates of the department and their current academic employment, the Michigan study made several suggestions as to how its graduate program should be altered to fit the current employment situation. Duren reported that one of the suggestions proposed by the committee was that the doctoral requirement would include “some serious exposure to applied mathematics. Not just ‘applicable mathematics,’ but some honest experience with the construction of mathematical models.”⁶⁷ Every PhD, Duren added, should have some knowledge of mechanics and familiarity with computing. As far as the department of mathematics at Michigan was concerned, every mathematics PhD needed to have some training outside of pure mathematics.

Eight years earlier during the height of the boom years, a joint committee of the Society and the Association charged with studying the graduate programs in mathematics had reached the exact opposite conclusion. “Although the Committee recognizes the importance of establishing connections between mathematics and related fields,” the final recommendations read, “it does not feel that this can be accomplished by a rigid requirement of minors in a field outside of mathematics for the Ph.D. program.”⁶⁸ Despite warning calls that mathematicians were pulling further away from the rest of the sciences, during the 1960s requiring graduate students to supplement their studies in core mathematics was just not judged to be a priority.

In fact, even during the 1970s there were those who warned against an overemphasis on application. Interestingly enough it was the representative graduate student on the panel, Robert

⁶⁷ “Graduate Education in Mathematics in the Coming Decade,” *Notices of the American Mathematical Society* 20, no. 3 (April 1973): 123–130.

⁶⁸ Charles E. Rickart to Henry L. Alder and John W. Green, 23 March 1964, RCP, Box 23 (new), Folder 3.

Jamison, who declared that “It would be very sad if, in an attempt to appease the gods for the present job crisis, we try to make our mathematics so ‘applied’ that we sacrifice the humane side of our subject, or if, in a rush for ‘relevance,’ we interpret that word to mean ‘economically useful’ with little allowance for the relevance of basic humane curiosity.” Jamison agreed that graduate education should include additional training in applied mathematics, but he emphasized that the “humanistic” sides of the field should not be neglected. Concluding, he remarked, “Why else were washing machines and automobiles invented if not to give man more time to admire the wonders of prime numbers?”⁶⁹

Mathematicians were not the only ones who began warning against the tendency of overspecialization when federal funds began to decrease and the employment prospects dwindle. Physicists, who felt the transformation most acutely, also began calling for a broadening of graduate education. Whereas physicists singled out particle theorists specifically for their lack of breadth, for mathematicians the problem was with training in pure mathematics on the whole (and as such with graduate programs in almost all departments of mathematics).⁷⁰ It was not that mathematicians thought that students who specialized in abstract algebra should take more classes in topology or number theory. Rather the call was for mathematicians to learn a completely different set of skills and approach mathematical theories not only as abstract entities but also as embedded in the world. It was one thing to require a student to become an expert in finite group theory, to master the literature in the field and possibly prove an open conjecture. It

⁶⁹ Ibid.

⁷⁰ Another important difference between mathematicians and physicists was that the growth in particle theorists in the aftermath of the war corresponded directly to a demand for high-energy physicists and the development of new experimental apparatus. The growth in pure (core) mathematics did not rise in direct correlation, as mathematicians began to realize in the 1970s, to any explicit demand. The overspecialization of mathematicians is, therefore, even more puzzling and reveals more about the overall growth of the field than about a specific specialization. For physicists’ response to overspecialization and its effect on the intellectual growth of the field, see: David Kaiser, “Whose Mass Is It Anyway? Particle Cosmology and the Objects of Theory,” *Social Studies of Science* 36, no. 4 (2006): 533–564.

was yet another thing to ask him or her to construct a mathematical model of some physical phenomena. These were two different approaches that only grew apart in the decades after the war. In its most radical form, therefore, the call for altering the doctoral requirement to include science and applied mathematics can be conceived of as a proposal to completely restructure the field.

The Committee on Employment and Educational Policy also began a project offering short courses during annual society meeting introducing young mathematicians to various mathematical applications. Thus, for example, the 1974 meeting was to include courses on biomedical mathematics (offered by Richard Bellman), complexity and computation (offered by Peter Lax), operation research; control theory, urban or traffic problems; and category theory (offered by Saunders MacLane). The idea behind these courses was to familiarize young mathematicians with the various areas in which they might be able to apply their skills and hence improve their job prospects. In addition, starting in 1974 the *Notices* began soliciting and publishing “case studies” of mathematicians with non-academic employment. These case studies consisted of short testimonials from mathematics PhDs who, either immediately upon or shortly after obtaining their degree, were employed by industry. Introducing the new column, the editors of the *Notices* explained that “it is hoped that this will not only give mathematicians in general some idea of what they can do outside of academia, but will also suggest how to go about obtaining such a position.”⁷¹ The first column included stories by three mathematicians employed at Bell Labs, Singer-Kearfott Aerospace and Marine Division, and at the Transportation and Urban Analysis Department at GM. All three authors described their academic background, the

⁷¹ “Case Studies: Some Mathematicians with Nonacademic Employment,” *Notices of the American Mathematical Society* 21, no. 157 (November 1974): 346–348.

type of courses they took, the dissertation they wrote, the type of work they encountered in their profession, the level of mathematics required, and the way they obtained the job.⁷²

While these measures were taken in order to help young mathematicians searching for a job, the Committee on Employment and Educational Policy was thinking about unemployment as a long-term problem. In 1973, the Committee drafted a preliminary proposal to the NSF for “an in-depth study of prospective of business, industry, and government mathematical manpower.”⁷³ The proposed study was to take place over a period of three years, and was to include an investigation of the possible non-academic employment for mathematicians in traditional Research and Development industries, but also in less traditional businesses. The proposal indicated that it would also include a re-examination on a national level of the graduate training program in mathematics focusing on the dissertation requirement and the graduate curriculum. The goal was to learn how training in the field should be altered to meet present day employment prospects. For decades the AMS maintained its position as the official

⁷² In deliberating about the future column, Gordon Walker, who was the editor of the *Notices*, wrote to Everett Pitcher, the secretary of the AMS, “I suggested that it would be better to have a substantial number of individual cases (perhaps 20 per year). And they should predominantly concerned with little people.” Assuming that by “little people” Walker meant ordinary PhD mathematicians, then the goal of publishing these case studies was to help young mathematicians imagine some professional career besides teaching or research. Yet Walker’s comment points to the fact that when making the case for the place of mathematicians in industry, mathematicians often did so by noting the important contribution of some outstanding mathematicians such as Von Neumann and not by more broadly emphasizing their place in the industrial workplace. This was evident, for example, during a national panel on Nonacademic Employment of PhDs that took place in January 16, 1974. One of the speakers at the panel was former Presidential Science Advisor Edward E. David. David began his talk by noting, “neither mathematicians nor industry have any great affinity to each other.” However, in making the case that there was no doubt that mathematicians had been tremendously effective in industry in the past, David referred to the work of Von Neumann, Claude Shannon, Norbert Wiener and Hendrik Bode. None of these men could be described as an “ordinary” mathematician. Gordon Walker to Everett Pitcher, 2 November 1973, SMP, Box 86-10, Folder “AMS: Employment and Educational Policy, Committee on – General 1973-1975.”; “Nonacademic Employment of Ph.D.’s,” *Notices of the AMS* 21 No. 4 (August 1974): 206.

⁷³ The proposal is included under the Minutes of the Committee on Employment and Educational Policy, 19 August 1973, Missoula, Montana. It is noted in the minutes that while a representative of the NSF said that a proposal such as this once could be submitted it must comply with certain guidelines the committee found discouraging. It was suggested during the meeting that the committee would begin approaching private foundations for support instead. “Preliminary Proposal submitted to the National Science Foundation,” SMP, Box 6.4/AAM 86-10/4, Folder “AMS: Employment and Educational Policy, Committee On – General 1973-1975.”

representative of academic research mathematicians. Through the 1950s it fought with vigor for federal support for pure mathematical research against what it conceived of as an overemphasis on applied research. It held, and prevailed, that mathematics and mathematicians should not be developed according to utilitarian considerations. In the 1960s, as the field was expanding and mathematical techniques were penetrating an ever-growing domains of knowledge, academic research mathematicians continued to maintain their autonomy by carefully controlling the boundaries of the field. It was only with the ensuing employment crisis that calls for overall reform were heard also from within the Society.

This is of course not to suggest that everyone necessarily agreed that the emphasis on research should be abandoned. Certainly, some immediate measures should be taken to help mathematicians find employment, but there was no reason to completely alter the character of the discipline. However, what is clear is that the 1970s forced many in the mathematical community to recognize that the emphasis on scholarly research could no longer *by itself* dominate the growth of the field. In describing the nature of the proposed study, the preliminary NSF proposal noted that traditionally research training “has been for the purpose of developing more researchers with ultimate academic employment in mind.”⁷⁴ However, since it was unlikely that the next twenty years would see a significant expansion in academic employment for mathematicians, there was now a genuine need to rethink the contemporary training in the field. The proposal noted, “indeed, there are some who believe that the nature of current research training is counter to wider-scale nonacademic employment of mathematicians since current research training may well orient the new Ph.D. toward ‘doing his own thing’ and not toward working on problems of society. The basic problem is to develop a pattern of graduate training in

⁷⁴ Ibid.

which research in mathematics can coexist and thrive while most doctorates are being trained for nonpersonal research job opportunities.”⁷⁵ How to maintain academic mathematical research while preparing most students for non-research positions became a source of tension for mathematicians.

A debate surrounding the role of the dissertation in graduate training illustrated this concern. Even more than requiring students to take courses in applied mathematics, whether or not the research dissertation should be altered became a source of tension and enabled mathematicians to voice their discomfort about the development of the field in the previous decades and their concern about its future. Possibly the most radical proposal was voiced by mathematician Calvin Moore, who was at the time the Dean of the Physical Sciences at the University of California Berkeley. During the 1972 panel discussion on the future of mathematical education, Moore declared, “we must effectively eliminate the Ph.D. degree as we know it today.” Moore argued that the current situation was such that the majority of graduate students “are receiving training which is at best irrelevant to their job opportunities and in many cases counterproductive.”⁷⁶ The main culprit, according to Moore, was the research dissertation, which he labeled as “irrelevant.” For most students a three-year program, which included mandatory work in applied mathematics and teaching experience, would be sufficient. The select few who would wish and would have the necessary qualifications to continue on in research, Moore explained, would be able to do so as part as their postgraduate career. He asserted that such an arrangement would in no way complicate the hiring practices of research departments,

⁷⁵ Ibid.

⁷⁶ “Graduate Education in Mathematics in the Coming Decade.”

because in any case hiring decisions are based on recommendation letters and not on one's dissertation.⁷⁷

A less extreme proposal had been brought forward by Herstein already in his 1969 article. The solution, according to him, was not to completely eliminate the dissertation but rather to broaden what counts as research to include more expository work. In 1974, another national panel was convened, this time to study the role of the dissertation in the PhD program. Herstein, who was one of the invited speakers, repeated his assertion that the dissertation did not serve the needs of most students, and that more alternative routes should be available for those students who were not planning to continue on in research. He suggested, for example, that instead of a research dissertation a student could develop a new junior or senior level course. The thesis, he explained, would be the text or extensive notes for the course. This type of dissertation would be much more appropriate for students who planned to pursue a teaching career. William Browder, a Princeton mathematician, who was also invited to speak on the panel, disagreed. The solution to the current overproduction and unemployment was not to lower the standard, but just the opposite to make the standards higher. More telling was Browder's assertion that "we owe a duty not only to our students and colleagues, but also to our subject, to further its development and progress."⁷⁸ The growth of mathematics, according to Browder, should not be dictated through considerations such as future employment but rather by the state of knowledge in the field.

⁷⁷ It is telling that Moore based much of his analysis on the growth in community colleges. Moore noted at the beginning of his talk that if one looked only at the state of California, the figures were staggering. The University of California and the state college system, according to him, enrolled approximately 350,000 students. While the community colleges enrolled closer to 700,000 students. Yet, community colleges, for the most part, were not hiring mathematics PhDs. In fact advanced graduate training in mathematics was seen as an inadequate preparation for such a teaching job. Two-year colleges were therefore singled out early on by members of the Society as a source of possible positions. A year later, in the annual meeting in 1973, a panel discussion was convened to study just the place of PhDs in two-year colleges.

⁷⁸ "The Role of the Dissertation in the Ph.D. Program," *No* 21, no. 154 (June 1974): 180–186.

Saunders MacLane, who also participated in the panel, concurred. He agreed with Herstein that the tendency toward overspecialization should be eliminated, but he asserted that standards for the dissertation should be increased. For MacLane the real question was whether in fact there were just too many theses. No one could necessarily point to a given PhD student and announce that he was extraneous, but on the whole there was just no real justification for the huge numbers of PhDs who were graduating each year. “No one can argue that the needs of mathematical research and discovery call for any such fantastic increase. The beauty of a mathematical result is not improved by multiple discoveries!”⁷⁹ Here again it was the *needs of mathematical research* that MacLane asserted should determine the development of the field. A few months earlier, MacLane published an editorial entitled, “Is This Doctoral Program Necessary?”⁸⁰ In it, MacLane posed a series of questions to mathematics departments that were supposed to help them determine whether their program should continue or be terminated. The list provides clues to how the needs of mathematical research were to be defined according to MacLane.

After a period of about ten or fifteen years, a graduate program should have some graduates who have done “outstanding work,” MacLane declared, only to then ask, “How many can you list for your program?” As far as the quality of the faculty was concerned, this was an easy matter to establish. MacLane explained, “Name the outstanding paper they have written and the reasons they are outstanding, and specify the national and international invited addresses given by members of the faculty.” Other questions included were “Does your program exist in part in order to provide graduate assistants?”; “Was your program the child of reform? Is this

⁷⁹ Ibid.

⁸⁰ Saunders MacLane, “Is This Doctoral Program Necessary?,” *Newsletter of the Conference Board of the Mathematical Sciences* 8, no. 4 (October 1973).

really an adequate reason for a new program?"; "Was your program established for institutional prestige or for economic advancement of your region of the country?" What becomes clear is that as far as MacLane was concerned the only legitimate reason to have a program in mathematics was if its members and its students wrote and published mathematical papers. A program, all of whose graduates obtained an industrial research position or teaching positions, following MacLane's analysis was not deemed worthy of continuation.

This extreme difference between Moore, who was willing to abandon the dissertation requirement all together, and Browder and MacLane, who wished to see the standards increase, says less about what these men thought about the development of research mathematics. Neither Moore nor Herstein believed in or called for the termination of academic research in pure mathematics. Both men were in fact pure mathematicians, and both emphasized that mathematical research should continue. Rather, the difference rises out of their distinct conception regarding the place of mathematics within higher education.

What was the ultimate impact of all these proposals? It is impossible to assign a clear chain of causality from one event to the next, but statistical data suggest some ways in which American mathematics changed in the 1970s. By 1971, the number of PhDs, which had reached a new height the year before, began declining. It took thirteen more years for the numbers to start rising again, and the record high number was only attained again seven years ago in 2006. Yet not all areas of mathematics were impacted equally. Among the doctorates degrees, research in pure (core) mathematics declined the most. In the height of the boom years, out of about 1200 PhDs, approximately 900 were in areas traditionally considered pure mathematics, though, already by 1974, a change was noticeable. In his annual report of doctorates and jobs, Anderson announced that the "good news" is that "the number of new Ph.D.'s in pure mathematics

continued to decline.”⁸¹ Anderson was able to show that over a period of only two years from 1972 to 1974, there was approximately a 23% decrease in the number of doctorates with concentration in core mathematics. At the same time the decrease in total number of PhDs was only around 6%. This decrease in pure mathematics was felt pretty much equally across all universities regardless of their national rank. In the top twenty-seven ACE ranked departments, the number PhDs in core mathematics dropped from 291 to 219 in that two years (a 25% drop). This analysis was encouraging, as Anderson noted, “Had the numbers not dropped... the present unemployment situation would be much more severe.”⁸² Pure mathematics was affected most strongly, not only by the expansions, but also by the sharp decline.

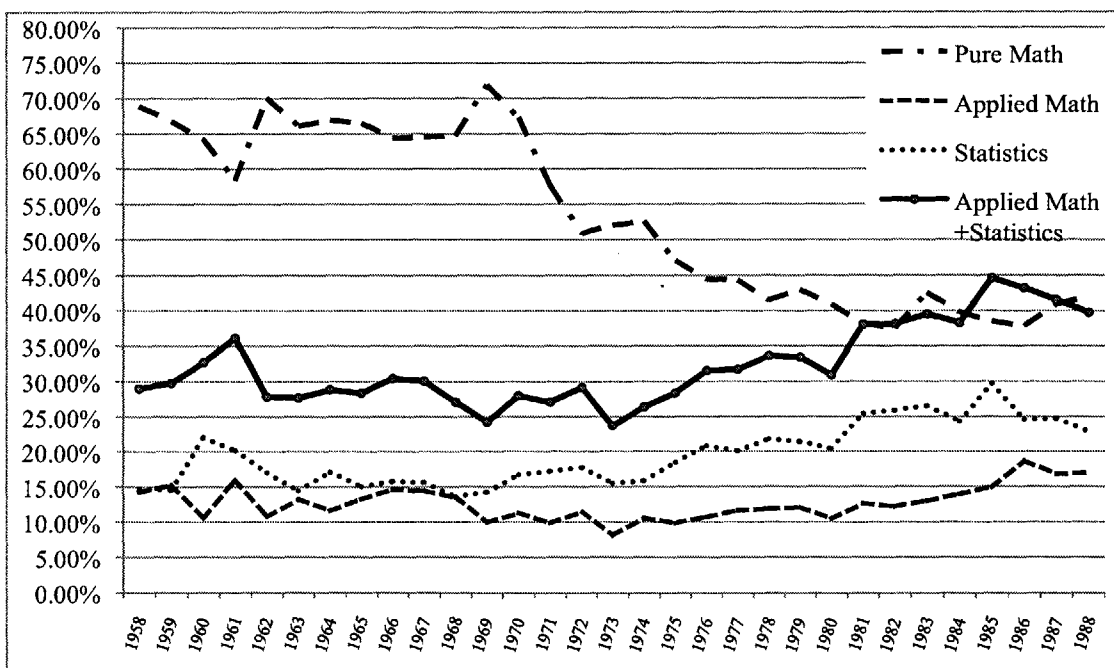


Figure 3: The percentage of PhDs in pure mathematics, applied mathematics, and statistics from 1958 until 1988. What becomes clear is that during the decade of growth degrees pure mathematics represented about 65% of all PhDs in the field with applied mathematics and statistics accounting on average for 30% of all degrees. Only when the job market began to change did the relative distribution among fields began to change, reaching an equilibrium in the early 1980s. The information for this graph was compiled by the author using the AMS annual survey, which appeared in the *Notices of the American Mathematical Society* each year.

⁸¹ Anderson, R. D., “Doctorates and Jobs, 1974 Report,” *Notices of the American Mathematical Society* 21 (1974), 337.

⁸² *Ibid.*

In his report, Anderson also made a prediction for the future. “With a prospective ten-year steady state annual employment demand of perhaps 200-300 pure mathematics doctorates (for long term retention), we should continue to reduce the numbers getting degrees until we are much closer to equilibrium.” Anderson was not so off the mark. The number of graduates in core mathematics continued to decline throughout the decade; from approximately 510 in 1972 it dropped to about 320 in 1982. At the same time, the number of doctorates in applied mathematics and statistics was not as strongly affected. The number of annual PhDs in applied mathematics did decrease a little, but only by a very small percentage, and stabilized for most of the 1970s at about 110 PhDs a year. As for the field of statistics, it is (surprisingly) quite difficult to gather statistics about the field, as its boundaries were shifting at the time (there was mathematical statistics, biostatistics, econometrics), yet it is safe to say that statistics was not adversely affected by the crisis. Throughout most of the 1950s and 1960s, pure mathematicians represented somewhere between 60% and 70% of all recent PhDs; by the mid-1980s, the percentage of pure mathematicians had decreased to a little over 40%.

* * *

In 1985, Brown University mathematician Thomas Banchoff took the opportunity to reflect on the changing distinction between pure and applied mathematics during a symposium honoring the work of Gail Young. Enticed by the symposium’s title, *New Directions in Applied and Computational Mathematics*, Banchoff chose to preface his talk with some personal recollections. “When I was an undergraduate student at Notre Dame,” he began, “I had the chance to form definite attitudes about the nature of applied mathematicians as I watched with

some fascination and dismay as my sophomore roommate gradually became one.” Banchoff explained that whereas he “took philosophy and literature courses” in addition to his more formal mathematical studies, his roommate “spent time in the physics and chemistry labs.” Further, he added, his roommate “actually read the optional applications chapters on fluid flow when we took an abstract graduate course in complex analysis.” It so happen that after they graduated from Notre Dame, both Banchoff and his roommate were accepted to the graduate program in mathematics at Berkeley University. Though, there, Banchoff recalled, their differences increased:

Whereas I took the geometry and topology option, he chose to study *differential equations*. He began to spend more and more time with numerical computations using *computers*, and he would rail against the evils of bugs and batch processing.

Ultimately he moved over the line into theoretical physics, where he worked in a *laboratory on other people's problems*. He wrote *joint papers with federal funding*.

While I went back to Notre Dame to teach in Arnold Ross's summer program, he worked as a *consultant* and he began to *make money*.⁸³

All those italicized terms Banchoff explained in his talk were the characteristics that set applied mathematicians apart from pure mathematicians like himself.

Yet before moving on Banchoff added, “Little did I suspect that virtually all of them would gradually begin to describe the work of pure mathematicians as well, precisely under the influence of several of the new direction in applied and computational mathematics.”⁸⁴ Upon receiving his doctorate from Berkeley, Banchoff took a position as a Benjamin Pierce instructor

⁸³ Thomas Banchoff, “Computer Graphics Application in Geometry: ‘Because the Light Is Better Over Here’,” in *The Merging of Disciplines: New Directions in Pure, Applied, and Computational Mathematics*, ed. Richard Ewing, Kenneth Gross, and Clyde Martin (New York, NY: Springer-Verlag, 1985), 1–2.

⁸⁴ *Ibid.*, 2.

at Harvard University for two years. He spent another year in Amsterdam working with Dutch mathematician Nicolaas Kuiper, after which he was offered a professorship at the Department of Mathematics at Brown University. Soon after he arrived at Brown, Banchoff was introduced to Charles Strauss. Strauss was completing his PhD at the Division of Applied Mathematics at Brown with a dissertation on what was at the time a completely new area of research – computer graphics. Strauss dissertation was on a three-dimensional design program for piping, and he was searching for new problems to which he could apply his graphic program. Banchoff had just the right one. For years Banchoff had been fascinated with four-dimensional geometry and what he realized as he learned more from Strauss about his work was that computer graphics provide a new tool with which to examine representation of these objects. Banchoff and Strauss began a decade-long collaboration in which they produced numerous computer graphic animations.⁸⁵ These films and the research they spurred called into question the distinction between pure and applied. Their topic, the geometry of four-dimensional surfaces, belonged to core mathematics, but their method of investigation, computer graphics, was applied.

The division between pure and applied mathematics did not end in 1970s, but it did get reconfigured once again. In the 1970s the field itself, which had been growing for more than two decades, was much larger than it was in the 1950s and consequentially and new space opened up for mathematicians who were interesting in bridging the gap between the two. As Banchoff himself noted, this new space was enabled to a high degree by the growing use of computers in mathematical research. As more and more so-called pure mathematicians began to use computers in their research, the oppositional distinction between pure and applied became harder to

⁸⁵ On Banchoff and Strauss's work, see: Thomas Banchoff, "Computer Graphics in Mathematical Research, From ICM 1978 TO ICM 2002: A Personal Reflection," 2008; Alma Steingart, "A Four-Dimensional Cinema: Computer Graphics, Higher Dimensions, and the Geometrical Imagination," in *Visualization in the Age of Computerisation*, ed. Annamaria Carusi, Aud Sissel-Hoel, and Timothy Webmoor (N: Routledge, Forthcoming).

maintain. In the 1970s and 1980s, a mathematician like Banchoff was able to work closely with computer graphics on problems in geometry, a proposition that is hard to imagine in the 1950s. The development of computer technology could not by itself account for this change. After all during the 1960s the boundaries between computing and core mathematics were still fiercely patrolled. Rather, blurring the lines between pure and applied mathematics became possible during the 1970s and 1980s because the conditions that underlined the postwar division no longer held. Looking at two-century history of mathematics, it is the strict separation of so-called pure and applied mathematics in the two and a half decades following the war that stands out, not what came before or after it.

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A Matter of Justification

As I was working on the final edits of this dissertation, the media went into a frenzy over a new report by the American Academy of Arts and Sciences regarding the state of the humanities and the social sciences.¹ Two years in the making, *The Heart of the Matter* was written by a commission of more than four dozen distinguished scholars, jurists, business leaders, and artists. The report, which was requested by a bipartisan Congressional group, strives to reaffirm the essential role of the humanities for the health and prosperity of American society. Warning against the diminishing role of the humanities, the authors of the report proposed several broad recommendations starting from K-12 education up to academic research that seeks to secure the place of the liberal arts in American life.² To support its case, the commission even released a short film composed of testimonials in praise of the humanities by household names such as George Lucas, John Lithgow, and Ken Burns.³

However, even before the report was officially released it became the target of criticism and ignited public debate.⁴ Two main points of contention emerged out of the discussions

¹ Commission on the Humanities and Social Sciences, *The Heart of the Matter* (Cambridge, MA: American Academy of Arts & Sciences, 2013).

² Whereas the report officially discusses both the humanities and the social sciences, as most commentators noted it pertains more directly to the humanities. In most of the media coverage that followed the publication of the report, the social sciences for the most part are not mentioned. I follow a similar path here.

³ The film can be accessed online at: <http://vimeo.com/68662447> (last accessed 3 August, 2013).

⁴ For a representative sample, see: Dan Berrett, "Humanities and Social Sciences Are Central to National Goals, Report Argues," *The Chronicle of Higher Education*, June 18, 2013, <http://chronicle.com/article/HumanitiesSocial-Sciences/139899/>; David Brooks, "The Humanist Vocation," *The New York Times*, June 21, 2013, sec. Opinion; Lynh Bui, "Report: Humanities, Social Science Education Needed for Innovation Along with STEM," *The Washington Post*, June 19, 2013, sec. Local, http://www.washingtonpost.com/local/education/report-humanities-social-science-education-needed-along-with-stem/2013/06/18/76076df6-d83e-11e2-a016-92547bf094cc_story.html; Stanley Fish, "A Case for the Humanities Not Made," *New York Times, Opinionator: Excusive Online Commentary From The Times*, June 24, 2013, <http://opinionator.blogs.nytimes.com/2013/06/24/a-case-for-the-humanities-not-made/>; Jennifer Schuessler, "Humanities Committee Sounds an Alarm," *The New York Times*, June 19, 2013, sec.

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surrounding the report; the first was the instrumental conception of the humanities presented in the report and second was its critique of research scholars for their tendency to overspecialize, turn inward, and separate themselves from the world around them.

As I was following the news coverage and reading various blog posts about the report, I was struck time and again by how familiar it all sounds. Having spent the past year in the world of mid-century American mathematicians, I had to remind myself that it was not mathematicians who were being discussed, but philosophers, art historians, and English professors. Not only was the nature of the criticism identical, but even the language was the same. Listening to one of my rolling lists of podcasts on my way to the office one rainy day, I laughed with recognition when I heard two journalists compare humanistic scholars to monks. “Academic writing is basically impenetrable,” one of them announced. “It is now all within the monasteries.” She then asked, “Where is it [humanistic research] represented out in the world?”⁵ The authors of the report itself already gesture toward this line of criticism when they call upon scholars to engage the public more broadly. “The public valuation of the humanities,” the authors explain, “will be strengthened by every step that takes this knowledge out of academic self-enclosure and connects it to the world.”⁶ Scholarship in English, philosophy, and the arts, the report (as well as many pundits) seems to suggest, is too insular, too specialized, and in some sense too abstract.

Despite criticizing academic scholars for their inward-looking tendencies, the authors of the report nonetheless affirm the importance of academic research. Here again the language was remarkably familiar. “As we commit to the broad-based education needed to build well-

Arts; Staff Editorial, “Oh the Humanities,” *The Duke Chronicle*, July 1, 2013, <http://www.dukechronicle.com/articles/2013/06/30/humanities-header>.

⁵ Stephen Metcalf, Dana Stevens, and June Thomas, *The Culture Gabfest “Steak All the Way Through” Edition*, The Cultural Gabfest, accessed July 29, 2013, http://www.slate.com/articles/podcasts/culturegabfest/2013/06/paula_deen_under_the_dome_and_the_decline_of_the_humanities_on_the_slate.html.

⁶ Commission on the Humanities and Social Sciences, *The Heart of the Matter*, 43.

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informed, broadly capable citizens for the future,” the authors proclaim, “we must make a renewed commitment to strengthening this scholarly *core*.”⁷ Covering the release of the report for the *New York Times*, Jennifer Schuessler quoted the president of the American Council of Learned Societies and commission member Pauline Yu in defense of the report’s treatment of scholarly research: ““The statement is right there: research is the “bedrock” of everything else.””⁸ The report, Yu reassured *New York Times* readers, was in no way discounting the importance of academic research.

Replacing scholarly research in the humanities with academic research in pure mathematics, the discussion could have just as easily taken place in mid-century America. The similarities are striking. Throughout the postwar decades, pure mathematics was derided as monastic, and calls such as the one made by the authors of the report were a common recurrence in mathematical discourse. If only mathematicians would communicate mathematical ideas and theories to a wider audience, if only they would demonstrate and actively pursue the applicability of mathematics in the world around them, many of the misconceptions about the field could be avoided. Mathematicians were being actively prodded out of their monasteries. Yet even as calls were made to remove mathematical research from the strict confines of university offices and lecture halls, mathematicians asserted that abstract mathematics is the “bedrock” of all mathematical research, in the process rebranding pure mathematics as at its core. In the 1960s, pure mathematics, like the present-day humanities, was the heart of the matter.

However, whereas the impetus behind the present report is the declining number of humanities majors and the growing prominence of science and engineering disciplines, mathematicians were making similar claims at a time of unprecedented growth. The number of

⁷ *Ibid.*, 39. Emphasis added.

⁸ Schuessler, “Humanities Committee Sounds an Alarm.”

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students flocking to mathematical courses in the 1950s and 1960s was rising faster than in any other field. Moreover, mathematical theories were penetrating growing domains of knowledge. I take this discrepancy as an invitation to meditate on the endless process of justification that pervades academic scholarship. What might account for this similarity in rhetoric underlining present-day humanities and mid-century mathematics? What is the relation between the goals of academic research and those of higher education? And how does this relationship differ between the sciences and the humanities?

In the introduction to the report the authors defend general education as an established American tradition. “It is time,” they write, “to recommit ourselves to our distinctly American form of education: broad, comprehensive, and balanced, recognizing the interdependence of all areas of knowledge.”⁹ An education based solely on the physical and the biological sciences, the report asserts, cannot secure the competitiveness and the inventiveness of the American public. Federal support to both research and education in the humanities and the social sciences, the authors argue, must be reestablished. The report does not spell out in great detail how this goal could (or should) be accomplished, and for the most part it reads more as a declaration of intent than a blueprint for action. Yet in considering the place of the humanities in two- and four-year colleges, the authors do note that the “key is defining a vision of education that meets students’ needs... not one that simply mirrors the map of current faculty specialization.”¹⁰ Here the report exemplifies the contemporary state of American academe: namely, the inherent tension between the role of the university as a bastion of liberal education on the one hand and as a research institution on the other.

⁹ Commission on the Humanities and Social Sciences, *The Heart of the Matter*, 19.

¹⁰ *Ibid.*, 34.

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In the *New York Times*, Stanley Fish took the authors of the report to task for what he conceived of as their ultimately pragmatic outlook. The report aims to fight against the instrumentalist conception of education that in many ways has been responsible for the decline of the humanities over the past few decades. Yet, according to Fish, in calling upon scholars “to apply their work to the great challenges of the era,” the authors of the report only end up reaffirming the philosophy they set to discredit.¹¹ Fish’s critique begins to point to the tangled objectives that underline liberal education and scholarly research. Whereas in biology and engineering, academic research is often justified for its promise to produce tangible societal benefits, in the humanities, academic research is often seen as an aside, a concomitant to liberal education.¹² This is not to say that academic research in the humanities cannot have discernable rewards outside the confines of a given profession (this is, after all, exactly what the report argues), but on the whole there is a gap between the production of scholarly research and the rationale for supporting it.

For the most part, scholars tend to treat the sciences and the humanities as two separate endeavors when discussing the growth of American higher education in the postwar period. Historians have noted the ways in which the new alliances formed between the federal government and academia during the Cold War transformed the university as a distinct American institution, but when treating the constitution of academic disciplines, a separation between the sciences and the humanities prevails.¹³

¹¹ Fish, “A Case for the Humanities Not Made.”

¹² In *The Marketplace of Ideas*, Louis Menand makes this point clearly when he writes, “research in the humanities is essentially a by-product of the production of college teachers. The system produces professors; professors produce research. When the demand for college teachers drops, the resources available for research drop as well.” Louis Menand, *The Marketplace of Ideas: Reform and Resistance in the American University* (New York: W. W. Norton & Company, 2010), 69.

¹³ On the transformation of the university see: Kerr, Clark. 2001 [1963]. *The Uses of the University*. Cambridge, MA: Harvard University Press; Roger L. Geiger, *Research and Relevant Knowledge: American Research*

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The development of mathematics during the Cold War brings these two narratives together. The discipline was completely altered in the war's aftermath, but it nonetheless maintained many of its characteristics as a humanistic pursuit. Mathematics increasingly benefited from federal and military support, but it was not Big Science. In a 1968 article entitled "Mathematics as a Creative Art," mathematician Paul Halmos proclaimed that "mathematics is a sociable science in the sense that... it cannot be done by one man on a desert island." But, he added, "it is not a mob science, it is not a team science."¹⁴ Neither a great painting nor a great theorem, Halmos added, could be achieved by a project or committee-based approach.¹⁵ Like scholarly research in English, history, and philosophy, academic mathematics continued to be an individualistic endeavor. It also remained highly abstract. Therefore, mathematics provides a limit case from which to understand the development of academe in the postwar period.

Under the heading "Who will lead America into a bright future?" the authors of the report proclaim that the humanities and the social sciences provide the "intellectual framework and context" for producing informed citizens. The humanities and the social sciences, they explain, "teach us to question, analyze, debate, evaluate, interpret, synthesize, compare evidence,

Universities Since World War II (New York: Oxford University Press, 1993); Rebecca S. Lowen, *Creating the Cold War University: The Transformation of Stanford* (Berkeley, CA: University of California Press, 1997); Philip G. Altbach, Patricia J. Gumpert, and Robert O. Berdahl, *American Higher Education in the Twenty-First Century: Social, Political, and Economic Challenges* (Johns Hopkins University Press, 2011). The postwar reconfiguration of the sciences was felt most acutely by the physics community. See: David Kaiser, *American Physics and The Cold War Bubble* (Chicago: University of Chicago Press, forthcoming). The literature on the growth of the social sciences in the Cold War university places them more squarely with the sciences than the humanities. See: David C. Engerman, *Know Your Enemy: The Rise and Fall of America's Soviet Experts* (New York: Oxford University Press, 2009); Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold War America* (New Brunswick, NJ: Rutgers University Press, 2013). Finally, for the humanities, see: Menand, *The Marketplace of Ideas*.

¹⁴ Halmos goes on to say, "the mathematical fraternity is a little like a self-perpetuating priesthood. The mathematicians today train the mathematicians of tomorrow and, in effect, decide whom to admit to the priesthood." Paul R. Halmos, "Mathematics as a Creative Art," *American Scientist* 56, no. 4 (1968): 375–389.

¹⁵ The period did witness some large-scale collaboration in mathematics, most notably the classification of finite simple groups by a team of more than a hundred mathematicians, but most mathematical papers were indeed written by one or at most three mathematicians. See: Alma Steingart, "A Group Theory of Group Theory: Collaborative Mathematics and the 'Uninvention' of a 1000-page Proof," *Social Studies of Science* 42, no. 2 (April 1, 2012): 185–213.

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and communicate – skills that are critically important in shaping adults who can become independent thinkers.”¹⁶ Compare this with the 1968 report on the mathematical sciences. “The Need for Mathematically Trained People” similarly asserts, “we need people who can understand a simple formula, read a graph, interpret a statement about probability. Indeed, all citizens should have these skills.”¹⁷ In both cases the rationale for growth is couched within an ideology that promotes not research per se, but basic educational skills. The gap between the two only grows wider.

In the aftermath of World War II, mathematics courses were oversubscribed and the level of courses offered in colleges around the country was rising not because there was a pressing need for more research mathematicians but because mathematical skills were required in a growing number of disciplines.¹⁸ Physicists, engineers, economists, sociologists and psychologists were expected to have a higher degree of mathematical training and it was the job of mathematicians to provide this training. Not only courses in elementary algebra and calculus, but ones in differential equations, mathematical statistics, and probability began to enroll students who were not necessarily majoring in mathematics.¹⁹ These were so-called service courses and their offering represented a sharp break from the kind of research produced by the faculty in the mathematics department. The demand on mathematics departments, that is, cannot be understood solely as one of producing more mathematical research or mathematicians. It sets mathematics apart from the rest of the sciences where instruction in biology and physics, for

¹⁶ Commission on the Humanities and Social Sciences, *The Heart of the Matter*, 17.

¹⁷ National Research Council, *The Mathematical Sciences: A Report* (Washington, DC: The National Academies Press, 1968), 5.

¹⁸ Calls for mathematical manpower emerged only in the mid to late 1950s, not in the immediate aftermath of the war. Still, even when a demand for mathematicians was forcefully expressed in the early 1960s, it was not for pure mathematicians, which as discussed in the dissertation continued to account for most of the research produced in mathematics departments.

¹⁹ John William Jewett and Clarence Bernhart Lindquist, *Report: Aspects of Undergraduate Training In the Mathematical Sciences* (Available from: Conference Board of Mathematical Sciences, 1967).

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example, is generally understood as preparation for a degree in the field. Like the objectives of liberal education, according to which every student regardless of his or her major should take some courses in the humanities and the social sciences, mathematics education is often presented as a fundamental to a well-rounded education.

Yet, unlike the humanities, in the aftermath of the war the case for support for mathematics also followed closely those of the sciences. It was mathematics research rather than education that was being promoted. As mathematical theories were increasingly appropriated in science, technology, and management, the field garnered the attention of policy makers. The mathematical community, however, was not willing to subscribe unconditionally to a pragmatic conception of their field and continued to defend the idealistic philosophy that characterized the development of the discipline in the prewar era. This is why the debates between pure and applied mathematicians became so hotly contested in the decades following the war. At core they were always about the growing dominance of an instrumental conception of knowledge and education that prevailed in American higher education. Debating what separates pure from applied research, mathematicians were in fact asking how relevance should be established and by whom.

The concept of basic research as advanced by Vannevar Bush was intended to free scientists from the direct influence of the military at the end of the war and afford them additional freedom to pursue their research. Despite the recognition by almost all of those involved that a clear distinction between basic and applied research is bound to be artificial, the enterprise had been extremely successful. The sciences prospered in the three decades following the war and new discoveries from biology to physics and oceanography abounded. However, the success of this new discourse came at a high cost. Basic research made pure research obsolete, if

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not blasphemous. In the aftermath of the war it became unthinkable to justify scholarly pursuit for its own sake. As long as the federal government was paying the bill, public accountability required a more direct correlation between the allocation of funds and measurable results. The whole academic enterprise, it seems, became engulfed in an endless process of justification.

In 1958, University of California, Berkeley mathematician Gerhard Hochschild wrote a letter to the editor of the *Notices* satirizing the recent decision by the AMS to sell tie clips displaying the emblem of the Society:

It is gratifying to observe that the American Mathematical Society has finally cast off its austere academic cloak to reveal its youthful nature in all its heartwarming splendor, no longer does it appear as an organization of maladjusted individuals who seek to support each other in their neurotic and lonely pursuit of abstract ideas. On the contrary, what we seem to have before us in a vigorous organization making a growing appeal to the more wholesome sentiment and inspirations of Man, and showing itself amply endowed with all the business acumen that is demanded in our modern world of prosperity.²⁰

Hochschild's lighthearted jab at the officers of the Society was meant as a joke, but it nonetheless reveals the transformation the mathematical community was undergoing at the time. In the aftermath of the war, mathematics (and for that matter any other scientific field) could no longer be conceived of as a community of "maladjusted individuals who seek to support each other in their neurotic and lonely pursuit of abstract ideas." Forget about liberal education; the mathematical enterprise had become too steeply immersed in the health of the nation and mathematicians had to be refashioned accordingly.

²⁰ Hochschild, Gerhard, "Editors, the Notices," undated, AMSR, Box 55, Folder 151.

Epilogue.

Returning to the humanities, when mathematicians were accused of their monastic tendencies in the aftermath of the war they immediately retorted that it was impossible to predict the future applications of present research. They were able to maintain their scholarly orientation by pointing on the one hand to the goal of broad-based mathematical education and the promise of unforeseeable applications. But what case could the humanities make?

This question of course invites an answer that is by definition instrumentalist. It asks us to conceive of humanistic research in terms of products and outcomes. It frames present work in terms of future gains. The influence of the Cold War on American universities has long been noted. The massive investment of the federal government in higher education gave rise to the expansion of the sciences, humanities, and the social sciences. Certain fields, such as high-energy physics and Soviet studies, received more direct support, but the entire academic enterprise was reconfigured. In the process, the philosophy of basic and applied research pervaded the entire educational system; not just the sciences but the humanities as well were refashioned in the postwar period. Research by definition now forces us to ask, to what end?

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