DISTRIBUTIONS IN POSTWAR MATHEMATICS

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Abstract

Distributions in Postwar Mathematics examines the intertwined histories of French mathematician Laurent Schwartz's theory of distributions and the American-hosted 1950 International Congress of Mathematicians in order to explain how mathematicians in the Second World War's wake rebuilt a discipline newly capable of and dependent on intercontinental exchanges of people and texts. Schwartz's theory, first formulated in 1944-1945, offered a new way to study differential equations for non-differentiable functions using topological vector spaces and an analogy to the basic calculus technique of integration by parts. Drawing on archives from three continents, *Distributions in Postwar Mathematics* shows how elite mathematicians joined with agents of government, philanthropic, and other institutions to foster a theory and a discipline across what would come to be called the First, Second, and Third Worlds. This account connects abstract theories to their communities of users, technologies of investigation and exposition, and social, political, and institutional contexts, casting mid-century mathematics as a heterogeneous enterprise sustained through a range of resource-intensive and partially integrated means of coordination.

Chapter 1 details European and North American mathematicians' efforts from the turn of the century through World War II to organize their discipline across national and regional scales, refracted through the Americans' two failed attempts to host interwar International Congresses of Mathematicians. These developments set the institutional and geopolitical background for Schwartz's endeavors, discussed in chapter 2, to promote distributions between 1945 and 1949. Chapters 3 and 4 offer a close analysis of American mathematicians' postwar struggles to organize the 1950 International Congress, tying their challenges to postwar reconstruction and the emerging Cold War. Both Schwartz and the ICM's organizers, in their separate contexts,

used shifting and ambivalent formulations of, respectively, techniques of mathematical analysis and ideals of internationalism, to coordinate a range of interests and entities across great distances. Chapter 5 then develops this account of coordination through polysemy and ambiguity into an explanation of distributions' intercontinental presence following the 1950 Congress, linking the theory's adoption and adaptation to postwar institutional configurations by tracing the theory to new venues in and beyond South America, North America, and Europe.

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Acknowledgements

My very first class as a freshman at Cornell University was Bob Strichartz's introductory graduate course in analysis. On the morning of that first meeting at the start of the Fall 2005 semester, Bob introduced the plan for his two-semester course by talking about a historian and philosopher of science named Thomas Kuhn who proposed that the sciences advance through paradigm shifts. The course would be structured around two paradigm shifts in modern analysis, Bob explained, relating to the two fundamental concepts in analysis of integration and differentiation: Henri Lebesgue's theory of the integral from the turn of the twentieth century and Laurent Schwartz's theory of the derivative from the mid-twentieth century. This dissertation on Schwartz's theory grew from a seed Bob planted that morning, which he and many others have cultivated over the past decade. That seed took hold in soil prepared by even more teachers, mentors, family, and friends. Though I cannot name you all here, please know that I am grateful for the lessons of many kinds that you have taught me.

My advisor Michael Gordin has shaped my scholarship, writing, and professional development in more ways than I dare enumerate. The depths of his care, insight, patience, and generosity continue to astonish me after six years under his wing. Every passage of this dissertation has benefited many times over, both directly and indirectly, from his efforts. I thank Angela Creager and Phil Nord for being part of my committee, for teaching and mentoring me in their courses, seminars, and beyond, and for sharing with me not just their collegiality but their genuine joy in doing all that they do. David Aubin, my external examiner, has been a vital advocate and critic at multiple junctures. I thank him for his insights and provocations, and especially for supervising my research in Paris during a pivotal early stage of my research. Beyond my committee, I owe special thanks to Charlie Fefferman, Joan Richards, Donald

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I have leaned heavily on the scholarship and advice of two others who have written dissertations about Laurent Schwartz and the theory of distributions. Jesper Lützen kindly shared his recollections, willingly compared notes, and graciously hosted me for a very fruitful visit to Copenhagen. Anne-Sandrine Paumier has been an invaluable guide (particularly during my time in Paris) and collaborator, and I have found myself returning again and again to her insights and observations in attempting to understand my own materials and research questions. This dissertation is quite different from what I proposed at the start of my research, and I have been able to pursue the topics I did with the detail and emphases I did only because I could rely for foundation and counterpoint on Jesper and Anne-Sandrine's own expansive investigations.

I could not have asked for a finer group of friends and classmates as interlocutors and companions. They have offered support, distraction, mirth, and meaning, have sharpened my thinking, broadened my perspectives, lightened my burdens, and added heft to my foundations. I am especially glad to count in this number a remarkable generational cohort of historians of mathematics who fill me with optimism for our field and excitement for many collaborations and debates ahead. Some names I associate with this project by way of anything from conversations shared to couches surfed (with apologies to those I have elsewhere compartmentalized): Ellen Abrams, Anthony Acciavatti, Josh Bauchner, Tom Beck, Natalie Berkman, Edna Bonhomme, Matthew Chan, Hannah-Louise Clark, Gerardo Con Diaz, Henry Cowles, Teresa Davis, Will

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This dissertation is, in part, about the importance of scholars' institutional settings for the personal, professional, and intellectual connections they aim to create. I have benefited from many institutional homes since starting this project. Foremost, Princeton's Program in History of Science and Department of History have been unfailingly stimulating and supportive from start to finish. Princeton's Department of Mathematics welcomed me as one of their own, even as the direction of my work made me more and more a misfit over the years, and I am deeply grateful to have been a part of the Princeton mathematics community. The Histoire des Sciences Mathématiques group at the Institut de Mathématiques de Jussieu hosted me for three crucial months near the start of my archival research and the Centre de Recherches Mathématiques at the Université de Montréal lent me a local footing for two and a half years at the end. I am also grateful to the History Department at The College of New Jersey for taking me on as an adjunct professor for the Fall 2013 semester. I owe a great deal to the staff and administrators of all these institutions, and especially to Princeton's Reagan Maraghy, Minerva Fanfair, Lauren Kane, Judy Hanson, and Debbie Macy, and most especially to Kristy Novak, Jackie Wasneski, and Jill LeClair.

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Though not all of them are evident in my cited sources, this dissertation drew on research conducted at some fifty different archives and libraries from across the United States as well as Argentina, Brazil, Canada, Denmark, England, France, and Uruguay. I thank the staff of all those archives and the many others who helped to facilitate my visits. In particular, this project would not have been possible without the special efforts of Erica Mosner of the Institute for Advanced Study, Adele Torrance of UNESCO, Margaret Hogan of the Rockefeller Archive Center, and Olivier Azzola of the École Polytechnique, who helped me navigate the materials in their care in ways that opened up many fruitful avenues of inquiry.

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This dissertation includes material that first appeared in the chapter "Remunerative Combinatorics: Mathematicians and their Sponsors in the Mid-Twentieth Century," in Brendan Larvor, ed., *Mathematical Cultures: The London Meetings 2012-2014* (Basel: Birkhäuser, 2016), 329-346; the article "The Myth and the Medal," *Notices of the American Mathematical Society* 62, no. 1 (January 2015): 15-20; and the online LiveScience/NSF Behind the Scenes article "How History Shaped the Global Mathematician," 21 December, 2013; and includes material from the forthcoming article "Fellow Travelers and Traveling Fellows: The intercontinental shaping of modern mathematics in mid-twentieth century Latin America," accepted for *Historical Studies in the Natural Sciences.* Further material under the title "Integration by Parts: Wordplay, Metaphor, and the Creation of an Intercontinental Mathematical Theory in the Early Cold War" received a special commendation in the British Society for the History of Science's Singer Prize competition and is currently under revision for publication. I drew as well on my ongoing collaborations with Anne-Sandrine Paumier, including an article in preparation with her and Jesper Lützen noted in the dissertation.

I presented work related to my dissertation research in the following venues: Princeton's Mathematics Graduate Student Seminar (2012, 2013), Princeton's History of Science Program Seminar (2012), the Princeton-Harvard-MIT Workshop on the History of the Physical Sciences (2013, 2014, 2015), the History of Science Society annual meetings (2013, 2014, 2015), the conference "Mathematical Superpowers: The Politics of Universality in a Divided World" at New York University (2014), the conference "Mathematical Cultures 3" in London (2014), the joint meeting of the Sociedad Latinoamericana de Estudios Sociales de la Ciencia y la Tecnología and Society for Social Studies of Science (2014), the Museu de Astronomia e Ciências Afins (2014), the Department of Rhetoric at the University of California – Berkeley

(2015), the Department of Mathematics at Cornell University (2015), the Laurent Schwartz centenary conference at the École Polytechnique (2015), the Danish History of Science Society (2015), the American Mathematical Society and Mathematical Association of America Joint Mathematics Meeting (2016), the Department of Mathematics at the State University of New York – New Paltz (2016), the Department of Mathematics at The College of New Jersey (2016), the workshop "Organizing Science for Humanity" at Columbia University (2016), and the Montreal Inter-University Workshop on the History and Philosophy of Mathematics (2016). I thank all of my hosts and interlocutors in these venues for their questions, comments, and interest in this project.

Even before Bob's analysis course at Cornell, I had started thinking about the relationships between mathematics, language, history, and philosophy at the 2004 Telluride Association Summer Program, taught by Masha Raskolnikov and Greg Tomso. Since then, in many different forms, the Telluride Association has given me an incomparable and irreplaceable community of friends, mentors, and colleagues. To my Telluride family: thank you for pushing me to be a better citizen, scholar, and person, for occupying me with important and challenging work during the year and giving me something to look forward to every June, for sharing your criticism, idealism, and love.

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better, for doing work of your own (as a “real doctor”) of unquestionable value with inspiring interest and conviction, and for so many more things than I can list here. Thank you for bringing me into your extended family, and thank you to your family for embracing me. Special thanks to Kate and Dave for welcoming me many times into their London home, including while I was in town for historical work, and to Petra for sharing your twin along with your wit and character.

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Notes on Sources

Translations. Translations of non-English sources are my own except where noted. In several places I have checked published English translations against the non-English originals and modified them where noted. I have not in general supplied the original non-English text for translated quotations, but have commented on linguistic questions where relevant to my argument and given non-English excerpts where appropriate. I thank Michael Gordin for his rigorous attention to issues of translation and his guidance on the challenges of writing in English about historical actors who wrote in many different languages and worried about language and translation in their own ways.

Transliterations. I have attempted, where citing translations into languages with Latin scripts from those with other scripts, to preserve the transliterations from the cited material. A number of Russian proper names, in particular, appear with multiple spellings in this dissertation.

Proceedings of the International Congresses of Mathematicians. The International Mathematical Union hosts a digital collection of proceedings from the International Congresses of Mathematicians as R. Keith Dennis and Ulf Rehmann, eds., *ICM Proceedings 1893-2010,* http://www.mathunion.org/ICM/. I refer to these by "[Year] ICM Proceedings," along with the titles and authors of individual reports where applicable. Here are the full citations for proceedings cited in this dissertation:

[1897] Ferdinand Rudio, ed., *Verhandlungen des ersten internationalen Mathematiker-Kongresses in Zürich vom 9 bis 11 August 1897.* Leipzig: Druck und Verlag von B.G. Teubner, 1898.

[1900] E. Duporcq, ed., *Compte Rendu du Deuxième Congrès International des Mathématiciens, tenu à Paris du 6 au 12 aout 1900. Procès-Verbaux et Communications.* Paris: Gauthier-Villars, 1902.

[1904] A. Krazer, ed., *Verhandlungen des dritten internationalen Mathematiker-Kongress in Heidelberg vom 8. bis 13. August 1904.* Leipzig: Druk und Verlag von B.G. Teubner, 1905.

[1908] G. Castelnuovo, ed., *Atti del IV Congresso Internazionale dei Matematici (Roma, 6-11 Aprile 1908), vol. I. Relazione sul Congresso – Discorsi e Conferenze.* Rome: R. Accademia dei Lincei, 1909.

[1912] E.W. Hobson and A.E.H. Love, eds., *Proceedings of the Fifth International Congress of Mathematicians (Cambridge, 22-28 August 1912).* Cambridge: Cambridge University Press, 1913.

[1920] Henri Villat, ed., *Comtes Rendus du Congrès International des Mathématiciens (Strasbourg, 22-30 Septembre 1920).* Toulouse: Édouard Privat, 1921.

[1924] J. C. Fields, ed., *Proceedings of the International Mathematical Congress held in Toronto, August 11-16, 1924.* Toronto: University of Toronto Press, 1928.

[1928] Nicola Zanichelli, ed., *Atti del Congresso Internazionale dei Matematici, Bologna 3-10 Settembre 1928.* Bologna: Società Tipografica già Compositori, 1929.

[1932] Walter Saxer, ed., *Verhandlungen des Internationalen Mathematiker-Kongresses Zürich 1932, I. Band: Bericht und Allgemeine Vorträge.* Zürich and Leipzig: Orell Füssli Verlag [1933].

[1936] *Comptes Rendus du Congrès International des Mathématiciens, Oslo 1936.* Oslo: A.W. Brøggers, 1937. No editor is credited on the title page, but the Congress's Secretary General was Edgar B. Schieldrop, and he was joined on the Publications Committee by Fredrik Lange-Nielsen and Nils Solberg.

[1950] Lawrence M. Graves, Paul A. Smith, Einar Hille, and Oscar Zariski, eds., *Proceedings of the International Congress of Mathematicians, Cambridge, Massachusetts, U.S.A. 1950.* Providence: American Mathematical Society, 1952.

Mathematical Abstracting Journals. Some bibliographical discussions, especially for certain difficult-to-access mathematical sources, are based on abstracts in the MathSciNet online database of entries in *Mathematical Reviews* (http://www.ams.org/mathscinet/) or the *zbMATH* online database of entries in the *Zentralblatt für Mathematik* (http://zbmath.org/). I justify this approach in the dissertation, based on the comparative accessibility of the (printed) abstract journals themselves to the mathematicians discussed therein. I cite these entries using the standard numerical reference conventions for the respective databases, e.g. “MR0035918 (12, 31d)” and “Zbl 37.07301” for the first volume of Laurent Schwartz’s textbook on the theory of distributions, which was the 35918th overall review in the MathSciNet database and was the fourth (‘d’) entry on page 31 of volume 12 of *Mathematical Reviews,* as well as the first entry on page 73 of volume 37 of the *Zentralblatt für Mathematik*. These reviews are not listed separately in the bibliography.

Archival Sources. I refer to archival material using the following abbreviations:

Acheson Memoranda	Dean Acheson Papers, Memoranda of Conversations File, Harry S. Truman Library and Museum, accessed online at http://www.trumanlibrary.org/hstpaper/acheson.htm.
AGU	Archivo General de la Universidad de la República, Montevideo, Uruguay.
Laguardia Papers	Archivo Privado Rafael Laguardia.
Massera Papers	Archivo Privado José Luis Massera.
AMS Records	American Mathematical Society Records, Ms. 75, John Hay Library, Brown University, Providence, RI.
Bancroft Library	Bancroft Library, University of California, Berkeley.
Lewy Papers	Hans Lewy Papers, BANC MSS 91/147.
Tarski Papers	Alfred Tarski Papers, BANC MSS 84/69.
Collège de France: Cours Peccot	Corpus Histoire du Collège de France, Affiches et programmes de cours, Fondation Peccot – Affiches des cours.
Courant Papers	The Richard Courant Papers, MC 150, New York University Archives, New York, NY.
Coxeter Papers	Harold Scott Macdonald Coxeter Fonds, Series 2: Professional Correspondence, Accession B2004-024, University of Toronto Archives and Records Management Services, Toronto, ON.
FBI	Federal Bureau of Investigation.
FBI Massera File	Headquarters File 100-HQ-341838.
FBI Schwartz File	Headquarters Case File 100-HQ-367305 re Laurent Schwartz 490/45/01/4, National Archives and Records Administration, Record Group 65, box 5327.
Jessen Papers	Børge Jessen Papers, Institute for Mathematical Sciences, University of Copenhagen, Denmark.

John Hay Library — John Hay Library, Brown University, Providence, RI.
- AMS Records — American Mathematical Society Records, Ms. 75.
- Stone Papers — Marshall H. Stone Papers, A95-32.

IAS Archives — The Shelby White and Leon Levy Archives Center of the Institute for Advanced Study, Princeton, NJ, USA.
- IAS Faculty Files — Director's Office: Faculty Files.
- IAS General Files — Director's Office: General Files.
- IAS Member Files — Director's Office: Member Files.
- IAS Mathematics — Records of the School of Mathematics: Members, Visitors, and Assistants.
- IAS Mathematics Grants — School of Mathematics, 2013-0005 accession.
- IAS Oral History — Oral History Project files and recordings.
- IAS Comptroller — Records of the Comptroller.

LBJ Memoranda — Lyndon B. Johnson National Security Files, 1963-1969, Western Europe, Lyndon Baines Johnson Presidential Library, Austin, TX.

Lighthill Papers — Lighthill Papers, GB 0103, University College London, United Kingdom.

LMS Minutes — Minutes of Council and Meetings, London Mathematical Society Archive, De Morgan House, London, United Kingdom.

McGill Mathematics Subject File — Mathematics Subject File, 1939-1947, McGill University Archives, Records Group 0002, box 0092, file 02634.

Morse Papers — Papers of Marston Morse, Harvard Depository HUGFP 106.10, courtesy of the Harvard University Archives, Cambridge, MA.

Nachbin Papers — Leopoldo Nachbin Papers, Archive of the History of Science, Museu de Astronomia e Ciências Afins, Rio de Janeiro, Brazil.

Rademacher Papers — Hans Rademacher Papers, Rockefeller University Faculty, FA128, Rockefeller Archive Center, Sleepy Hollow, NY.

Rockefeller Foundation Archives — Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, NY.
- RF Projects: U.S. — Record Group 1.1, Series 200D.
- RF Projects: France — Record Group 1.1, Series 500D.
- RF Projects: U.S.S.R. — Record Group 1.1, Series 785.
- RF Projects: Sweden — Record Group 1.1, Series 800D.
- RF Paris Field Office — Record Group 6.1, Subseries 2.
- RF Fellowships: Argentina — Record Group 10.1, Series 301E.
- RF Fellowships: Brazil — Record Group 10.1, Series 305E.
- RF Fellowships: Uruguay — Record Group 10.1, Series 337E.

Schwartz Papers — Fonds Laurent Schwartz, Archives de l'École polytechnique, Palaiseau, France.

UNESCO — United Nations Educational, Scientific and Cultural Organization Archive, Paris, France.
- Unesdoc — UNESCO online document and publication database.

Introduction: The Promised Land

Out of the Wilderness

On a mild late-August day in Cambridge, Massachusetts, Danish mathematician Harald Bohr took the podium at the opening ceremony of the 1950 International Congress of Mathematicians to bear witness to the future of modern mathematics.[1] Already a minor national celebrity at the start of his career as a star of the 1908 Olympic silver medalist Danish soccer team, the brilliant and gregarious "little Bohr" (as he was affectionately known in Denmark) quickly rose as a leader of the Danish, Scandinavian, and Western European mathematical communities.[2] Through his training and collaborations, he developed strong ties to idealistic internationalist mathematicians in Germany, Britain, Sweden, and the United States. Like his older brother, the physicist Niels, Bohr positioned Copenhagen in the 1920s and especially the 1930s as an oasis from the tensions and rivalries that roiled Europe.[3] This was his fourth consecutive International Congress, the first of what he hoped would be a new era of peace and cooperation among the world's mathematicians.

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[1] Weather from www.wunderground.com/history/airport/KBOS/1950/8/30/DailyHistory.html (accessed 2016). Bohr's address is Harald Bohr, "Address of Professor Harald Bohr," *1950 ICM Proceedings*, 127-134.

[2] See, e.g., the caricature and poem from the *Berlingske Tidende,* 10 Feb 1934, discussed in Anita Kildebæk Nielsen, Peter C. Kjærgaard, and Henrik Knudsen, "Danske avisers fremstilling af naturvidenskabsmænd Eksempler fra der 20. århundrede," Hosta Works-in-Progress 15, 2003 [first published in *Orbitalen* 2002], History of Science Department, University of Aarhus, Denmark (accessed at http://css.au.dk/forskning/publications/hosta/, 2016), 6-7.

[3] Henrik Kragh Sørensen, "Confluences of agendas: Emigrant mathematicians in transit in Denmark, 1933-1945," *Historia Mathematica* 41 (2014): 157-187, on 160-164; Henrik Knudsen and Henry Nielsen, "Pursuing Common Cultural Ideals: Niels Bohr, Neutrality, and International Scientific Collaboration during the Interwar Period," in Rebecka Lettevall, Geert Somsen, and Sven Widmalm, eds., *Neutrality in Twentieth-Century Europe: Intersections of Science, Culture, and Politics after the First World War* (New York: Routledge, 2012), 115-139.

Though he kept the thought mostly to himself, Bohr sensed that he might be dying and considered the 1950 trip his valedictory voyage.[4] A Moses in the wilderness of interwar and wartime international mathematics, he died five months after his 1950 address, unable to enter the promised land he vividly evoked at the first postwar Congress. This dissertation is about Bohr's promised land, its contours and contexts, its potent imaginaries and tangled realities. It is about the changing nature of mathematical theories and institutions in times of strife and ferment. It is about how the discipline of mathematics emerged after the Second World War as not just international but intercontinental.

Bohr's official purpose on the opening program in 1950 was to present the two winners of the Fields Medal, an award born out of the modest financial surplus from the 1924 International Congress of Mathematicians that Canadian mathematician John Charles Fields arranged at a nadir of international goodwill among mathematicians.[5] Bohr used the occasion to herald a young French mathematician whom he considered his discipline's brightest lodestar, Professor Laurent Schwartz, of the University of Nancy.[6] Three years earlier, Schwartz had caught Bohr's attention at one of France's first postwar international mathematical symposia with a new approach to functions and differential equations he called the theory of distributions. Bohr urged him to scout the terrain of postwar mathematics and his theory's place in it, introducing Schwartz to prominent figures abroad and arranging for Schwartz's first

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[4] This was the assessment of Bohr's close friend and protégé Børge Jessen, Jessen to Courant, 30 Jan 1951, Courant Papers, box 55, folder 2.

[5] E.M. Riehm and F. Hoffman, *Turbulent Times in Mathematics: The Life of J.C. Fields and the History of the Fields Medal* (Providence: American Mathematical Society, 2011), 129-161, 179-187.

[6] Here, one Moses heralded another: a secular Jew, Schwartz's full first name was Laurent Moïse.

international and intercontinental mathematical travels.[7] Now, at the world's largest ever gathering of mathematicians, Bohr compared Schwartz's theory to Descartes's analytic geometry as an epoch-making advance in mathematics meriting interest and excitement across the globe. Schwartz, Bohr made clear, would lead the world's mathematicians out of the wilderness.

Both conceptually and pedagogically, Schwartz built his theory around a new understanding of integration by parts, a basic calculus technique that allows one to simplify the integral of a complicated expression by breaking that expression into two pieces, one of which is easy to integrate and the other of which is easy to differentiate. Schwartz used this technique to find derivatives for distributions (his generalization of mathematical functions) that were not just difficult but mathematically impossible to differentiate using established methods. This gave an abstract mathematical justification to several well-worn methods and concepts from the last half-century of mathematical physics and analysis that were based on such paradoxical derivatives. In Bohr's promised land, the theory of distributions would be a part of the basic calculus curriculum across the world's universities and technical schools.[8] While that never came to pass, within little more than a decade Schwartz's theory was an active topic of research and advanced pedagogy on five different continents. It was among the first mathematical theories to have such an intercontinental research community so soon after its inception. In many respects it was the definitive intercontinental theory, marking out for mathematicians and bureaucrats what mathematics meant in a postwar discipline that for the first time could sustain research communities across oceans and hemispheres.

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[7] Anne-Sandrine Paumier, Michael J. Barany, and Jesper Lützen, "From Nancy to Copenhagen to the World: The internationalization of Laurent Schwartz and his theory of distributions," in preparation.

[8] Bohr, "Address," 133.

The twinned histories of intercontinental mathematics and Schwartz's theory of distributions in this dissertation turn on a shared central theme: that rhetorical and conceptual ambiguity, ambivalence, and incoherence, rather than being antithetical to large-scale programs of scientific theory or diplomacy, are precisely what make them possible. Historians are accustomed to the fuzziness of political rhetoric—after all, politics is a realm with an abundance of words and few hard truths. The organizational history of intercontinental mathematics, which played out through diplomatic rhetoric and action at a variety of scales, turned on internationalist mathematicians' ability to use forceful but ambiguous claims to recast history, assert legitimacy, command resources, and circumvent the many inevitable obstacles they faced in forging new international disciplinary infrastructures. Something similar happened in the period's mathematics, a realm premised to its core on the power of clear language and hard truths. Theoretically, Schwartz, Bohr, and distributions' other advocates used their own kinds of forceful but ambiguous claims to recast their own history, insist on their concepts' relevance, and create an appearance of coherence and unity. These provisional and shifting claims, for both diplomacy and distributions, necessarily came before the long and difficult processes of exchange and reconciliation that underwrote more lasting and durable intercontinental theories and institutions.

Accordingly, this dissertation primarily intervenes in two broad historiographies. First, it places mathematics in the historiography of the globalization of elite science, with a special focus on the question of how internationalism as an ideology relates to international institutions as situated historical achievements. Second, it uses the institutional, geographical, material, and conceptual histories of distributions to develop an account of modern mathematics and the mathematical sciences in Europe and North America in a global context. Together, these interventions recast the mid-century rise of an American-led intercontinental system of professional mathematics from a story about migration and economic leadership into a story about the changing nature of mathematical theories and institutions in a contested geopolitical terrain.

International and Intercontinental Science

At its simplest, this dissertation's explanation of the globalization of mathematics begins by marking the space between words and actions, between theory and practice, and asking what that space opens up for historical speakers and actors. The specific space in question, here, lies between internationalism as an expressed goal or ideal and the actual international undertakings mathematicians and others pursue (by a combination of words and actions) in the name of those goals or ideals. Both international ideals and realized international projects harbor important ambiguities. On the one hand, historical actors may advocate widely varied and sometimes mutually contradictory actions on the basis of what they assert to be one and the same internationalism. On the other, the designation "international" can apply to undertakings under a broad range of circumstances, from those involving members of dozens of nations participating on equal terms to those with one dominant nation and token participation from another—even leaving aside, for the moment, how one defines a nation or membership therein.

Mathematicians formed new kinds of international institutions and infrastructures in the mid-twentieth century, and did so under the banner of many different and shifting internationalisms. In order to trace the effect of unstable international rhetoric on the historical stabilization of a certain kind of international formation, I designate the latter with the analytic term of "intercontinental" mathematics. The most salient feature of intercontinental mathematics, as the name implies, is elite mathematicians' ability to coordinate their professional activities (including but not limited to research and publication) across multiple continents, something they did not routinely do before 1945. I preserve the term "international" as an actors' way of jointly asserting many different claims under a superficially common rubric.

This gap between inconsistent, ambiguous usage and the *appearance* (indeed, the *enunciation*) of a single meaning is what made "international" rhetoric so significant in the history of intercontinental mathematics. Twentieth-century scientists and mathematicians used the adjective "international," often accompanied by emphatic adverbs like "truly" or "genuinely," as though its meaning were both self-evident and compelling. The term signaled an ambition that could guide policies and values and command cooperation and support, while leaving considerable room for variations and adaptations in just how those policies and values took shape in response to considerable political and logistical obstacles. The most important ambiguities concerned the twin prerogatives of inclusion and non-exclusion. Inclusion implied the participation of multiple nations, though the imagined scope of this multiplicity and what counted as a nation could vary. Non-exclusion, conversely, meant that individuals or organizations should not be barred on the basis of national criteria, though such criteria frequently blurred into other desiderata. Rather than signal a commitment to one or another value of inclusion or non-exclusion, the term "international" operated as a rallying cry, a common and consequential motto that accommodated multiple simultaneous meanings. Turning again and again to a single headline phrase let mathematicians emphasize different prerogatives at different points and for different audiences, all without ever having to acknowledge their shifting interpretations.

Their most important ambiguity, perhaps, involved the very meaning of nationhood that underwrote their conception of internationalism. Mathematicians bootstrapped their definition of nationhood according to the presence of autonomous organizations of mathematicians operating at something like a national scale. These did not necessarily correspond to politically autonomous states or other entities of the sort commonly recognized by international political organizations. At the 1950 Congress, for example, England, Scotland, and Northern Ireland all had their own national delegations, as did some British colonies, while "Germany" had a single delegation composed entirely of mathematicians from the Federal Republic of Germany, that is, West Germany (itself a political entity of recent advent). States and regions without national mathematical organizations or recognizable surrogate institutions were virtually invisible to the organizers' outreach efforts, which were based on a combination of communicative norms and practices from political diplomacy and mathematical scholarship. The Congress's lone representative of an African nation, for instance, was the Oxford-educated Englishman Frederick Valentine Atkinson, who would already have been aware of the Congress while a Lecturer at Oxford before leaving to head the new mathematics department at University College, Ibadan, Nigeria.[9] With their focus on national and institutional matters, organizers ignored or dismissed other parameters of diversity like gender, race, and class.

Legal historians trace the term "international" to Jeremy Bentham's 1789 coinage of "international law" to replace and clarify a dubious "law of nations."[10] Bentham initially framed international law as concerning juridical interactions among states in themselves. But with the

[9] See Angelo B. Mingarelli, "A glimpse into the life and times of F. V. Atkinson," *Mathematische Nachrichten* 278, no. 12-13 (2005): 1364-1387, on 1368-1369.
[10] M.W. Janis, "Jeremy Bentham and the Fashioning of 'International Law'," *The American Journal of International Law* 78, no. 2 (1984): 405-418.

advent of the nation-state, the rise and fall of colonial empires, and the political and cultural transformations these entailed, it did not take long before internationalism's uses and implications multiplied to include a much wider range of relationships spanning individuals, states, and civilizations.[11]

The phrase "truly international," with its emphatic adverb, began to appear regularly in the mid-nineteenth century in discussions of law, commerce, sports, the arts, and other areas of international interest—usually in the form of a boast or an aspiration. An 1851 advertisement in English, German, and French, for instance, sought contributions for a Greenwich seamen's hospital by touting it as a "truly international Charity," a "wahrhafte International-Wohlthätigkeits-Anstalt," and a "vraie philanthropie internationale," respectively.[12] By the century's end the phrase peppered writings about the policies, projects, and membership of international organizations, including those devoted to science and medicine. In the first half of the twentieth century, a period whose two World Wars marked out an expanded scale of geopolitics and public consciousness, "truly international" became a watchword for the ambitions and challenges of an era.

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[11] E.g. Samuel Moyn, *The Last Utopia: Human Rights in History* (Cambridge: Belknap, 2010); Samera Esmeir, *Juridical Humanity: A Colonial History* (Palo Alto: Stanford University Press, 2012); Akira Iriye, *Cultural Internationalism and World Order* (Baltimore: Johns Hopkins University Press, 1997); Akira Iriye, *Global Community: The Role of International Organizations in the Making of the Contemporary World* (Berkeley: University of California Press, 2002); Jessica Wang, "The United States, the United Nations, and the Other Post-Cold War World Order: Internationalism and Unilateralism in the American Century," in E.W. Schrecker, ed., *Cold War Triumphalism: The Politics of American History After the Fall of Communism* (New York: New Press, 2004), 201-234.

[12] "Exhibition Official Catalogue Advertiser," supplement to *Official Catalogue of the Great Exhibition of the Works of Industry of All Nations, 1851* (London: Spicer Brothers; W. Clowes & Sons, 1851), 37, accessed at https://books.google.com/books?id=18M9AAAAcAAJ&pg=PA361. The Greenwich advertisers' French and German translations did not quite adhere to the adverbial grammar of the English text, but adding the equivalent of the adjective "true" in translation had the same ambiguous but emphatic effect as the English adverb "truly."

The institutions and ideals of science have been potent loci for new international imaginaries.[13] From the birth of "international" rhetoric onward, science became a way of articulating national differences, a means of national control and administration, a source of national pride or concern, and a platform for cooperation or competition among nations.[14] International ideals and rhetoric, in turn, shaped science and mathematics. Historians of science have repeatedly explored national, international, transnational, and related contexts of scientific organization, and have interrogated claims of national identity or transnational significance while elucidating the roles of such claims in the ideals and practices of science.[15] Historians of mathematics, as well, have examined the historical contexts of the discipline through studies of

[13] For a historiographical survey, see Geert J. Somsen, "A History of Universalism: Conceptions of the Internationality of Science from the Enlightenment to the Cold War," *Minerva* 46 (2008): 361-379. See also Elisabeth Crawford, *Nationalism and internationalism in science, 1880-1939: four studies of the Nobel population* (Cambridge: Cambridge University Press, 1992). Cf. Paul Forman, "Scientific Internationalism and the Weimar Physicists: The Ideology and Its Manipulation in Germany after World War I," *Isis* 64, no. 2 (1973): 150-180.

[14] E.g. John Krige and Kai-Henrik Barth, eds., *Global Power Knowledge: Science and Technology in International Affairs, Osiris* 21 (2006); Carol E. Harrison and Ann Johnson, eds., *Science and National Identity, Osiris* 24 (2009); Gabrielle Hecht, ed., *Entangled Geographies: Empire and Technopolitics in the Global Cold War* (Cambridge: MIT Press, 2011); Chris Manias, "The *Race prussienne* Controversy: Scientific Internationalism and the Nation," *Isis* 100 (2009): 733-757; Nancy Stepan, *The Idea of Race in Science: Great Britain 1800-1960* (London: MacMillan, 1982); Suman Seth, ed., "Focus: Relocating Race," *Isis* 105, no. 4 (2014): 759-814.

[15] E.g. Simone Turchetti, Néstor Herran, and Soraya Boudia, "Introduction: have we ever been 'transnational'? Towards a history of science across and beyond borders," *British Journal for the History of Science* 45, no. 3 (2012): 319-336; Mark Walker, "The 'national' in international and transnational science," *British Journal for the History of Science* 45, no. 3 (2012): 359-376; Carola Sachse and Mark Walker, eds., *Politics and Science in Wartime: Comparative International Perspectives on the Kaiser Wilhelm Institute, Osiris* 20 (2005); Michael D. Gordin, Karl Hall, and Alexei Kojevnikov, eds., *Intelligentsia Science: The Russian Century, 1860-1960, Osiris* 23 (2008); Stuart McCook, ed., "Focus: Global Currents in National Histories of Science: The 'Global Turn' and the History of Science in Latin America," *Isis* 104, no. 4 (2014): 773-817.

its regional and international organization, as well as of the role of ideals of internationalism and universalism in mathematics in the nineteenth and twentieth centuries.[16]

By and large, mathematicians followed the familiar disciplinary and organizational arc of their counterparts in the natural sciences in the first part of the twentieth century.[17] They began the century with their elites concentrated in Europe, filled with international ambitions but less accomplished in international practice. The Great War tore asunder much of the goodwill on which the turn-of-the-century mathematicians built their cross-border cooperation, but left institutional infrastructures intact to varying degrees.[18] The interwar period was a time of grave suspicions and divisions combined with efforts at reconciliation.[19] In particular, debates over

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[16] Karen H. Parshall and Adrian C. Rice, eds., *Mathematics Unbound: The Evolution of an International Mathematical Research Community, 1800-1945* (Providence: American Mathematical Society, 2002), especially the editors' introduction, "The evolution of an international mathematical research community, 1800-1945: An overview and an agenda," 1-15; Olli Lehto, *Mathematics Without Borders: A History of the International Mathematical Union* (New York: Springer, 1998); Catherine Goldstein, Jeremy Gray and Jim Ritter, eds., *Mathematical Europe: History, Myth, Identity / L'Europe Mathematique: Histoires, Mythes, Identités* (Paris: Éditions de la Maison des sciences de l'homme, 1996), esp. Herbert Mehrtens, "Modernism vs. counter-modernism, nationalism vs. internationalism: style and politics in mathematics 1900-1950," 517-529.

[17] E.g. Christa Jungnickel and Russell McCormmach, *The Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein* (Chicago: University of Chicago Press, 1986), vol. 2; Daniel J. Kevles, *The Physicists: The History of a Scientific Community in America* (Cambridge: Harvard University Press, 1995 [1977]); Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), ch. 2-4; Mary Jo Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950* (Berkeley: University of California Press, 1993); John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press, 1990).

[18] See esp. David Aubin and Catherine Goldstein, eds., *The War of Guns and Mathematics: Mathematical Practices and Communities in France and Its Western Allies around World War I* (Providence: American Mathematical Society, 2014).

[19] See esp. Lehto, *Mathematics Without Borders*, 23-90; Riehm and Hoffman, *Turbulent Times,* 103-204; Reinhard Siegmund-Schultze, *Rockefeller and the Internationalization of Mathematics Between the Two World Wars: Documents and Studies for the Social History of Mathematics in the 20th Century* (Basel: Birkhäuser, 2001).

whether or not to exclude scientists from the former Central Powers in new international organizations prompted some of the sharpest articulations yet of the values of inclusion or non-exclusion under the rubric of internationalism. National and regional mathematical communities responded in different ways to the tumult of political and military change on the European continent. American mathematicians began to come into their own as a world-leading disciplinary community, in part through domestic reorganization and in part by taking over some of what the scientific institutions of a war-torn and divided Europe could no longer supply.[20] The Second World War saw the collapse of many foundations of European scientific leadership while creating or reinforcing many of the conditions for American and Soviet postwar prominence. Cold War exchanges brought American values and infrastructures to more and more parts of the world while incorporating foreign personnel and new connections abroad within American mathematics.[21]

These broad similarities can sometimes overshadow important differences. One concerns just how connected national mathematical communities were to each other at different points in this history. Historians and sociologists of science now take for granted that practitioners of laboratory sciences face particular obstacles to sharing their practices, apparatus, and specimens across great distances. While many of the peculiarities of laboratory objects and the tacit skills required to manipulate them do not have direct parallels in mathematics, historians and sociologists of mathematics and mathematical physics have shown that mathematical theories

[20] See esp. Karen Hunger Parshall, "'A New Era in the Development of Our Science': The American Mathematical Research Community, 1920-1950," in David E. Rowe and Wann-Sheng Horng, eds., *A Delicate Balance: Global Perspectives on Innovation and Tradition in the History of Mathematics, a Festschrift in Honor of Joseph W. Dauben* (Basel: Birkhäuser, 2015), 275-308.
[21] Cf. Zuoyue Wang, "Transnational Science during the Cold War: The Case of Chinese/American Scientists," *Isis* 101, no. 2 (2010): 367-377.

and practices can be tightly bound up in their local contexts nonetheless.[22] Without projects that demanded particular international collaboration or inspired particular international competition, moreover, by some measures mathematicians were among the least internationally connected scientific professionals at the turn of the twentieth century. Some traveled and exchanged texts, to be sure, but it was not unusual for leading mathematicians to focus exclusively on their own regional, national, or even local literatures if based in the right kind of institution. Mathematicians almost uniformly lacked intercontinental ties of the sort found in the life sciences and several areas of the physical sciences (such as astronomy or geophysics), forged through geography-specific projects of shared concern. Intellectual and practical divisions between national communities of mathematicians could thus be particularly stark and difficult to bridge, despite the discipline's oft-presumed universalism.

Another difference is in the specific geographic contours of intercontinental undertakings when they did begin to arise. Latin America became particularly important for postwar mathematicians for a variety of economic, political, and geographical reasons, many of which derived from specific mathematical practices and institutions and the values and experiences of specific mathematicians. This range of reasons is evident in the itinerary of the theory of distributions, which spread through the interlocking postwar infrastructures of the Rockefeller Foundation and UNESCO to become the first postwar mathematical theory to be widely studied

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[22] E.g. Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003); David Kaiser, *Drawing theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005); Martina Merz and Karin Knorr Cetina, "Deconstruction in a 'Thinking' Science: Theoretical Physicists at Work," *Social Studies of Science* 27, no. 1 (1997): 73-111; Michael J. Barany and Donald MacKenzie, "Chalk: Materials and Concepts in Mathematics Research," in Catelijne Coopmans, Michael E. Lynch, Janet Vertesi, and Steve Woolgar, eds., *Representation in Scientific Practice Revisited* (Cambridge: MIT Press, 2014), 107-129.

in South America. Mathematical infrastructures that arose in Latin America through European migration and American hegemony became, after the Second World War, resources for mathematicians on both sides of the Atlantic to respond to the opportunities and constraints of an increasingly connected world cleaved in two by the Cold War.

A third difference comes from the specific role of the Second World War in reshaping the discipline. Historians of science, especially of physics, have studied the institutional, cultural, and epistemic changes associated with the war-mediated advent of "big science," among other developments that would come to characterize the postwar era.[23] Although some accounts of international mathematics portray the Second World War as little more than an ellipsis, historians of mathematics have begun to account for the war's distinctive effects on the discipline and its international dimensions.[24] Mathematicians lacked many of the trappings of big

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[23] E.g. Peter Galison and Bruce Hevly, eds., *Big Science: the Growth of Large-Scale Research* (Stanford: Stanford University Press, 1992); Galison, *Image and Logic*; Kaiser, *Drawing Theories Apart*. Two foundational statements are 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, and Dan Kevles, "Cold war and hot physics: Science, security, and the American state, 1945-56," *Historical Studies in the Physical and Biological Sciences* 20, no. 2 (1990): 239-264. Jessica Wang has argued that historians' early preoccupation with physics, as opposed to the social sciences (for instance), has led to an over-emphasis on World War II and postwar periodizations for many phenomena of Cold War science. Jessica Wang, "Colonial Crossings: Social Science, Social Knowledge, and American Power from the Nineteenth Century to the Cold War," in Jeroen van Dongen, ed., *Cold War Science and the Transatlantic Circulation of Knowledge* (Leiden: Brill, 2015), 184-213, on 184-190. See also Hunter Heyck and David Kaiser, "Introduction, Focus: New Perspectives on Science and the Cold War," *Isis* 101, no. 2 (2010), 362-366.
[24] E.g. Olli Lehto's *Mathematics Without Borders* skips directly from 1933-1939 (chapter 3) to 1945-1951 (chapter 4) in the chronology of its table of contents. The war consumes just three paragraphs (on pp. 125-126) of Guillermo P. Curbera's *Mathematicians of the World, Unite! The International Congress of Mathematicians: A Human Endeavor* (Wellesley, MA: A.K. Peters, 2009). This dissertation discusses a range of contributions to the recent literature on mathematics and World War 2, much of it focused on applied mathematics, including 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; Bernhelm Booß-Bavnbek

science, from centralized national laboratories to extensive partnerships between academics and industry. Other well-known features of postwar science had outsized effects for mathematicians. Over the course of the 1940s, American mathematicians went from worrying about a shortage of posts to worrying about training enough men (and occasionally women) to fill them. Americans learned to work on contract models for military sponsors, and used their new sources of support to assist foreign colleagues. There were more positions for mathematicians at research institutes and in several industries and more fellowships and subventions for beginning and established mathematicians alike. Like their American counterparts, and (in Western Europe) with substantial American funding, European universities expanded rapidly as well. In the developing world, new institutions and resources allowed many mathematicians for the first time to sustain academic mathematics as a stable career and profession.

Modern Mathematics and the Nature of Mathematical Theory

Mathematical texts, like all texts, require interpretation. This simple observation can have significant historiographical consequences in view of a philosophical and technical tradition in mathematics that often treats the discipline's formalisms as perfectly transparent.[25] For even if individual mathematical statements offer less interpretive flexibility than, say, a sentence by Marcel Proust, mathematical arguments and theories can be enormously complex and leave room

and Jens Høyrup, eds., *Mathematics and War* (Basel: Birkhäuser, 2003); Alma Steingart, *Conditional Inequalities: American Pure and Applied Mathematics, 1940-1975* (PhD Dissertation, Massachusetts Institute of Technology, 2013), esp. ch. 1; and a large body of work by Reinhard Siegmund-Schultze.

[25] On these historiographical consequences, see, e.g., Catherine Goldstein, *Un théorème de Fermat et ses lecteurs* (Saint-Denis: Presses universitaires de Vincennes, 1995); Karine Chemla, ed., *History of Science, History of Text* (Dordrecht: Springer, 2004); Caroline Ehrhardt, *Itinéraire d'un texte mathématique: Réélaborations d'un mémoire de Galois au XIXe siècle* (Paris: Hermann, 2012).

for a substantial variety of understandings. Such variations, in turn, mean that even individual mathematical statements can have very different meanings when interpreted in different contexts. I have argued elsewhere that modern mathematics is such a complex and variegated discipline that its scholars not only routinely struggle to understand each other's work but also structure their disciplinary institutions and personal habits around their common expectation of such struggles.[26] Following mathematicians' efforts to communicate across continents shows the extent of these struggles and how deeply they affected mathematicians' goals and practices in a changing world.

This dissertation shares its premise with the recent historiography of mathematics that treats mathematical theories as more than the sum of their formal definitions, deductions, theorems, and conjectures. Instead, theories must be understood through the entire range of practices and interpretations that individuals and communities bring to bear in studying, teaching, and developing them. For Schwartz's theory to succeed on an intercontinental scale, the nature of theory itself had to change. That is, mathematicians were only able to study something like the theory of distributions collectively across continents because of new practical and interpretive contexts that fundamentally altered how mathematicians conceptualized and engaged with their theories as such. These changes, with their origins in late-nineteenth and early-twentieth century mathematical modernism, took their most significant forms amidst the intellectual and institutional transformations of the middle decades of the twentieth century.[27] While mathematics

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[26] Barany and MacKenzie, "Chalk," esp. 109-112.

[27] On mathematical modernism, see e.g. Leo Corry, *Modern Algebra and the Rise of Mathematical Structures*, 2nd edn. (Basel: Birkhäuser, 2004 [1996]); Leo Corry, "Introduction: The History of Modern Mathematics – Writing and Rewriting," *Science in Context* 17 no. 1/2 (2004): 1-21; Herbert Mehrtens, *Moderne – Sprache – Mathematik: eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme* (Frankfurt am Main:

has never been a univocal subject, the form and effects of its polyvocality shifted discernibly after the Second World War, in ways that helped distributions to spread with particular speed and efficacy. Borrowing a central term from Schwartz's theory, I describe a postwar discipline that was *partially integrated* through many overlapping but distinct ways of knowing, researching, and teaching the theory of distributions. This heterogeneity of expertise and practice furnished the conditions for the theory's spread and development.

Identifying postwar mathematics with integration by parts is not simply a suggestive appropriation of terminology. Rather, the technique of integration by parts itself sustained a variety of practices and understandings in contexts ranging from basic calculus to the theory of topological vector spaces. Common terminologies, symbolic operations, and conceptual explanations let distribution theorists borrow different conceptions of integration by parts for different purposes and audiences. Crucially, this promiscuity of contexts and meanings allowed distributions to gain a foothold among professional mathematicians years before Schwartz published anything like a comprehensive development of the theory's foundations. Mathematicians fostered a unified-seeming theory not in spite of the heterogeneity of its manifestations, but by way of them, not in spite of the diversity of its postwar contexts, but in response to them.

Distributions were a product of their time, but Schwartz's was no typical theory, and this dissertation is not a case study.[28] On the one hand, distributions spread exceptionally far across

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Suhrkamp, 1990); Ivor Grattan-Guinness, *The Search for Mathematical Roots, 1870-1940: Logics, set theories and the foundations of mathematics from Cantor through Russell to Gödel* (Princeton: Princeton University Press, 2000); Jeremy Gray, *Plato's Ghost: The Modernist Transformation of Mathematics* (Princeton: Princeton University Press, 2008).

[28] On the use and abuse of the terminology and genre of case studies in the history of science, see Galison, *Image and Logic*, 55-63.

an exceptionally wide disciplinary terrain at a time when many leading mathematicians worried about the discipline's seeming fragmentation. Even before the Great War, mathematicians mourned the passing of an era where great mathematicians could claim to understand new developments spanning the entire discipline.[29] On the other hand, amidst the vast range of activity in postwar pure and applied mathematics, it is hard to call the theory of distributions (or any one theory, for that matter) typical of the period's research or pedagogy. Research in differential equations and other areas of mathematics to which distributions were most relevant developed according to researchers' distinctive constellations of habits and interactions. A distribution theorist was more likely to adopt practices and intuitions from certain physicists or engineers than from certain other mathematicians in far-off branches of the discipline. Within the subfields that harbored distribution theorists, meanwhile, not all researchers would have considered themselves experts on distributions, and a far smaller share would consider themselves to have worked actively on or even with the theory. Mathematicians developed the theory of distributions in a context with many distinct theories but with minimal direct competition among them.

It is thus insufficient to portray new mathematical programs as entering a single field of theory and argument, altering or offering alternatives to existing programs. Such contact and conflict among contemporaneous mathematical theories requires difficult and sustained

[29] E.g. G.H. Darwin, in "Part I. Report of the Congress," *1912 ICM Proceedings,* I: 33-36. Cf. Nicolas Bourbaki, "The Architecture of Mathematics," trans. Arnold Dresden, *The American Mathematical Monthly* 57, no. 4 (April 1950): 221-232, first published in F. Le Lionnais, ed., *Les grands courants de la pensée mathématique* (Marseille: Cahiers du Sud, 1948), where the worry is expressed as an adamant insistence on the unity of mathematics. Unity or disunity in the sciences have long been themes of interest for practitioners, historians, philosophers, and sociologists. See, e.g., Peter Galison and David J. Stump, eds., *The Disunity of Science: Boundaries, Contexts, and Power* (Stanford: Stanford University Press, 1996).

interventions by working mathematicians, and these interventions are not confined to technical manipulations or demonstrations. Historians have crucially begun to distinguish between mathematicians' and others' images of the nature and meaning of mathematics from the technical body of claims and practices in mathematicians' theoretical research.[30] This distinction has allowed for accounts of the relationship between mathematicians' philosophical and institutional ambitions and their technical work, a relationship that is rarely straightforward or transparent. However, even works that account for barriers to communication and understanding that hamper coordination on the technical side of mathematical work often advance a univocal view of theories: mathematicians know them all or in part, but not heterogeneously.[31] In the history of distributions, by contrast, different partial ways of knowing a mathematical theory each had different functions and effects, and intercontinental mathematical institutions and research programs relied on such heterogeneity.

Mathematicians learned, taught, and pursued distributions, when they did so at all, in a variety of registers. Distributions as a program of research had a comparatively narrow reach, while distributions as a mathematical ideal or political tool extended much farther. As Peter Galison has stressed, the history of science can appear very differently depending on one's scale of analysis, and the means by which historical actors coordinated their ideas and practices across

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[30] Corry, *Modern Algebra*; Amy Dahan Dalmedico, "An image conflict in mathematics after 1945," in U. Bottazzini and A. Dahan Dalmedico, eds., *Changing images in mathematics: from the French Revolution to the new millennium* (London: Routledge, 2001), 223-253; 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. 2 (1997): 297-342; Christopher J. Phillips, "In Accordance with a 'More Majestic Order': The New Math and the Nature of Mathematics at Midcentury," *Isis* 105, no. 3 (2014): 540-563.

[31] E.g. Alma Steingart, "A group theory of group theory," *Social Studies of Science* 42, no. 2 (2012): 185-213; Christopher Hollings, *Mathematics Across the Iron Curtain: A History of the Algebraic Theory of Semigroups* (Providence: American Mathematical Society, 2104).

different scales can illuminate aspects of scientific knowledge and institutions and their respective transformations that would otherwise remain opaque.[32] Galison focused on the relationship between theories, instruments, and experimental and organizational practices in high-energy physics. His distinction amongst scales involved unpacking the different roles of varying kinds and sizes of instruments and institutions as a way of interrogating the march of apparently scale-less theories.

The history of distributions shows that theories too, in and of themselves, can appear very differently at different scales. Vague or partial conceptual justifications, suggestive images and analogies, detailed classroom lessons, and comprehensive research agendas all represented different ways of knowing the theory of distributions that functioned in different institutional settings and scales. Crucially, the limits to communication associated with different sites and media could be as important as what those contexts enabled. The theory thrived through ambiguity and approximation alongside exposition and investigation. At each scale, distributions demanded different forms of allegiance, of learning and unlearning, related to but not wholly determined by each other scale. Such micro-, macro-, and mesocosms together defined what it was to know the theory of distributions in a world where some perhaps could follow one's formalisms but many more could follow one's dreams.

The Dissertation's Parts

Chapter 1 (Disintegration by Parts) begins by surveying the formal efforts, concentrated in Europe, to organize professional mathematicians internationally between 1897 and 1939. Though

[32] Galison, *Image and Logic*, 59-63, 781-803. On the problem of scale in the historiography of Cold War science, see also David Kaiser, "Booms, Busts, and the World of Ideas: Enrollment Pressures and the Challenge of Specialization," *Osiris* 27 (2012): 276-302.

mathematicians spoke in lofty terms of their international principles, their limited efforts at coordination beyond small regional or national organizations revealed a highly fragmented discipline held together by the travels and correspondence of a narrow elite. Where historians of mathematics have tended to draw a sharp distinction between the idealism that preceded the Great War and the conflict and apparent loss of innocence that followed it, this chapter instead emphasizes the continuities in both institutional structures and obstacles to organization that persisted across the twentieth century's first four decades, as well as mathematicians' gradual and piecemeal institutional consolidation at a range of scales. By renewing attention to the range of actors and motivations that defined interwar mathematics, I account for and challenge mathematicians' retrospective view of prewar mathematics and interwar internationalism. I unpack these developments by examining two failed American efforts to host an International Congress of Mathematicians, after their invitations were endorsed by the 1920 and 1936 Congresses to host meetings, respectively, in 1924 and 1940. The chapter then explains how mathematicians responded to the Second World War, focusing on the United States and its mathematicians' adaptations to changing conditions in Europe. American elites emerged from the war with access to vastly greater resources than they enjoyed previously, while European mathematicians found themselves scattered in ways that forged and reinforced connections across borders and continents.

Chapter 2 (A Widely Anticipated Invention) charts the initial development, from 1944 to 1949, of Laurent Schwartz's theory of distributions, drawing on published documents and archival materials from the period and on the many post-hoc accounts from Schwartz and others. I examine the simultaneous creation of the theory's invention narrative alongside several different versions of the theory for its different initial audiences. These included the limited readership of Schwartz's 1945 announcement of his theory, the wider readership for reviews of that and subsequent papers in *Mathematical Reviews* and the *Zentralblatt für Mathematik*, participants in a pivotal 1947 Rockefeller-sponsored colloquium on harmonic analysis in Nancy, and Schwartz's audiences at the French Society of Radio-electricians and the 1949 Canadian Mathematical Congress. From the start, Schwartz and his advocates depicted his theory as one that was widely anticipated in the techniques and frameworks of the disciplinary communities to whom they presented Schwartz's new formulations. These portrayals offered the basis for claims to relevance and insight while exposing Schwartz to challenges to his priority and originality. Dissecting Schwartz's earliest articulations of his theory, those he circulated before publishing his definitive 1950-51 textbook on distributions, this chapter explains how Schwartz used analogies, promises, ambiguities, and wordplay to build inchoate communities of interested consumers and potential researchers. Many of these narrative tactics derived from specific technical features of Schwartz's presentation of distributions, such as his use of hierarchies of generalization in the theory of operators and function spaces, as well as his reinterpretation of the calculus technique of integration by parts. I contextualize such articulations in an emerging approach to mathematical theory and argument, associated with but not limited to the radical French "Bourbaki" group of which Schwartz was a part, that would define this period for many.

Chapter 3 (The Closed Circuit of Open Ideals) follows the small cohort of American organizers of the 1950 International Congress of Mathematicians (initially planned for 1940) as they attempt to revive the Congress after the Second World War's end. The chapter situates their negotiations over funding and sponsorship and their efforts to create a new International Mathematical Union amidst the rapidly changing economic and political contexts of postwar reconstruction and the early Cold War. I trace how different conceptions of internationalism helped organizers tie their disciplinary ambitions to different sponsors' goals for a durable postwar peace while reinforcing the disciplinary authority of a narrow elite. These ideals proved most fraught and ambivalent in Marshall Stone's failed attempt to route plans for the new Union through the newly formed United Nations Educational, Scientific and Cultural Organization (UNESCO) and his ultimately successful fallback effort to constitute the Union at the 1950 Congress itself. Stone and his fellow organizers advanced an internationalism based on a politically charged narrative about prewar and interwar mathematics, the nature of mathematics and mathematicians, and their respective roles within the sciences and the community of nations. The chapter closes by examining a long-overlooked development from the 1950 Congress's vexed process of organization that may have been one of its most important legacies: the incorporation of Latin American mathematicians into the upper echelons of international mathematics. This was, in some respects, a long-brewing result of twentieth-century regimes of American hegemony in the Western hemisphere. In other respects, however, it appears as an accidental consequence of Stone's opportunistic adaptations to his inability to assert American hegemony through traditional European channels.

Chapter 4 (Internationalism by Committee) examines the detailed ideological and logistical negotiations that underwrote the programmatic efforts discussed in chapter 3. I begin with Marston Morse's work to assemble a slate of distinguished foreign plenary speakers in his field of mathematical analysis, a challenge that required negotiating the limited available information about analysts abroad and reconciling the range of attitudes and priorities of American mathematicians involved in the selection process. Seemingly abstract assertions about internationalism from the previous chapter found concrete manifestations in Morse's struggles to present a balanced and representative list of speakers. I then turn to the diplomatic machinations required to convert invitations into attendance, starting with the little-known story of Uruguayan mathematician José Luis Massera, who intended to attend the Congress but was prevented from doing so by a combination of financial and diplomatic barriers. I compare Massera's struggles to those faced by Laurent Schwartz, whose visa was initially denied over his Communist politics, but who was ultimately able to attend the Congress that propelled him to international celebrity. The so-called "Schwartz Affair," which dragged out over months of tense negotiations and a threatened French boycott, exposed many of the persistent transatlantic political tensions that frustrated American organizers seeking to claim their place at the helm of international mathematics.

Chapter 5 (Integration By Parts) charts the theory of distributions from 1950 into the second half of the decade, by which time its community of established researchers spanned Western and Eastern Europe, North and South America, and several points beyond. The chapter begins with Harald Bohr's address at the opening ceremony of the 1950 Congress, when he presented Schwartz with a Fields Medal in recognition of the latter's mathematical promise. Bohr first encountered Schwartz and his theory at the 1947 colloquium discussed in chapter 2, and Bohr's evangelism contributed to both Schwartz's and his theory's swift international acclaim. I use Bohr's address to explain how a diverse intercontinental collection of mathematicians began to incorporate the theory of distributions into their research, accounting for how they reconciled ideas and techniques as well as how they manufactured a sense of coherence across a diverse body of work engaging with distributions. The chapter then traces the theory of distributions across seminars, lecture halls, publications, and individuals' travels from Paris to Chicago to Manchester to Warsaw, Bombay, Buenos Aires, Montevideo, Rio de Janeiro, and several other sites of research and training. Across these contexts, I show how distributions' users interacted with new infrastructures of global development, and how those infrastructures in turn shaped mathematicians' theories and their social and institutional relationships. These theories and communities, I contend, were partially integrated through a range of communicative media and practices, and I conclude by developing this observation into a claim about the relationship between theories and their technical details for the historiography of modern mathematics.

Chapter 1: Disintegration by Parts

At a Glance

The first International Congress of Mathematicians took place in Zürich in 1897 at a time when Europe seemed brimming with international scientific endeavors.[1] From expositions to world's fairs to special-purpose gatherings to reconcile theories and nomenclatures, scientists in a variety of disciplines found more and more occasions to cross borders and commune with foreign counterparts—indeed, even to think of those counterparts as colleagues.[2] As tabulated in the proceedings*,* its 242 registered participants at the Zürich Congress hailed from sixteen countries, with 68 from the host Switzerland. Fewer than one in five participants crossed more than one

[1] As part of a broader historical revision discussed in chapter 3 of this dissertation, American mathematicians later claimed as the first such Congress the so-called International Mathematical Congress that they hosted with two visiting mathematicians from Germany, one from Austria, and one from Italy in conjunction with the 1893 World's Columbian Exposition. J. R. Kline, "Secretary's Report," *1950 ICM Proceedings,* 121-145, on 121. The 1893 attendance is listed in E. Hastings Moore, Oskar Bolza, Heinrich Maschke, and Henry S. White, eds., *Mathematical Papers read at the International Mathematical Congress held in connection with the World's Columbian Exposition Chicago 1893* (New York: MacMillan and Co., 1896), ix-xii. Many have adopted this revisionist claim, e.g. R. Keith Dennis and Ulf Rehmann, eds., *ICM Proceedings 1893-2010,* online at http://www.mathunion.org/ICM/ (accessed 2016); Donald J. Albers, G.L. Alexanderson, and Constance Reid, *International Mathematical Congresses: An Illustrated History, 1893-1986* (New York: Springer, 1987), 2-3. I here follow the sequential identification European mathematicians used in the first half of the twentieth century, beginning with the Zürich Congress as the first. See also Olli Lehto, *Mathematics Without Borders: A History of the International Mathematical Union* (New York: Springer, 1998), 2-7.

[2] E.g. Brigitte Schroeder-Gudehus, "Les congrès scientifiques et la politique de coopération internationale des académies des sciences," *Relations internationales* 62 (1990): 135-148; Mary Jo Nye, ed., *The Question of the Atom: From the Karlsruhe Congress to the First Solvay Conference. 1860-1911. A Selection of Primary Sources* (New York: American Institute of Physics, 1984); Richard Staley, *Einstein's Generation: the Origins of the Relativity Revolution* (Chicago: University of Chicago Press, 2008), part II; Evan Hepler-Smith, *Nominally Rational: Systematic Nomenclature and the Structure of Organic Chemistry, 1889-1940* (PhD Dissertation, Princeton University, 2016), 69-175.

national border to reach the Congress, and just the seven from North America crossed an ocean.[3] By the numbers, the Congress was hardly more than a regional affair with a smattering of interlopers from afar.

But that is not how the organizers saw it. Speaking on their behalf, Ferdinand Rudio, a professor at the renowned Eidgenössischen Polytechnikum, pronounced that "It suffices to consult the program or to cast a glance in this room to agree that the Congress already fulfilled its purpose if it had no other end than to procure for the mathematicians of every country in the world the occasion to interact amicably and exchange their ideas."[4] And while the Congress's participants were not strictly representative of the mathematicians of every country in the world, by some measures Rudio's assessment was not far off the mark. In 1897, professional mathematics was predominantly a regional enterprise centered in Western Europe, with only a narrow elite regularly traveling great distances after their initial training. Mathematicians sustained international connections, to the extent such connections existed, largely through handwritten letters and printed journals and books, supplemented by individuals' occasional trips abroad when they had the means and freedom to roam.[5] Moreover, one could be an elite European mathematician while hardly traveling at all and only occasionally taking notice of new work from beyond one's own backyard. The mathematicians of every country in the world could

[3] *1897 ICM Proceedings*, 65-78. I have attempted to follow the national identifications, as well as such word choices as "country" versus "nation," as closely as possible from the respective proceedings considered here. In the table cited here, "country" translates the German "Land."
[4] Ferdinand Rudio, "Über die Aufgaben und die Organisation internationaler mathematischer Kongresse," accompanied by a translation as "Sur le but et l'organisation des congrès internationaux des mathématiciens," in *1897 ICM Proceedings*, 31-42, on 32-33, 39. The Eidgenössischen Polytechnikum received its current name, the Eidgenössische Technische Hochschule, in 1911. Here, "every country" translates "aller Länder" in German and "tous les pays" in French.
[5] See Karen H. Parshall and Adrian C. Rice, eds., *Mathematics Unbound: The Evolution of an International Mathematical Research Community, 1800-1945* (Providence: American Mathematical Society, 2002).

not be taken in at a glance. But for a privileged group of Western European mathematicians, a glance could well take in most of those who really counted.

Rudio spoke as the Congress was just beginning, and there were other purposes yet to fulfill. These included inspiring solidarity and common enterprise among mathematicians, preparing a directory of the world's mathematicians, solving the problems of mathematical bibliography, and giving an overview of the current state of the diverse branches of mathematics with the "form, color, warmth, life, and personality" of spoken rather than written discourse. For, up to that point, international mathematics (insofar as it was international) was a discipline on paper.[6]

Those, like Rudio, with the ambition and resources to read widely could easily appreciate their paper discipline's persistent divisions. Calling for a directory, Rudio acknowledged how hard it was to know where all one's far-flung colleagues resided, much less their special expertise and topics of research.[7] When it came to solidarity, Rudio saw "questions of terminology" and conventions (such as the decimal division of the circle) to require an "accord analogous to that of the physicists on the essential units Volt, Ampère, Ohm."[8] He hoped new

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[6] On these paper connections' scope and limitations, see Sloan Evans Despeaux, "International Mathematical Contributions to British Scientific Journals, 1800-1900," in Parshall and Rice, eds., *Mathematics Unbound,* 61-87; Jesper Lützen, "International Participation in Liouville's *Journal de mathématiques pures et appliquées*," in Parshall and Rice, eds., *Mathematics Unbound,* 89-104; June E. Barrow-Green, "Gösta Mittag-Leffler and the Foundation and Administration of *Acta Mathematica,*" in Parshall and Rice, eds., *Mathematics Unbound*, 139-164; Jeremy J. Gray, "Languages for Mathematics and the Language of Mathematics in a World of Nations," in Parshall and Rice, eds., *Mathematics Unbound*, 201-228.
[7] On the effort in the 1950s to realize Rudio's envisioned world directory, a project that continues today as the *Electronic World Directory of Mathematicians* (http://www.mathunion.org/ewdm/), see Lehto, *Mathematics Without Borders,* 130-136.
[8] Indeed, questions of notation stayed with the prewar international congresses. For instance, the 1912 Congress attempted, but failed to resolve, "the unification of vectorial notations." "Part I. Report of the Congress," *1912 ICM Proceedings*, I: 7-66, on 40. By contrast, when attempting to revive the International Congresses of Mathematics and International Mathematical Union after

bibliographic efforts could acquaint readers with “all that has appeared in a given domain not only in the last years, but also in the last months or even the last weeks.” These aspirations underscored the contrast between the interpersonal discipline Rudio could spy at a glance and the patchwork international discipline whose documentary traces eluded such a survey.

Historians of mathematics have tended to treat the period between the Zürich Congress and the outbreak of the Great War as, to quote one pair, “the golden years” of international mathematics.[9] Their accounts, tinted by the retrospective lens of mathematicians’ interwar turmoil, have centered on irenic and apolitical cooperation, on an innocence and common initiative soon to be lost. National competition, realized through new journals and societies, through training and travel, had an open spirit that redounded toward common progress. The Société Mathématique de France, for instance, listed foreign addresses for more than a third of its 298 members in 1914.[10] Despite their national, institutional, and linguistic heterogeneity,

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World War II, American mathematician Marshall Stone asserted that mathematicians had “no serious problems of nomenclature,” a claim his peers would not have found controversial. Extract of 19 Apr 1947 NRC-CISU minutes enclosed in Fleming to Dresden, 27 Aug 1947, AMS Records, box 32, folder 62.

[9] “The Golden Years” is the subtitle of chapter 6, treating this period, in E.M. Riehm and F. Hoffman, *Turbulent Times in Mathematics: The Life of J.C. Fields and the History of the Fields Medal* (Providence: American Mathematical Society, 2011). See also Lehto, *Mathematics Without Borders,* 7-14; Guillermo P. Curbera, *Mathematicians of the World, Unite! The International Congress of Mathematicians: A Human Endeavor* (Wellesley, MA: A.K. Peters, 2009), 9-54. On such “golden years” historiography, see Brigitte Schroeder-Gudehus, “Probing the Master Narrative of Scientific Internationalism: Nationals and Neutrals in the 1920s,” in Rebecka Lettevall, Geert Somsen, and Sven Widmalm, eds., *Neutrality in Twentieth-Century Europe: Intersections of Science, Culture, and Politics after the First World War* (New York: Routledge, 2012), 19-42.

[10] David Aubin, Hélène Gispert, and Catherine Goldstein, “The Total War of Paris Mathematicians,” in David Aubin and Catherine Goldstein, eds., *The War of Guns and Mathematics: Mathematical Practices and Communities in France and Its Western Allies around World War I* (Providence: American Mathematical Society, 2014), 125-177, on 132.

towering figures of mathematics seemed to work together in print and occasionally in person to realize a fruitful and newly unified discipline bursting with promise.[11]

Look past the cosmopolitan itineraries of a few towering figures and the podium pronouncements from famous gatherings, however, and one finds a rather more fragmented scene. This chapter's next section begins with a demographic tour of the International Congresses from the so-called golden years. Rather than a flourishing international discipline unconfined by national borders, participation rates for these congresses attest to a discipline predominantly concentrated in a few national or regional centers. Mathematicians at German, French, Italian, and (to a lesser extent) Swiss institutions consistently led the field, but their oscillating participation figures show that geography and language continued to divide mathematicians more than common ideas and spirits united them. Travel was slow, the warmth of personal contact fleeting, and the felt limitations of written communication plain to see. The famous globetrotters of the turn of the century were far and away the exceptions to a rule that mathematicians tended to stay near home. And with a somewhat broader collection of exceptions tied to famous names and theorems, their work tended to stay near home with them. Between the lines of the congresses' proceedings, one finds mathematicians steeped in local idiosyncrasies, disagreeing on fundamental points of notation, and struggling to share their work abroad when they cared to share it at all.

The "golden years" trope contrasts a resplendent fin-de-siècle with an interwar nadir and the glorious postwar rebirth of international mathematics. If one ceases to see the start of the twentieth-century through rose-tinted lenses, the period encompassing the two World Wars looks

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[11] On linguistic heterogeneity and its relationship to national and international coordination in the sciences in this period, see Michael D. Gordin, *Scientific Babel: How Science Was Done Before and After Global English* (Chicago: University of Chicago Press, 2015), ch. 2-5.

different as well. This chapter develops a reading of this middle period based on recent scholarship and archival findings, setting the ground for the interpretation of postwar mathematics elaborated in the remainder of this dissertation. Here, the obvious divisions of the wartime and interwar mathematical communities were not a resounding failure of international endeavor but rather a vital condition for local and national institutional consolidation. Mathematicians' local responses to geopolitical conflict helped them establish the discourses and means to build a newly intercontinental postwar discipline.

A large part of the story is the rise of the American mathematical community. For reasons of economics and geography, many of the most important interwar transformations to international mathematics were transatlantic in scope. Yet this is not simply an account of Europe's eclipse by the United States. On the one hand, while modern mathematics had certainly been centered in Europe, the converse (that Europe, as such, was the center of modern mathematics) was never the case.[12] For French mathematician Henri Lebesgue, it was "well known" in 1907 that "the French are rarely well acquainted with English work."[13] The same could be said of virtually any pairing of countries, regions, or even some cities with sufficiently robust local mathematical communities. Mathematics in Europe was always more confined and divided than later generations would remember it to have been. To the extent the "European" moniker implies international coordination beyond a few weakly-connected institutions and national communities, one could hardly call much of modern mathematics European, as opposed to French (indeed, Parisian), German, Italian, or English.

[12] A wide-ranging and important analysis of and challenge to the notion of "European mathematics" is Catherine Goldstein, Jeremy Gray and Jim Ritter, eds., *Mathematical Europe: History, Myth, Identity / L'Europe Mathematique: Histoires, Mythes, Identités* (Paris: Éditions de la Maison des sciences de l'homme, 1996).

[13] Henri Lebesgue, Review of Young and Young, *The Theory of Set Points*, *Bulletin des sciences mathématiques* 31 (1907): 132-134, quoted in Aubin, Gispert, and Goldstein, "Total War," 163.

On the other hand, the eclipse story tends to emphasize European migration rather than domestic organization in the American ascent. Importantly, American mathematical institutions' domestic reconfiguration began well before the European intellectual exodus.[14] Migration accentuated many of these changes' effects, but the most consequential and lasting features of interwar and wartime American mathematics were largely endogenous, even when European mathematicians and institutions supplied important motivations and models. In fact, the divisive middle period from the outbreak of World War I to the close of World War II saw important institutional transformations in Europe as well as the United States (albeit for quite different reasons). At a time when Europe's mathematical communities appeared at some scales to be disintegrating, the precise contours of their disintegration made room at smaller scales for emerging infrastructures and interconnections that would help bring postwar mathematicians together on a wider stage once more.

A Series Interrupted

To those gathered at the international congresses that followed Rudio's, what did mathematicians' warmer, more personal international community look like? One indicator can be found in the synopses of members tallied by each successive organizer. Every set of congress proceedings included a list of registered participants and guests and their addresses, and all but the 1900 Paris proceedings closed the list with a tabulation by country.[15] For its part, Paris played host to roughly the same number of participants as had converged in Zürich (with a larger share from domestic institutions), but the Paris participants hailed from some 30 different nations (to

[14] Cf. S. 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 (1986): 55-98.
[15] Attendance figures from these tables are compiled in Table 1 at the end of this chapter.

Zürich's 16).[16] Fewer mathematicians made the trip from the German-speaking countries that dominated the previous Zürich congress, but participation from the Americas surged. Nineteen from the United States joined lone mathematicians from Argentina, Brazil, Canada, and Mexico, and two from Peru. A French receveur particulier (tax collector) represented colonial Algeria, and one mathematician came all the way from Tokyo. At the closing session, Gaston Darboux "emphasized how it is necessary for mathematicians to rub elbows with each other, since there are no scholars for whom work is more disinterested, nor less susceptible to conquer the profane masses."[17] By virtue of their elite métier, mathematicians were alone and so had to stick together.

Participation was up but the number of represented countries went down at the 1904 Heidelberg Congress. Its 336 full participants and 60 registered guests hailed from 19 countries, with three out of five participants recorded as German.[18] The 1908 Rome Congress boasted 535 "congressisti" and 165 family members from 22 nations.[19] Nearly a third were Italian, a quarter traveled from Germany, an eighth from France, and a tenth from Austria-Hungary. As in 1897, Switzerland and its four neighbors thus accounted for more than four in five participants. The 1908 proceedings also contain another notable census, listing twelve financial contributions totaling some 4,481 Lira from government ministries, the University of Rome, insurance

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[16] "Liste Générale des Membres," *1900 ICM Proceedings*, 3-10. I have here followed the national identifications in the member list, which separates England and Scotland, for instance, and which listed a few German mathematicians instead under Bavaria, Württemberg, or Prussia. The 1900 Paris Congress is best remembered today for David Hilbert's list of 23 problems, while few recall that the opening plenary address was by Mortiz Cantor, "On the Historiography of Mathematics." The themes of internationalism and historical memory meet many times in this dissertation.
[17] "Compte Rendu Résumé du Congrès," *1900 ICM Proceedings,* 12-13. I have quoted the paraphrase of Darboux's remarks from the proceedings.
[18] "Verzeichnis der Kongreßmitglieder," *1904 ICM Proceedings*, 11-23.
[19] "Elenco dei Delegati e dei Congressisti," *1908 ICM Proceedings*, 7-20.

companies, publishers, and the German Mathematical Society, which paid forward its surplus balance of 800 Marks from the Heidelberg Congress.[20]

In 1912, the Congress took leave of the Swiss and their neighbors for Cambridge, England. The hosts continued to hold the largest delegation: 221 "members" and 49 "members of family" represented the United Kingdom, among a total of 574 members and 134 family from 28 listed nationalities.[21] Astronomer and Cambridge Philosophical Society President Sir George Howard Darwin (Charles's fifth child) welcomed congress-goers with a reflection on mathematical psychology, pure and applied mathematics, the discipline's stark specialization, and the recent passing of Henri Poincaré, perhaps the last person "fully competent to understand mathematical research in all its many diverse branches."[22] Recalling Rudio from 1897 and Darboux from 1900, the University of Cambridge's Vice-Chancellor R. F. Scott then expressed "the hope that [the Congress] will also have its lighter and more personal side"—colored by the "distinctive" collegiate settings of the "ancient English Universities" of Oxford and Cambridge.[23]

Among their final acts in Cambridge in 1912, the assembled congregants unanimously accepted Swedish mathematician Gösta Mittag-Leffler's invitation, with King Gustav's blessing, to hold the 1916 Congress in Stockholm. (They deferred decisions on invitations to subsequent Congresses in Budapest and Athens.) In several respects, the Stockholm invitation marked a

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[20] "Preparazione del Congresso," *1908 ICM Proceedings,* 3-6, on 4.

[21] "Part I. Report of the Congress," *1912 ICM Proceedings*, I: 7-66, on 10-28. While Scottish participants were sometimes listed separately in such accounts, here they were included under "United Kingdom." Though Austria-Hungary remained politically unified, Austrian and Hungarian members were counted separately.

[22] Ibid., 33-36, on 33.

[23] Ibid., 36. The Vice-Chancellor also made special mention of "the Mathematical Tripos, and of its influence on the study and progress of Mathematics both in Cambridge and Great Britain," particularly in the emphasis on "problems" in relation to "general theories" (p. 37). Cf. Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003).

departure from the established pattern of the International Congresses of Mathematics. Most significantly, Sweden and its mathematical institutions were far from the national mathematical powers that the hosts to date had been. Rather, as Turner and Sørensen have argued, Mittag-Leffler premised his bid on the *regional* emergence of Scandinavia as a significant and distinctive home of professional mathematics.[24] Turner and Sørensen identify what they term a "conational" mathematical community that Mittag-Leffler consolidated through a series of Scandinavian Congresses of Mathematicians beginning in 1909, in part as a response to Norway and Sweden's 1905 political disunion and in part in an attempt to emulate the virtues Mittag-Leffler found in the International Congresses of Mathematicians (especially the 1908 Rome Congress).[25]

Like the International Congresses, the conational Scandinavian Congresses rested squarely on national mathematical communities and relied on robust regional participation—though in the Scandinavian case this regionalism was more explicit in the design. By developing a regional identity, Scandinavian mathematicians asserted for themselves a greater relevance in the broader European discipline, both rhetorically and institutionally.[26] They asserted this international relevance, for instance, by publishing the first Scandinavian Congress's proceedings in French with a major Leipzig publisher.[27] Thanks to "the plasticity of its institutionalisation," their regional layer of organization proved to be a remarkably durable locus

[24] Laura E. Turner and Henrik Kragh Sørensen, "Cultivating the Herb Garden of Scandinavian Mathematics: The Congresses of Scandinavian Mathematicians, 1909-1925," *Centaurus* 55 (2013): 385-411.
[25] Ibid., 392-393, 396.
[26] Ibid., 388.
[27] Ibid., 400-401.

for Scandinavian mathematicians to negotiate national and international exigencies and aspirations across the many upheavals of the early twentieth century.[28]

Mittag-Leffler had the last word at the Cambridge Congress's closing session: "Congresses for mathematics are perhaps of greater importance than for the other sciences. Mathematics, the science of number, […] treats at its foundation nothing but pure abstractions. That is what renders the study of ideas communicated solely through print much more laborious than in the other sciences whose object is rather concrete. It follows that the study of the mathematical literature, more again perhaps than in the other sciences, finds a precious complement in the verbal exchange of ideas."[29] As the founder and editor of the period's leading international mathematical journal, *Acta Mathematica,* Mittag-Leffler knew the promises and shortcomings of the mathematical literature better than most.[30] As a participant in the International Congresses and a leading organizer of the Scandinavian Congresses, he spoke with authority on what such congresses could offer mathematicians. He hoped the Cambridge Congress "would be none other than a term in a never-interrupted series of like congresses, renewed every four years."

Before Mittag-Leffler could host the 1916 Congress, the Great War interrupted the series. And that interruption had far-reaching consequences for mathematicians' exchange of ideas, verbal and otherwise. Most immediately, war refocused mathematicians' attention on national exigencies, even when (as with Mittag-Leffler) they aimed to hold on to the burgeoning regional and international formations of the years just prior to the war. With the outset of war, mathematicians from belligerent nations loudly questioned international projects on ideological

[28] Ibid., 397, 405.
[29] *1912 ICM Proceedings*, 45, in French.
[30] Barrow-Green, "Gösta Mittag-Leffler."

grounds, asserting fundamental cultural and civilizational differences that extended even to the discipline Darboux once described as the most disinterested.[31] Reeling from his son's death at the front, French mathematician Émile Picard vowed to Mittag-Leffler in 1915 that "German barbarity, however scholarly, must be slaughtered lest France disappear from the rank of great nations."[32] That year, the council of the French Mathematical Society purged its rolls of "members who belong to enemy nations."[33] Darboux himself volunteered for the French Academy of Science's efforts to support the French war effort, as did Picard and other French mathematical elites.[34]

The war's effects on international mathematics were practical as much as ideological. On the one hand, the war dramatically curtailed the travel and communication necessary to sustain robust international institutions. After an official International Congress in 1916 proved untenable without the cooperation of the major European mathematical societies, Mittag-Leffler soldiered on by extending a wide invitation to a "less risky" Scandinavian Congress on neutral ground in Stockholm where foreigners could gather without "need to fear the public opinion in their own country."[35] Even among those who remained ideologically committed to such

[31] Lehto, *Mathematics Without Borders*, 14; Riehm and Hoffman, *Turbulent Times,* 106-114.
[32] Picard to Mittag-Leffler, 22 Feb 1915, Institut Mittag-Leffler, Picard, Brev. 38, quoted in Riehm and Hoffman, *Turbulent Times,* 109. I have departed somewhat from Riehm and Hoffman's translation. For more on Picard's objections to German mathematics, see Aubin, Gispert, and Goldstein, "Total War," 143-144.
[33] Société mathématique de France, "État de la Société Mathématique de Frace au commencement de l'année 1915," *Bulletin de la Société Mathématique de France* 43, supplement ("Vie de la société"), p. 2. See Aubin, Gispert, and Goldstein, "Total War," 143.
[34] Aubin, Gispert, and Goldstein, "Total War," 141.
[35] Nørlund to Mittag-Leffler, 18 Nov 1915, Institut Mittag-Leffler, quoted in Turner and Sørensen, "Scandinavian Mathematics," 397, 407. Turner and Sørensen assert that Nørlund's characterization of the 1916 plans matched Mittag-Leffler's.

gatherings, attendance at this shadow ICM was slim, with limited participation even from non-Swedish Scandinavians.[36]

At the same time, European mathematicians with a wide range of national and international commitments mobilized within their respective nations.[37] Some took up arms and headed to the front, others applied themselves mathematical problems of military significance, and yet others taught elementary mathematics to officers or servicemen or played significant administrative roles in prosecuting a modern war. In 1915, Sir George Greenhill, a retired military academy professor of mathematics, called the present conflict "a Mathematical War" in his widely reprinted Mathematical Association presidential address.[38] In a booklet distributed on the German front in 1916 titled *Mathematics in War,* a Hamburg high school teacher shared Greenhill's hyperbolic claim that the present conflict should "be called a mathematical war."[39]

The mathematical demands of different aspects of the war varied considerably, and many mathematical tasks were scarcely related to the special skills or interests of professional mathematicians. In many situations, however, mathematicians were able to ply advanced mathematics to considerable effect, especially in areas related to ballistics and aeronautics. Some mathematicians found in their wartime activities new conceptions of the relation between mathematical ideas and institutions and the modern state. Others, even those engaged in

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[36] Turner and Sørensen, "Scandinavian Mathematics," 397.
[37] On the Western side of this mobilization, see esp. Aubin and Goldstein, eds., *War of Guns and Mathematics*. The volume also includes a discussion of American mathematicians' World War I itineraries. A rare comparative study including parties from both sides of World Wars I and II (with a focus on the latter war) is Reinhard Siegmund-Schultze, "Military Work in Mathematics 1914-1945: an Attempt at an International Perspective," in Bernhelm Booß-Bavnbek and Jens Høyrup, eds., *Mathematics and War* (Basel: Birkhäuser, 2003), 23-82.
[38] June Barrow-Green, "Cambridge Mathematicians' Responses to the First World War," in Aubin and Goldstein, eds., *War of Guns and Mathematics,* 59-124, on 59, 103-104.
[39] Paul Riebesell, *Mathematics in War* (Leipzig: Teubner, 1916), 1, quoted in David Aubin and Catherine Goldstein, "Placing World War I in the History of Mathematics," in Aubin and Goldstein, eds., *War of Guns and Mathematics*, 1-55, on 2.

extensive wartime mathematical work, tended in retrospect to mark the war off as a break or interruption unconnected with their peacetime studies.[40]

While the mathematical continuity between wartime and peacetime work is a matter for debate, then as now, historians have a much less equivocal view of the institutional consequences of the war. The war effort in several countries brought research councils, review boards, and other mechanisms of patronage and organization that complemented and in some ways reshaped scientists' and mathematicians' traditional system of universities and academies.[41] These new formations were often improvised, sometimes hastily and ineffectively, to meet new wartime needs. Aubin has emphasized, however, that they also often drew on long-standing military and governmental structures that, transfigured during the war, came to define a lasting approach to management, funding, and collaboration in a variety of fields.[42]

In retrospect, mathematicians (and not a few historians) tended to view the Great War as a break in international mathematics, one requiring some three decades to mend before mathematicians could return to their halcyon age of international comity. The interruption to Mittag-Leffler's never-interrupted series did, indeed, mark a turn toward national divisions in professional mathematics. The war and its associated geopolitics were not, however, just barriers that future generations of internationalists would have to break down or surmount. Wartime

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[40] Aubin and Goldstein, "Placing World War I," 6 *et passim*.

[41] See e.g. Aubin, Gispert, and Goldstein, "Total War"; Pietro Nastasi and Rossana Tazzioli, "Italian Mathematicians and the First World War: Intellectual Debates and Institutional Innovations," in Aubin and Goldstein, eds., *War of Guns and Mathematics*, 181-227; David Aubin, "'I'm Just a Mathematician': Why and How Mathematicians Collaborated with Military Ballisticians at Gâvre," in Aubin and Goldstein, eds., *War of Guns and Mathematics,* 307-349. Comparable institutional effects in the United States are discussed in Thomas Archibald, Della Dumbaugh, and Deborah Kent, "A Mobilized Community: Mathematicians in the United States during the First World War," in Aubin and Goldstein, eds., *War of Guns and Mathematics,* 229-271, on 255-268.

[42] E.g., Aubin, "Ballisticians at Gâvre," 329-337.

politics and practicalities returned mathematicians' attention to local and national institutions that they had never really left, notwithstanding their turn-of-the-century rhetoric. In these national and nationalist contexts, mathematicians found new sources of support, new forms of organization, and new approaches to domestic and international relations. Precisely these smaller-scale reconfigurations proved most important in the two decades following the Great War, in ways that have tended to obscure that period's significance in laying the ground for the apparently sudden postwar ascent of global mathematics. But none of that could have been foreseen as the Great War waned. Instead, mathematicians who had passed their war years in a variety of roles and offices saw an unsteadily international discipline with an uncertain future and nobody at the reins.

A Peace Apart

Mittag-Leffler had a clear idea who should lead mathematicians back to the path of international cooperation and peace. Sweden had remained formally neutral during the war, a position that won its national leaders few friends but also let them hope to emerge with their ties to those on both sides of the conflict relatively intact.[43] During the war, Mittag-Leffler labored to maintain international ties in mathematics, both through personal correspondence and through his journal *Acta Mathematica*. These sustained contacts, he believed, suggested "that mathematicians will be able one day to take over the direction when the time again comes to reestablish international

[43] See Sven Widmalm, "'A superior type of universal civilisation': Science as Politics in Sweden, 1917-1926," 65-89 and Robert Marc Friedman, "'Has the Swedish Academy of Sciences … seen nothing, heard nothing, and understood nothing?' The First World War, Biased Neutrality, and the Nobel Prizes in Science," 90-114, in Lettevall, Somsen, and Widmalm, eds., *Neutrality in Twentieth-Century Europe*.

scientific contacts."[44] At the same time, Mittag-Leffler repeatedly defended the Swedes' "right that was once awarded to us" to host the next International Congress of Mathematicians. Wartime speculation about alternative locations (including the United States before it entered the war) taught him that incumbency was no guarantee.[45] On Armistice Day, 11 November 1918, he resumed his advocacy for a Stockholm Congress with full force.[46]

Unfortunately for Mittag-Leffler, the institutional apparatus for postwar mathematics was in the hands of the war's victors, under whose auspices plans for restructuring peacetime science had begun even before the war's formal conclusion. Gaston Darboux, who in 1900 touted elbow-rubbing's value for disinterested mathematicians, began advocating as early as 1916 in his capacity as Permanent Secretary of the Academy of Sciences that the Allied nations set a course for resumed international cooperation. He died before this aim came to fruition in the form of the October 1918 Inter-Allied Conference on International Scientific Organizations in London. His successor as Permanent Secretary could not have been worse for those like Mittag-Leffler who would argue for a return to the prewar order: Émile Picard, a mathematician who during the war had come to see all things German as existential threats to all that was French and good.[47]

Picard was adamant that scientists from the former Central Powers be excluded from any peacetime international institutions. He worried, in particular, that representatives from neutral

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44 Mittag-Leffler to Fejér, 29 Nov 1917, Mittag-Leffler *Letterbook* (1915-1918), No. 6279, quoted in Joseph W. Dauben, "Mathematicians and World War I: The International Diplomacy of G. H. Hardy and Gösta Mittag-Leffler as Reflected in their Personal Correspondence," *Historia Mathematica* 7 (1980): 261-288, on 263.
45 Mittag-Leffler interview in *Aftonbladet*, 30 Aug 1916, quoted and translated in Turner and Sørensen, "Scandinavian Mathematics," 401, 408.
46 Mittag-Leffler to Nørlund, 11 Nov 1918, Institut Mittag-Leffler, referenced in Lehto, *Mathematics Without Borders,* 14.
47 Lehto, *Mathematics Without Borders*, 16; Daniel J. Kevles, "'Into Hostile Political Camps': The Reorganization of International Science in World War I," *Isis* 62, no. 1 (1971): 47-60, on 49; Riehm and Hoffman, *Turbulent Times,* 116-120 *et seq*.

nations might, if allowed too great a role during the early stages of peacetime reorganization, manage to pave the way for participation from the Central Powers.[48] Over a series of meetings beginning at the Inter-Allied Conference, Picard guided scientists from the Allied powers to form a new International Research Council that afforded limited representation to those from neutral countries and rigorously excluded those tainted, in Picard's view, by being on the wrong side of war, history, and civilization. These views would dominate negotiations behind the 1919 Treaty of Versailles. However, as Riehm and Hoffman note, mathematicians were leading indicators and proponents of the kinds of consequential claims later associated with Versailles and not (as many later claimed) helplessly swept up in the wake of score-settling generals and diplomats.[49] Along with sympathetic mathematicians principally from France and Belgium, particularly Belgian mathematician Charles de la Vallée Poussin, Picard also used this broader organizational process to take control of the International Congress of Mathematicians and to hold the first congress of his new era of international science in France.[50] And not just anywhere in France.

For the 200 mathematicians and 57 registered "family members" in attendance at the 1920 International Congress of Mathematicians, the just-finished war colored the entire

[48] Kevles, "Into Hostile Political Camps," 57-60.
[49] Riehm and Hoffman, *Turbulent Times,* 121. Mathematics was far from alone among disciplines with new or renewed international unions steeped in the politics of World War I. Cf. Nikolai Krementsov, *International Science between the World Wars: The Case of Genetics* (London: Routledge, 2005); Roger Fennell, *History of IUPAC, 1919-1987* (Oxford: Blackwell Science, 1994), chs. 2-3; Brigitte Schroeder-Gudehus, *Les Scientifiques et la Paix: La communauté scientifique internationale au cours des années 20* (Montreal: Les Presses de l'Université de Montréal, 1978); A.G. Cock, "Chauvinism and Internationalism in Science: The International Research Council, 1919-1926," *Notes and Records of the Royal Society of London* 37, no. 2 (1983): 249-288. On neutrality and science in this period, see Rebecka Lettevall, Geert Somsen, and Sven Widmalm, eds., *Neutrality in Twentieth-Century Europe: Intersections of Science, Culture, and Politics after the First World War* (New York: Routledge, 2012).
[50] Lehto, *Mathematics Without Borders*, 15-33.

proceedings.[51] The Société Mathématique de France hosted the Congress in Strasbourg, just "returned to this French motherland to which it is bound by its ancient origins and sympathies that always remained perennial across the wanderings of its history," as Picard was quick to remind the audience at the opening ceremony.[52] Invitations to the Congress were limited to "allies and friends," and in keeping with the IRC's new policies the hosts' German neighbors were among those formally excluded.[53]

Though the 1920 Congress was later remembered as a nadir for mathematicians' internationalism, comparing its demographics to those of preceding Congresses tells a somewhat different story. In raw numbers the drop in attendance from 574 scientific participants in 1912 to 200 in 1920 seems impressive, but the difference owed to just a handful of national delegations. The excluded former Central Powers accounted for 90 mathematicians at the 1912 Congress. The United States' contingent dropped from 60 to 11, though the more relevant comparison would be to their delegation of 16 to the last non-anglophone congress in Rome. The Russian cohort fell from 30 to 1, understandable for a nation mired in civil war, and the one participant was in fact an émigré resident in France.[54] Spanish numbers dropped from 25 to 10, and the passed-over Swedes from 12 to 1. Regarding the latter, Mittag-Leffler explained that "Since […] the next

[51] "Récapitulation par Nationalités," *1920 ICM Proceedings*, xv.
[52] Émile Picard, "Séance d'Ouverture du Congrès: Allocution de M. Émile Picard," *1920 ICM Proceedings*, xxvi-xxix, on xxvi.
[53] Lehto, *Mathematics Without Borders,* 33.
[54] Dimitri Riabouchinski is listed with an address in the Basses-Pyrénées but enumerated separately under "Russia" in the table of nationalities in the proceedings. *1920 ICM Proceedings,* xiii, xv. On Riabouchinski's role in interwar French fluid mechanics, see David Aubin, "'Audacity or Precision': The Paradoxes of Henri Villat's Fluid Mechanics in Interwar France," forthcoming, May 2010 preprint available on the author's website (accessed 2016).

(6th) International Congress is supposed to be held in Stockholm, it was doubtful if any Swede could have gone to Strassbourg."[55]

British delegates plummeted from 221 as the host nation to 9 as guests, an extreme drop-off compared to other former hosts but not an altogether surprising one. Prominent British mathematicians, particularly Godfrey Harold Hardy, maintained close ties to their German counterparts and reacted bitterly to the prospect of German exclusion.[56] As Picard wrangled together the new IRC, Hardy wrote to Mittag-Leffler to say "I should regard the loss of my personal relationships with German mathematicians as an irretrievable calamity."[57] In other forums, Hardy railed more explicitly against "the many imbecilities" of the IRC's exclusion policies, arguing that "The object of this council is not to promote international co-operation, but to exclude the Germans from it."[58] For years following the 1920 Congress, Hardy mustered British scientists and mathematicians to protest the IRC and demand an end to its membership proscriptions.[59]

Ignoring the officially excluded nations, however, the number of formally represented national groups in Strasbourg actually increased. Scottish physicist Alexander David Ross had

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[55] Mittag-Leffler to L. Königsberger, 20 Dec 1920, Mittag-Leffler *Letterbook* (1918-1921), No. 6861, quoted in Dauben, "Mathematicians and World War I," 274-275. Dauben notes that for months after the Strasbourg Congress (as late as March 1921) Mittag-Leffler preserved the hope that Picard's undertaking would be disqualified as the rightful heir to the prewar Congresses, and that Stockholm might still pick up where Cambridge had left off.

[56] E.g. Riehm and Hoffman, *Turbulent Times*, 121. On British mathematicians' pacifism during World War I, see Barrow-Green, "Cambridge Mathematicians," 69-78. Cf. Matthew Stanley, "'An Expedition to Heal the Wounds of War': The 1919 Eclipse and Eddington as Quaker Adventurer," *Isis* 94, no. 1 (2003): 57-89.

[57] Hardy to Mittag-Leffler, 7 Jan 1919, quoted in Dauben, "Mathematicians and World War I," 264.

[58] The "many imbecilities" quotation is from Hardy's just-cited 7 Jan 1919 letter to Mittag-Leffler. Hardy's IRC comments are in a letter published as G.H. Hardy, "The International Research Council," *Nature* 107, no. 2682 (24 Mar 1921): 107-108, quotation on 108.

[59] E.g. Lehto, *Mathematics Without Borders,* 34.

joined the faculty of the just-established University of Western Australia in Perth in 1912, and the Congress gave him an occasion to return briefly to Europe as Australia's first representative at an International Congress of Mathematicians.[60] Poland and Czechoslovakia, both newly independent after the war, each sent delegations. Indeed, the latter's twelve full members were matched only by the fourteen from Switzerland and the French host's eighty. Manila engineer Emilio Maria de Moreta represented the United States' colony of the Philippines as a distinct delegation.[61] In this respect, mathematicians treated the Philippines like British colonial India as a mathematically independent nation, without regard to its lack of political independence. Most of the smaller cohorts from the previous congresses held stable or grew slightly.

The host nation had about the same share of mathematicians in 1912 and 1920, while in other respects the 1920 Congress was at least as diverse as its predecessor. With a diminished presence from mathematical heavyweights excluded by fiat or by choice, those from emerging mathematical communities were more visible. Financing for the Congress came mostly from domestic sources, and Picard and his fellow organizers were especially successful at converting postwar national solidarity into monetary contributions. The Congress's list of sponsors ran to three full pages of the proceedings, led by municipal and government grants, followed by philanthropists including Ernest Solvay of Belgium and Edmond de Rothschild of France, and counting numerous companies (especially banks), professional societies, and interested individuals. Alsatian sponsors, both individual and corporate, were particularly numerous.

[60] On Ross, see D. E. Hutchison, "Ross, Alexander David (1883–1966)," *Australian Dictionary of Biography*, National Centre of Biography, Australian National University, 1988, http://adb.anu.edu.au/biography/ross-alexander-david-8270/text14487 (accessed 2016).
[61] That Moreta was an active engineer in the Philippines is confirmed in his 1923 testimony regarding an unfulfilled contract to build a chapel, Thomas A. Street, "G.R. No. 20586, October 13, 1923, Santiago Navarro, et al., plaintiffs-appellant, vs. Felix Mallari, et al., defendant-appellees," Republic of the Philippines, Supreme Court, Manila, http://www.lawphil.net/judjuris/juri1923/oct1923/gr_l-20586_1923.html (accessed 2016).

Picard welcomed the Strasbourg congregants by observing that “After the frightful torments of these last years, which have broken so many ties, rapprochement is necessary among scholars who esteem each other and who, without ulterior motives, have no other care than the disinterested cultivation of the truth.”[62] Congresses like these were especially useful, Picard claimed, for mathematicians who tended to isolate themselves in the far corners of their discipline, “buried in their symbols and lost in their abstractions.”[63] Here, they could learn the current status of big questions. They could affirm that “mathematic is not the strange and mysterious science that so many people depict it as; it is an essential piece in building natural philosophy.”[64] Indeed, “the history of science shows that it was foolish to affirm that such or such notion will not one day be used [in the study of physical phenomena].” Mathematics was a nourishing and multi-faceted discipline, or as Picard attributed to Joseph-Louis Lagrange, “mathematics are like the pig: all of it is good.”[65]

This theme returned with an important twist in the closing ceremony. If mathematics permeated the world, Picard stressed, now the world also permeated mathematics. After the Great War, few men of science could “isolate themselves in an ivory tower; though scholars, we remain men.”[66] As men, Picard insisted, mathematicians ought to recognize the conclusion of the recent conferences behind the International Research Council, that “nameless crimes will leave a

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[62] Picard, “Allocution [Ouverture],” xxvii-xviii.
[63] Ibid., xxviii.
[64] Ibid. While “mathematics” is usually plural in French, Picard here used the singular “la mathématique.” The singular/plural distinction would become a rhetorical centerpiece of the Bourbaki program of mathematical reform in and beyond France, discussed below.
[65] Picard, “Allocution [Ouverture],” xxviii. In view of Picard’s mixing of singular and plural mathematic(s), I have preserved his usage here at the expense of a more conventional English translation.
[66] Émile Picard, “Séance de Cloture du Congrès: Allocution de M. Émile Picard,” *1920 ICM Proceedings,* xxxi-xxxiii, on xxxii.

stain on culpable nations which signatures at the base of a peace treaty would not know how to wash. So we must abandon old international associations and create new ones."[67]

Picard followed this exhortation to create new associations not just with the International Research Council but with a new International Mathematical Union that adhered to the IRC's restrictive policies. At the close of the 1912 Cambridge Congress, G. H. Darwin countered calls for "an International Association of Mathematicians" with the observation that "our existing arrangements for periodical Congresses meet the requirements of the case better than would a permanent organisation of the kind suggested."[68] But the integrity of international mathematics (and German exclusion, in particular) could not be left to the whims of a new set of hosts every four years. As part of the 1919 Constitutive Assembly of the International Research Council, Picard's ally Charles de la Vallée Poussin led a session to draft statutes for the IMU, and became its first President after the statutes were confirmed at the Strasbourg Congress.[69] A new era of geopolitics, for Picard, required new forms of scientific organization, not unlike the new military alliances that had just succeeded in defending civilization itself.

Where Mittag-Leffler, Hardy, and others cast backward to advocate for an international order that resurrected a prewar golden age, for Picard such a view was untenable. German scholars may once have claimed a privileged position in international mathematics, but scholars remained men, and war indelibly stained those scholars' pasts *as men*. The golden age could only offer a template for a golden future if, in casting for continuity, one ignored a war that had changed everything. The Strasbourg hosts did not desire the mantle of prewar international mathematics, and by and large they did not assume it.

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[67] Ibid., xxxi-xxxii.
[68] *1912 ICM Proceedings,* I: 40.
[69] Lehto, *Mathematics Without Borders,* 24-27.

Mathematics With Borders

The Strasbourg Congress set the tone for the interwar period of international mathematics. Even as Picard and his allies' direct influence waned, Europe's mathematicians continued, in Strasbourg's wake, to grapple foremost with questions of nationalism and exclusion. While national communities had long been primary in mathematicians' institutional and intellectual infrastructure, the borders that divided them now became especially salient. As with the historiography of World War I itself, many have cast the interwar period as one where mathematicians' international projects failed or regressed. The leading example for this failure narrative is the fate of the International Mathematical Union, inaugurated at the 1920 Strasbourg meeting and formally dissolved at the 1936 ICM in Oslo after a resolution at the 1932 Zürich Congress "to re-study the question of the international collaboration in the sphere of mathematics and to make propositions with regard to its reorganisation at the next congress."[70]

Mathematicians founded a new International Mathematical Union in 1950, and its postwar importance has led mathematicians and historians alike to overstate the significance of its interwar predecessor's troubled tenure. But by most measures the interwar Union was little more than window-dressing, a lightning rod for those opposed to German exclusion and a vanity project for mathematicians from the former Allied powers to pretend to authority. A few high-profile mathematicians cared deeply about the old IMU and its perceived flaws at the time, but the mathematical rank-and-file as well as many elites mostly ignored it. As the IMU's most thorough chronicler has put it, "It is striking how few scientific activities the [interwar] Union

[70] A. Speiser, "Protokoll der Schlussitzung," *1932 ICM Proceedings*, 58-61, on 60-61. I have quoted the English translation from the *Proceedings*, which appears following more idiomatic statements in German, French, and Italian.

undertook."[71] In retrospect, detractors saw the interwar Union's failure as an inevitable consequence of its ill-starred birth in the politics of national exclusion, even though both the IRC and its official exclusion policies had both themselves perished well before the Union's 1936 dissolution.[72] If one sees the first IMU as the Strasbourg Congress's principal progeny, it is hard to avoid an entirely blighted view of interwar international mathematics.

Seen in another light, however, the Strasbourg meeting marked the continuity between the Great War's domestic transformations and those changes' interwar consolidation at national and regional scales into a foundation for more lasting international projects following World War II. Between the wars, European and North American mathematicians forged new and newly remunerative partnerships with domestic governments, philanthropies, and commercial interests. Capitalizing on these new sources of support, they developed new approaches to training, collaboration, and publication. Many of these features, from mathematicians' variety of financial sponsors to the scale, goals, and organization of interpersonal mathematics, could be seen in one form or another in Strasbourg. Visible as well, of course, were the deep national enmities and ambivalences that defined the terrain for these interwar formations.

This shift in historiographical perspective, from emphasizing interwar institutions' failures to excavating the period's consequential intellectual and infrastructural innovations and renovations, has been especially significant in scholarship on French mathematics.[73] Driven by

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[71] Lehto, *Mathematics Without Borders,* 33.
[72] After a similar motion was narrowly defeated in 1925, the IRC's member nations officially repealed its exclusion policy during an extraordinary assembly in 1926 and invited Germany to adhere. Policy revisions continued until 1931, when the IRC was re-constituted as the International Council of Scientific Unions. See Brigitte Schroeder-Gudehus, "Challenge to Transnational Loyalties: International Scientific Organizations after the First World War," *Science Studies* 3, no. 2 (1973): 93-118.
[73] For the latter historiography, see especially Juliette Leloup, *L'entre-deux-guerres mathématique à travers les thèses soutenues en France* (PhD Dissertation, Université Pierre et

memories and analyses from a later generation of mathematicians, the interwar French failure narrative took as its starting point the trauma of masses of young mathematicians from the École Normale Supérieure perishing at the front.[74] The missing generation of those young enough to die for France left a void of new leadership in the discipline. Outmoded prewar elites, the narrative went, thus retained control over the discipline longer and more completely than they otherwise would have, and accordingly failed to promote new curricula, theoretical innovations, or institutional structures.

Historians of French mathematics have recently begun to counter this failure narrative by reconsidering prominent interwar mathematicians' careers, projects, and circumstances, while also tracing the failure narrative's origins in French mathematicians' postwar revisionism. These historians have recast the interwar period as one of substantial growth and diversity for French mathematics in both Paris and the provinces, though the war's effects certainly remained pertinent both in images of the disciplinary community and in its work and organization. Juliette Leloup has documented this diversity with particular effect through a systematic analysis of interwar French doctorates in mathematics—whose topics and examiners alike belie the narrative of generational retrenchment. This diversity extended to younger mathematicians' engagement with foreign scholarship, with French authors routinely citing new developments from leading mathematicians across Western Europe (including Germany) in a variety of fields.

Others have challenged tales of disciplinary stasis, decline, or isolation using a variety of institutional and personal archives. These studies offer a potent counterpoint to histories overly

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Marie Curie, 2009); Liliane Beaulieu, ed., *Regards sur les mathématiques en France entre les deux guerres*, *Revue d'histoire des sciences* 62, no. 1 (2009).

[74] Leloup, *L'entre-deux-guerres mathématique,* 21-26, 36; Aubin and Goldstein, "Placing World War I," 7-10; Hélène Gispert and Juliette Leloup, "Des patrons des mathématiques en France dans l'entre-deux-guerres," in Beaulieu, ed., *Regards*, 39-117, on 40-41.

centered on the IMU and a select few disciplinary elites. To be sure, Picard and his allies continued to thwart other European mathematicians' attempts at international reconciliation through their compromised transnational organizations.[75] Outside the IRC and IMU, however, French mathematicians and their domestic and foreign sponsors forged significant new international ties, including new financial and institutional connections across the Atlantic.

Reinhard Siegund-Schultze has called attention to two aspects of modernization in interwar French mathematics, associated with the two different generations of French mathematicians that consolidated intellectual and institutional authority in that period.[76] The first involved established figures in the middle and older generations who used their prewar international relationships to secure the prestige and patronage necessary to build well-funded institutions with strong connections to mathematicians outside of France.[77] They had a willing partner in the Rockefeller Foundation, whose International Education Board led an effort after the Great War to chart, understand, and replicate the most successful aspects of European mathematics for the benefit of scientific communities on both sides of the Atlantic.[78] With financing from the IEB and a substantial matching grant from Edmond de Rothschild,

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[75] See Lehto, *Mathematics Without Borders,* 44-60. On Picard's control of interwar mathematical publishing in France, with some exceptions, see Gispert and Leloup, "Des patrons des mathématiques," 59-60.

[76] Reinhard Siegmund-Schultze, "The Institute [sic] Henri Poincaré and mathematics in France between the wars," in Beaulieu, ed., *Regards,* 247-283. This generation-based approach to intellectual and institutional history, shared by Gispert and Leloup ("Des patrons des mathématiques"), has parallels in other historiographies of early twentieth-century France, e.g. Jean-François Sirinelli, *Génération intellectuelle: Khâgneux et normaliens dans l'entre-deux-guerres* (Paris: Fayard, 1988).

[77] Gispert and Leloup offer a detailed analysis of these generations of interwar "patrons" of mathematics, including academicians, professors at elite institutions, and thesis examiners, in "Des patrons des mathématiques," op. cit.

[78] Reinhard Siegmund-Schultze, *Rockefeller and the Internationalization of Mathematics Between the Two World Wars: Documents and Studies for the Social History of Mathematics in the 20th Century* (Basel: Birkhäuser, 2001), esp. ch. 3-4.

establishment French mathematicians founded the Institut Henri Poincaré in Paris in 1928. The IEB's officers hoped that the new Institut would provide a stable base of support, including a library, offices, and meeting spaces, for the leaders of French mathematics, a role the Institut quickly assumed and continues to hold today. Importantly, the IEB funded the IHP as part of a parallel effort to support the mathematics profession in both France and Germany, with the famous institute in Göttingen the primary German beneficiary of the IEB's largess.[79] Thus, while the IEB and Rockefeller Foundation shared the IRC and Picard's focus on organizing mathematics on national lines, the American philanthropists concentrated within those nations on individual scientists and institutions and so sidestepped the latter's overt preoccupation with nation-scale competition.

In Siegmund-Schultze's account, the second aspect of interwar modernization in French mathematics came from a younger generation who used their older counterparts' institutional successes as a springboard for farther-reaching interventions in mathematical pedagogy, theory, and eventually ideology and culture. (Taking their formative institutional matrix for granted, mathematicians of that younger generation later tended to discount the institutional transformations that made their comparatively cosmopolitan training and ambitious disciplinary agendas possible.) This younger generation's most prominent members included an eccentric group of mathematicians who collaborated and published under the collective pseudonym of Nicolas Bourbaki.[80] The collaborators of Bourbaki, as they called themselves, came together

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[79] Ibid., ch. 5. Cf. Aubin, Gispert, and Goldstein, "Total War," 162, on French efforts "to build a strong, viable countermodel to Göttingen" on the heels of World War I.

[80] The definitive scholarly work on Bourbaki's early years remains Liliane Beaulieu's unpublished dissertation, *Bourbaki: Une histoire du groupe de mathématiciens français et de ses travaux (1934-1944)* (PhD Dissertation, Université de Montréal, 1989). On this formative period, see also Liliane Beaulieu, "A Parisian Café and Ten Proto-Bourbaki Meetings (1934-1935), *The Mathematical Intelligencer* 15, no. 1 (1993): 27-35. I here follow the Bourbaki mathematicians'

initially out of a dissatisfaction with the state of French textbooks in mathematical analysis, which seemed to them to lag in rigor and vision behind new works being published in German.[81] Indeed, with Rockefeller funding many founding Bourbaki collaborators visited leading German institutions in the 1920s and returned to France eager to introduce what they had learned abroad.[82] The group held regular "congresses" to debate and draft their collective writings and to explore current topics of research, and these congresses were also occasions to assess and induct new members (mostly but not all French) who shared the collaborators' convictions. The Bourbaki project rapidly expanded into a more comprehensive pedagogical and philosophical program concerned with the broader foundations of many areas of mathematics. Famously (or notoriously, depending on one's perspective), Bourbaki philosophy and ideology figured in such postwar contexts as French structural anthropology and American primary and secondary pedagogical reform, offering what David Aubin has called a "cultural connector" among a

usage in referring to those who collaborated on Bourbaki publications as "collaborators of Bourbaki" or "Bourbaki collaborators." Mathematicians and historians have referred to these mathematicians in a variety of ways, for instance as "members" of the "Bourbaki group," often downplaying the mathematicians' intentional provocations about authorship and identity. I have elsewhere developed this theme of Bourbaki's identity at mid-century, e.g. Michael J. Barany, "Whither the Immortality of Nicolas Bourbaki?" Princeton-Harvard-MIT Workshop on the History of the Physical Sciences, Harvard University, Cambridge, MA, 18 Apr 2015; Michael J. Barany, "Making a Name in Mid-century Mathematics: Individuals, institutions, and the American reaction to Nicolas Bourbaki," American Mathematical Society and Mathematical Association of America Joint Mathematics Meeting, Seattle, WA, 8 Jan 2016.

[81] See Leo Corry, "Writing the ultimate mathematical textbook: Nicolas Bourbaki's *Élements de mathématique*," in Eleanor Robson and Jacqueline Stedall, eds., *The Oxford Handbook of the History of Mathematics* (Oxford: Oxford University Press, 2009), 565-588.

[82] Catherine Goldstein, "La théorie des nombres en France dans l'entre-deux-guerres: De quelques effets de la première guerre mondiale," in Beaulieu, ed., *Regards,* 143-175, esp. 157-168; Beaulieu, *Bourbaki,* 65-105, esp. 69-73, 77-102.

variety of intellectual domains.[83] While Bourbaki's influence waned by the 1970s, a variety of formations initiated under his name—such as the Séminaire Bourbaki—persist to this day.[84]

While most of the Bourbaki group's best-known interventions in international mathematics took place in the first postwar decades, Paris's concentrated interwar pedagogical and research infrastructure (along with opportunities in some provincial universities) made it possible for the group to coalesce in such a way as to take swift and dramatic advantage of the new opportunities for international engagement that would follow the Second World War. This infrastructure included the first two prominent mathematical research seminars, those of Jacques Hadamard at the Collège de France (launched in 1913 and resumed in earnest after the war) and Gaston Julia at the Institut Henri Poincaré (begun in 1933).[85] As Anne-Sandrine Paumier has argued, their models of mathematical seminars would become, after the Second World War, the social and intellectual centerpieces of postwar French and international mathematics.[86]

The interwar mathematical institutions of other former Allied and neutral nations in Europe largely await the sort of detailed historical excavation and reconsideration now well underway for French mathematics. In England, Picard's rival Hardy held sway in the London Mathematical Society and urged the organization to pursue international cooperation in and beyond Europe to the extent its limited coffers allowed. Italian mathematicians figured

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[83] 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. 2 (1997): 297-342; Christopher J. Phillips, "In Accordance with a 'More Majestic Order': The New Math and the Nature of Mathematics at Midcentury," *Isis* 105, no. 3 (2014): 540-563.

[84] See the website of the Association des collaborateurs de Nicolas Bourbaki, http://www.bourbaki.ens.fr/.

[85] On Hadamard's and Julia's respective generational roles between the wars and on their seminars, see Gispert and Leloup, "Des patrons des mathématiques," 45-46, 53-54, 70-75, 88-89.

[86] Anne-Sandrine Paumier, "Le séminaire de mathématiques: un lieu d'échanges défini par ses acteurs. Incursion dans la vie collective des mathématiques autour de Laurent Schwartz (1915-2002)," *Philosophia Scientiæ* 19, no. 2 (2015): 171-193.

prominently in the interwar IMU and hosted the 1928 International Congress of Mathematicians in Bologna, but faded from the international scene amidst political and economic turmoil.[87] After Mittag-Leffler's death in 1927 his own Scandinavian community proved durable, if not the leading confederation he had hoped it would become. Mathematicians and mathematical physicists in the new Soviet Union, meanwhile, established thriving research schools and engaged in significant exchanges with Europe, and especially France, but sometimes struggled under domestic political and economic constraints.[88]

German mathematicians between the wars continued, despite the war's devastation and new challenges to their legitimacy from former Allies, to hold a crucial place in international mathematics, at least at first. Intellectually, 1920s Göttingen played host to a critical conceptual ferment culminating in what Leo Corry has identified as the structural image of algebra (and of mathematics as a whole) that would come to define the modern discipline—not just for the future Bourbaki collaborators who earned their stripes among Göttingen's mathematicians.[89] Göttingen and Berlin stood as leading centers, increasingly in open competition with each other, amidst a regional German-speaking research community that led the world in many areas of mathematics and theoretical physics. In broad terms, Göttingen mathematicians pursued and embraced international connections in the discipline, while Berlin mathematicians held more influence domestically and focused on German mathematics. These centers' preeminence insulated them

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[87] Aldo Brigaglia, "The First International Mathematical Community: The Circolo matematico di Palermo," in Parshall and Rice, eds., *Mathematics Unbound,* 179-200, on 195-198.
[88] Karl Hall, "The Schooling of Lev Landau: The European Context of Postrevolutionary Soviet Theoretical Physics," *Osiris* 23 (2008): 230-259; Loren Graham and Jean-Michel Kantor, *Naming Infinity: A True Story of Religious Mysticism and Mathematical Creativity* (Cambridge: Belknap, 2009). On interwar Soviet mathematics in relation to the Rockefeller fellowships and philanthropy, see Siegmund-Schultze, *Rockefeller,* 125-133.
[89] Leo Corry, *Modern Algebra and the Rise of Mathematical Structures*, 2nd edn. (Basel: Birkhäuser, 2004 [1996]), ch. 5.

from the full force of Picard and the IRC's scorn, ensuring both a robust domestic discipline and a regular stream of foreign correspondence, travel, and exchange. More concerned with international scientific prestige than with war guilt, officers of the Rockefeller foundation had few hesitations sending both money and young mathematicians to Germany (and especially Göttingen) in the 1920s and early 1930s.[90]

Even those from outside of Germany who detested every facet of German mathematics could not avoid the Germans' dominant position in interwar mathematical publishing.[91] Beyond German publishers' regular output of significant mathematical journals and monographs, the nation's mathematicians and publishers distinguished themselves as the unrivaled leaders in mathematical bibliography. First issued in 1871, the Berlin-based *Jahrbuch über die Fortschritte der Mathematik,* under the auspices of the Prussian Academy of Sciences, supplied mathematicians with somewhat German-centric syntheses of the annual mathematical literature beginning with 1868. While widely considered invaluable to working mathematicians as references, the *Jahrbuch* volumes were notorious for the long intervals between their publication and the initial appearance of the works they covered. Indeed, at the very first International Congress in 1897 Rudio had identified timely bibliography as "perhaps the most important" point of concern and initiative for international mathematics.[92]

Immediately following the Great War, amidst concerns over the concentration of bibliographic authority in German hands, American and British mathematicians led an unsuccessful effort to launch an alternative to the *Jahrbuch*.[93] During that same period, however,

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[90] Siegmund-Schultze, *Rockefeller,* ch. 4-5.
[91] Cf. Hepler-Smith, *Nominally Rational,* ch. 5.
[92] Rudio, "Über die Aufgaben," 35-36, 41-42.
[93] Reinhard Siegmund-Schultze, "'Scientific Control' in Mathematical Reviewing and German-U.S.-American Relations between the Two World Wars," *Historia Mathematica* 21 (1994): 306-

domestic changes to German mathematical publishing combined to provide, by 1931, the *Jahrbuch*'s first broadly successful competitor. Specifically, around the war's end Ferdinand Springer, Jr., grandson to the Springer publishing house's founder, began to expand the publisher's scientific offerings to include mathematics.[94] Cultivating a catalogue through the internationally-oriented Göttingen mathematics community, Springer found a profitable niche that continues to this day. As Siegmund-Schultze observes, abstracting journals like the *Jahrbuch* are expensive to produce, and have historically subsisted primarily through subsidies from scientific societies.[95] Springer, however, found that with a sufficiently wide audience such a journal could be worth the expense in advertising his firm's growing collection of publications with the extra imprimatur of scientific authority of a bibliographic enterprise edited by elite mathematicians themselves. Thus began, in 1931, the Göttingen-based *Zentralblatt für Mathematik und ihre Grenzgebiete*.

The *Zentralblatt* rapidly acquired an international profile, publishing reviews on a much quicker timescale than the *Jahrbuch* and covering non-German material more comprehensively. Just as rapidly, the new journal faced domestic challenges to its operation. The journal's principal opponent was Berlin mathematician Ludwig Bieberbach, a well-connected nationalist who loudly fulminated against perceived foreign threats to the glory of German science, including Picard and the IRC's very deliberate efforts to marginalize Bieberbach's mathematical

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329, on 310-317. My account below of the *Jahrbuch*'s interwar history is based primarily on Siegmund-Schultze's article.

[94] Mary H. Munroe, *The Academic Publishing Industry: A Story of Merger and Acquisition* (Association of Research Libraries, 2007), online at http://www.ulib.niu.edu/publishers/Springer.htm (accessed 2016).

[95] Siegmund-Schultze, "Scientific Control," 319.

community.[96] An ardent convert to the Nazi cause in 1933, Bieberbach and a number of like-minded mathematicians tried and failed to shut down the *Zentralblatt* by official action, but worked within new German race laws to exclude Jewish and foreign reviewers and editors from both the *Jahrbuch* and *Zentralblatt,* a process that disproportionately affected the *Zentralblatt*'s international editorial corps. For many editors, the last straw came with Italian Jewish mathematician Tullio Levi-Civita's 1938 suppression as a *Zentralblatt* editor, which prompted a mass resignation among the journal's chief editors.[97] Those resigning included Otto Neugebauer, the founding editor-in-chief, who had already by 1934 relocated his editorial operation to Copenhagen—with encouragement from his friend and leading Danish mathematician and internationalist Harald Bohr and with financial support from the Rockefeller Foundation—to avoid persecution for his opposition to the Nazis and past connections to the political left.[98]

The Strasbourg Congress and its policy of German exclusion inaugurated an interwar period of European mathematics where borders mattered more than ever. With the *Zentralblatt*'s collapse under the weight of German domestic politics and growing foreign concern over the Nazi regime's own quite different exclusion policies, interwar European mathematics hurtled

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[96] On Bieberbach's Naziism and its relation to mathematical modernism and counter-modernism, see Herbert Mehrtens, "Modernism vs. counter-modernism, nationalism vs. internationalism: style and politics in mathematics 1900-1950," in Goldstein, Gray, and Ritter, eds., *Mathematical Europe*, 517-529, on 519-520. On German scientists' balance of domestic politics and international ideals and institutions under the Nazis, cf. Philip Ball, *Serving the Reich: the struggle for the soul of physics under Hitler* (Chicago: University of Chicago Press, 2014).

[97] Richard Courant collected a range of resignation-related correspondence in Courant Papers, box 15, folder 9.

[98] In addition to Siegmund-Schultze, "Scientific Control," see Silke Göbel, "Glimpses into the history of Zentralblatt MATH," in Olaf Teschke, Bernd Wegner, and Dirk Werner, eds., *80 Years of Zentralblatt MATH: 80 Footprints of Distinguished Mathematicians in Zentralblatt* (Springer, 2012 [2011]), 1-16, on 4-5. On Springer's further troubles with establishment German mathematicians in this period, see Volker R. Remmert, "Mathematical Publishing in the Third Reich: Springer-Verlag and the Deutsche Mathematiker-Vereinigung," *The Mathematical Intelligencer* 22, no. 3 (2000): 22-30.

toward a rocky and disconcerted end with the next war's arrival. A new World War was no denouement, but by returning the world's mathematicians to the national cloisters of the previous war it brought full circle an era where national concerns prevailed even over the most international-seeming of projects. But European mathematicians found more lasting institutional triumphs closer to home. Long overshadowed by the period's international discord, these national and regional endeavors helped mathematicians adapt to changing institutional and political circumstances and laid the groundwork for a renewed postwar internationalism.

A Bid Deferred

Interwar mathematics started with one German exclusion and ended with another. A different pair of bookends, related in several ways to those of German exclusion, would be just as consequential. In 1920 and 1936, American mathematicians set out to host International Congresses of Mathematicians in 1924 and 1940, respectively. Both bids went unfulfilled as Europe thrashed in political and then military conflict. Both represented an American promise to lead international mathematics, dashed by the discipline's American and European geopolitics. The Americans' path from the first failure to the second, however, shows how much had changed between 1920 and 1940, and how the Americans' interwar transformations became, in the balance of time, the most decisive ones for postwar mathematics.

From the start, American mathematicians participated in the successive International Congresses of Mathematicians in numbers disproportionate to their relatively minor mathematical prestige and substantial geographic marginality. Where most nations' attendance was a tidy function of their linguistic and geographic proximity to the Congress's host and the size of their domestic mathematical elite, the transatlantic tourists' numbers only grew. The

United States accounted for 7 participants in 1897, 19 in 1900 and again in 1904, 27 in 1908, and 87 in 1912, when they made up the largest foreign delegation at the first International Congress with an anglophone host. Founded in 1888, the American Mathematical Society seemed poised, by the Great War's arrival, for greater things.[99]

The United States' late but decisive military intervention in that war positioned American scientists in several fields to help shape a victors' peace for international science. Picard's French delegation claimed the first spoils, but the Americans were not far behind. At a special meeting held during the Strasbourg Congress to ratify statutes for the new International Mathematical Union, the University of Chicago's L. E. Dickson and Princeton University's Luther Eisenhart proposed to host the 1924 congress in New York, apparently without having formally consulted the American Mathematical Society.[100]

Their bid was a risky gambit. Without the requisite guarantees from their counterparts back home, Dickson and Eisenhart could guarantee neither the financial nor the political wherewithal necessary to host such a Congress. Both were considerable hurdles. Politically, the 1924 Congress inherited Strasbourg's divisive questions over exclusion, with Americans by and large leaning against the IRC's official position. Financially, European mathematicians would

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[99] On American mathematics leading up to the war, see especially Karen H. Parshall and David E. Rowe, *The Emergence of the American Mathematical Research Community, 1876-1900: J. J. Sylvester, Felix Klein, and E. H. Moore* (Providence: American Mathematical Society, 1994); Archibald, Dumbaugh, and Kent, "A Mobilized Community."

[100] Gabriel Koenigs, "Rapport de M. G. Koenigs, Secrétaire Général du Congrès," *1920 ICM Proceedings*, xxxiv-xxxix, on xxxv. After New York, Belgium was decided "by common agreement" to host the 1928 Congress. See Raymond C. Archibald, *A Semicentennial History of the American Mathematical Society, 1888-1938* (New York: American Mathematical Society, 1938), 19; Lehto, *Mathematics Without Borders,* 33-34. On Dickson, see Della Dumbaugh Fenster, "American Initiatives Toward Internationalization: The Case of Leonard Dickson," in Parshall and Rice, eds., *Mathematics Unbound,* 311-334. As the IMU had just been ratified at that same meeting, there was no standing official procedure for a nation or national organization to bid for an ICM.

find it difficult to cross the Atlantic in great numbers, even if (as was far from assured) a large share of leading American institutions could support visiting Europeans on lecture invitations timed to coincide with the big event.

Dickson discussed both questions with Hardy on a visit to Oxford following the Strasbourg Congress. On the political question, Hardy later relayed to Mittag-Leffler what he took, from Dickson, to be "the general American attitude." To wit, "They hope that, by 1924, the French attitude will have changed enough for the proposed Congress to be made genuinely international; but they were not prepared to raise the point definitely at Strassbourg [sic]."[101] Though Dickson and his colleagues were formally bound by the IMU and IRC's framework (which had, after all, been endorsed at the very same meeting where the Americans presented their invitation), with the language of "genuinely international" they telegraphed their sympathy with those like Hardy and Mittag-Leffler who saw the Strasbourg Congress as an illegitimate heir to the prewar series of international gatherings. Picard's internationalism, the phrase suggested, was somehow incomplete or insincere. Where the French saw internationalism in terms of combined enterprise among nations, such as the Allied internationalism that had won the war, opponents of German exclusion countered that internationalism could be as much about who was left out as it was about how many nations were counted in.

The political and financial questions quickly merged. As one American told it a decade and a half later, "By 1922 it was clear that financial backing was unobtainable in the United States, with the restrictions imposed by the IMU."[102] Dickson continued to assert in 1922 that "it is desirable to have a truly international congress," and American Mathematical Society

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[101] Hardy to Mittag-Leffler, 30 Sep 1921, excerpted in Dauben, "Mathematicians and World War I," 276.
[102] Archibald, *A Semicentennial History,* 19.

Secretary Roland G.D. Richardson avowed that the Americans "should hope later on to have a congress really international in character."[103] But barring a solution that would satisfy both the IMU and the defenders of this revisionist internationalism the Americans already had a fallback plan.

John Charles Fields, one of the few internationally well-connected Canadian mathematicians of the period, organized the December 1921 meeting of the American Association for the Advancement of Science in Toronto. In the evenings following the formal scientific program, American mathematicians grumbled and commiserated about their forthcoming quagmire of a Congress. Fields later claimed to have heard a suggestion, perhaps in jest, that the Americans could bypass their troubles by ceding the meeting to Toronto. Lacking Dickson's compunctions over German exclusion and eager to capitalize on French solidarity with a one-third francophone country, Fields ran with the idea. A consummate organizer, he formalized the proposal and secured initial financial commitments by the end of 1922, at which point the Congress's relocation seemed assured.[104]

The 1924 Toronto Congress lived up to its origins in compromise and controversy.[105] Owing to unresolved difficulties with transatlantic travel, for the first time the *Proceedings* enumerated 82 corresponding members in addition to the 444 members and delegates in attendance.[106] The hosts accounted for nearly a quarter of the members present, and another two in five came from the United States, whose mathematicians had to decide how far to carry the same objections to German exclusion that had foreclosed their own attempt to play host. Two

[103] W.E. Tisdale (attributing quotation to Dickson) to R.G.D. Richardson, 24 Nov 1922, and R.G.D. Richardson to O. Veblen, 8 Nov 1922, AMS Records, box 21, folder 83, in Riehm and Hoffman, *Turbulent Times,* 131, 133-134.
[104] Riehm and Hoffman, *Turbulent Times,* 81, 129-137.
[105] Lehto, *Mathematics Without Borders,* 34-37; Riehm and Hoffman, *Turbulent Times,* 137-161.
[106] "Geographical Distribution," *1924 ICM Proceedings*, I: 48.

fifths of the remainder hailed from Britain, some of whose members may also have come for the collocated meeting of the British Association for the Advancement of Science. Two dozen mathematicians from France and another eleven from Italy rounded out the double-digit national delegations. Despite these limitations, Fields managed to run a surplus of some $2,700 for the Congress, which he set aside to fund a medal, posthumously named after Fields himself and first awarded in 1936, that remains one of the most prestigious honors awarded at the International Congresses of Mathematicians.[107]

The Toronto Congress ended without a designated successor, with delegates instead granting the IMU's Executive Committee two years to select a host. The Belgians, anointed in Strasbourg as incumbents for 1928, were the first of the three main contenders to withdraw. The remaining two were Mittag-Leffler's Stockholm and Bologna, backed by the newly-elected IMU President Salvatore Pincherle. When French IMU officials raised concerns over an unfavorable Swedish exchange rate, Bologna became the presumptive site. As consolation, Pincherle offered Mittag-Leffler the distinction of presiding over "the first truly international congress of mathematicians" since the war, but the latter died before he could take up the honor.[108]

Discounting the internationalisms of Strasbourg and Toronto, Pincherle demonstrated in 1926 that even within the IMU attitudes toward exclusion had shifted. At the start of that year, the American Mathematical Society added its voice to a growing chorus calling for an official end to the IRC's exclusion policy. Its resolution threatened the Society's withdrawal from the

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[107] See Riehm and Hoffman, *Turbulent Times,* 179-187; H. S. Tropp, "The Origins and History of the Fields Medal," *Historia Mathematica* 3 (1976): 167-181; C. Riehm, "The Early History of the Fields Medal," *Notices of the American Mathematical Society* 49, no. 7 (2002): 778-782; Michael J. Barany, "The Myth and the Medal," *Notices of the American Mathematical Society* 62, no. 1 (2015): 15-20.

[108] Pincherle to Mittag-Leffler, 20 July 1926, Institut Mittag-Leffler Archives, quoted in Lehto, *Mathematics Without Borders,* 44.

IRC if the policy were not abolished "so that membership in the Union may be entirely international."[109] "Entirely" here joined "genuinely" and "truly" among the Americans' emphatic adverbs for an internationalism capacious enough for German participation. (The exhortation to be genuinely or truly international, of course, had other uses beyond nucleating opposition to IRC exclusion policies. A decade later, German mathematician Helmut Hasse implored American Marshall Stone to be "truly impartial and hence genuinely international" rather than taking the one-sided position of supporting Jewish mathematicians dismissed from the *Zentralblatt*.[110])

The IRC relented, but German scientific societies were not impressed. This left the organizers of the 1928 Bologna Congress at an impasse. While Germans were no longer officially excluded, neither were they party to the IRC or IMU. The IMU's Secretary General, French mathematician Gabriel Koenigs, joined Picard in ruling that only those from nations party to the IMU and IRC could legitimately participate in an ICM. Pincherle disagreed and invited German mathematicians on behalf of the Congress but without the IMU's imprimatur. This, according to Lehto, marked the Congresses' *de facto* separation from the interwar Union, a distinction that became official over the course of the next two meetings.[111]

Italy's 336 mathematicians hosted 500 from abroad. German mathematicians, welcome once more in some quarters at least, formed the largest foreign delegation at the 1928 ICM, albeit not so overwhelmingly as they had at the Rome Congress twenty years prior.[112] France, the

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[109] American Mathematical Society resolution of 1 Jan 1926, quoted in Lehto, *Mathematics Without Borders*, 39.
[110] Hasse to Stone, 15 Mar 1939, quoted in Reinhard Siegmund-Schultze, "The Effects of Nazi Rule on the International Participation of German Mathematicians: An Overview and Two Case Studies," in Parshall and Rice, eds., *Mathematics Unbound*, 335-351, on 341.
[111] Lehto, *Mathematics Without Borders,* 44-50.
[112] "Distribuzione dei Congressisti per Nazioni," *1928 ICM Proceedings*, I: 63.

United States, and England each contributed around a tenth of the foreign total, compared to Germany's 15%. The remaining members composed what was far and away the most nationally diverse Congress to date, with significant participation from across Europe and visitors from as far away as Japan, Argentina, and Palestine. Legendary German mathematician David Hilbert is said to have received a standing ovation at the opening ceremony, and to have remarked that "all limits, especially national ones, are contrary to the nature of mathematics, [… for which] the whole cultural world is a single country."[113]

A Shifting Center

The Americans' first failure to host an interwar ICM was an embarrassing setback for a mathematical community aspiring to lead on equal terms with Europe's traditional mathematical powers. By the time their second opportunity arrived, the entire landscape had shifted. After an initial period of reinforcing domestic institutions, American elites began to refashion themselves as the privileged guardians of international mathematics.[114] The continent's continued turmoil, in many Americans' view, exposed its institutions as insufficiently reliable for supporting the discipline's international future. From their universities and institutes to their publications and fellowship programs, American mathematicians and their sponsors moved to build domestic replacements for their compromised European counterparts.

Like their counterparts in Europe, American mathematicians built on their experiences of the Great War to pursue potentially remunerative ties to government and to industries (such as

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[113] Constance Reid, *Hilbert* (Berlin: Springer, 1970), 188.
[114] See, e.g., Karen Hunger Parshall, "'A New Era in the Development of Our Science': The American Mathematical Research Community, 1920-1950," in David E. Rowe and Wann-Sheng Horng, eds., *A Delicate Balance: Global Perspectives on Innovation and Tradition in the History of Mathematics, a Festschrift in Honor of Joseph W. Dauben* (Basel: Birkhäuser, 2015), 275-308.

insurance and telecommunications) that could eventually become stable sources of employment and research funding.[115] As part of a coordinated effort to offer a domestic counterweight to European, and especially German, publishing infrastructures, the American Mathematical Society's officers used such new financial connections to expand the number, size, and circulation of its journals.[116]

Beginning in the 1930s, United States mathematicians positioned their departments and institutes as replacements for their European counterparts and as a safe haven for colleagues facing unstable European circumstances. A coalition of philanthropists and academic reformers founded the Institute for Advanced Study in Princeton, New Jersey, with the explicit aim to fill a role in advanced training and cultural authority that had been played before the Great War by German institutions.[117] The war shattered the Americans' sense of those German institutions' reliability, and the ensuing decade gave them little cause for optimism. With the rise of Hitler and the Nazi party, the Americans' approach to replacing the once-dominant German system evolved from prudential and precautionary to urgent and reactive.

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[115] Loren Butler Feffer, "Oswald Veblen and the Capitalization of American Mathematics: Raising Money for Research, 1923-1928," *Isis* 89 (1998):474-497; Feffer, "Mathematical Physics and the Planning of American Mathematics: Ideology and Institutions," *Historia Mathematica* 24 (1997): 66-85; Parshall, "A New Era," 275-284.

[116] Reinhard Siegmund-Schultze, "The Emancipation of Mathematical Research Publishing in the United States from German Dominance (1878-1945)," *Historia Mathematica* 24 (1997): 135-166.

[117] E.g. Flexner to Keppel, 31 Oct 1936 and 22 Jan 1937, IAS General Files, box 12, "Carnegie Corporation" folder. See Karen H. Parshall, "Marshall Stone and the Internationalization of the American Mathematical Research Community," *Bulletin of the American Mathematical Society* 46, no. 3 (2009): 459-482, on 467; Parshall and Rowe, *Emergence*, ch. 4, 5, 10. As many historians have documented, such transatlantic (especially U.S.-German) ties were common across a wide range of disciplines, from history to physics. On the longer effort to establish Princeton (along with the IAS) as a mathematical center, see William Aspray, "The Emergence of Princeton as a World Center for Mathematical Research, 1896-1939," in William Aspray and Philip Kitcher, eds., *History and Philosophy of Modern Mathematics* (Minneapolis: University of Minnesota Press, 1988), 346-366.

This sudden shift was starkly visible in the Rockefeller Foundation's operations. Where once the foundation lavished resources on European institutions, around 1933 its program officers abruptly shifted their focus to identifying and relocating scientists threatened by developments in and beyond Germany.[118] In addition to facilitating diplomatic connections, an American-led network of foundations and institutions worked to find refugee mathematicians temporary appointments in the United States or to subsidize their employment in more hospitable European institutions. Transatlantic relocations, however, were both controversial and logistically difficult, and American mathematicians struggled to balance their limited resources before the American entry into World War II decisively altered American mathematicians' material circumstances.[119]

Hermann Weyl, a long-time professor at the ETH in Zürich who took a prestigious chair in mathematics at Göttingen in 1930 and then abruptly left in 1933 to join the new Institute for Advanced Study, exemplified the kind of political and ideological whiplash underlying American reactions to Nazism in the mid-1930s. As head of the German delegation to the 1932 ICM in Zürich, Weyl observed that the Congress's return to its original 1897 host furnished the opportunity to begin anew with a pledge: "we should never again betray one another, as man or as mathematician, for the sake of a political struggle."[120] Half a decade later, the American Mathematical Society debated how to respond to an invitation to participate in bicentennial celebrations for a Göttingen University now thoroughly transformed within the Nazi state.

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[118] Siegmund-Schultze, *Rockefeller,* ch. 6.

[119] Nathan Reingold, "Refugee Mathematicians in the United States of America, 1933-1941: Reception and Reaction," *Annals of Science* 38 (1981): 313-338.

[120] "Wir wollen um des politischen Kampfes willen einander nie wieder als Menschen und als Mathematiker verraten." Hermann Weyl, "Ansprache von Herrn Prof. Weyl, Präsident und Deligierter der deutschen Mathematiker-Vereinigung, Göttingen," *1932 ICM Proceedings,* I: 71-74, on 72.

Recalling his 1932 Zürich remarks, Weyl wrote to AMS Secretary Richardson that "I understand perfectly well that the American mathematicians are anxious to preserve such academic connections of old standing without regard to 'politics',- that incurable mother of all nuisance" but that "I have also experienced the complete political Gleichschaltung of the German universities in the summer of 1933" and so believed the Göttingen celebration "will be a political rather than an academic demonstration."[121] Weyl urged a variant on what would become Richardson's recommendation, that the Society acknowledge the university's past but refrain from endorsing its present by sending a representative. In a memorandum to the AMS, Richardson paraphrased Weyl that "in his speech in Zurich he made a plea to the mathematicians that they never again allow internationalism [sic[122]] to interfere with mathematical fellowship. He admits that circumstances may, however, alter cases."[123]

While most mathematicians who weighed in on the American Mathematical Society's invitation shared Weyl's view of the German situation, if not his first-hand experience of it, others were less willing to condemn the new regime. "It would be an error for the Society to take a position on either side of the political and racial struggle now going on in Germany," wrote

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[121] Weyl to Richardson, 12 Apr 1937, AMS Records, box 26, folder 50. Gleichshaltung here translates to "bringing into line," and was a term of art under the Nazi regime for consolidating authority across German society.

[122] Referring to a conflict between *inter*nationalism and mathematical fellowship, Richardson may have had in mind Picard's divisive use of international institutions, corresponding to what Weyl termed "political struggle" in his 1932 address. That is, internationalism—with its *political* focus on nations and institutions in contact and conflict—interfered with mathematical fellowship as a *personal* phenomenon shared among individuals who might happen to be from different nations.

[123] R. G. D. Richardson, "Memorandum on the Göttingen Invitation," AMS Records, box 26, folder 55. Weyl objected to Richardson's paraphrase, adding "what I meant to say was that non-participation is not a breach of mathematical fellowship, and will be felt by most of our German colleagues as moral support rather than a snub." Weyl to Richardson, 10 May 1937, AMS Records, box 26, folder 55; see also a somewhat apologetic Richardson to Weyl, 9 Jun 1937, in the same folder.

City College of New York professor Bennington Gill. He added "And one must keep in mind the duty of the Society to uphold the doctrine of the transcendence of the pursuit of mathematical truth over barriers of nationality and race. The Society cannot celebrate DEUTSCHE Mathematik, though it would welcome an opportunity to celebrate deutsche MATHEMATIK!"[124]

Rather than Weyl's hoped-for opportunity to reset international relations, the 1932 Zürich Congress amounted to a last interwar gasp for disciplinary comity before the clouds of Nazism and Fascism undeniably rolled in. The 1936 Oslo Congress realized Mittag-Leffler's ambition of a Scandinavian ICM at face value, but without anything resembling his irenic vision for boundless cooperation among the world's mathematicians. The Italian government barred its mathematicians from attending and the Soviets sharply limited attendance and did not send an official delegation. Foreign currency shortages limited German participation, and its delegation's rapporteur was forced to acknowledge that some of the most prominent German names on the program belonged to political emigrants.[125] Though the combined Scandinavian delegation exceeded any individual nation's cohort, political and economic turmoil affecting the longstanding hubs of European mathematics meant that the Americans boasted the largest individual delegation with 86 participants and an additional 56 guests.[126]

The Americans' first bid to host an ICM came amidst international crisis, and so did the second. Luther Eisenhart, one of the agents behind the bid for 1924, came to Oslo having learned his lesson. This time, he chaired a committee of prominent American mathematicians, including

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[124] Gill to Richardson, 9 Apr 1937, AMS Records, box 26, folder 48.
[125] Siegmund-Schultze, "Nazi Rule," 342-244; see also Lehto, *Mathematics Without Borders,* 69-71; *1936 ICM Proceedings*, I: 39.
[126] *1936 ICM Proceedings,* I: 39. A delegation's guests tended to index economic conditions affecting that nation's leading mathematicians, as well as the difficulty of travel and the destination's desirability for that nation's tourists.

his Princeton colleague Solomon Lefschetz and two leaders of the new mathematics faculty at the nearby Institute for Advanced Study, Oswald Veblen and Marston Morse.[127] Eisenhart stressed at the closing ceremony that "This invitation is presented by the official delegates of the [American Mathematical] Society in accordance with action taken by the Council of the Society."[128] Earlier in the closing session, the assembled mathematicians formally dissolved the IMU, casting aside what had been another barrier for the Americans' 1924 attempt.[129] With his invitation accepted by acclamation, Eisenhart returned to the United States and immediately constituted a fifteen-person Committee on the International Congress of Mathematicians to devise a robust blueprint for the upcoming meeting. The committee focused most of its effort on designing a fair and comprehensive system for issuing prestigious invitations for plenary lectures, arranging thematic conferences on different branches of mathematics, and soliciting attendance from foreign mathematicians. Eisenhart submitted his committee's official plan to the council of the American Mathematical Society on March 13, 1937, and the council adopted it with a few amendments.[130]

As with the Americans' previous attempt, securing the assent of the international mathematics community to host such a Congress was no guarantee of the institutional and financial wherewithal to do so in practice. The task fell principally to the chair of the 1940 Congress's Financial Committee, the accomplished Harvard-trained analyst Marston Morse, who had joined the faculty of the Institute for Advanced Study in 1935, just before joining Eisenhart in Oslo to present the Americans' bid. Morse was to use his substantial professional connections

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[127] See Aspray, "The Emergence of Princeton," 357-359.
[128] *1936 ICM Proceedings*, I: 49.
[129] Ibid., I: 46-47.
[130] Report as amended by the American Mathematical Society in Morse Papers, box 7, "ICM – Analysis" folder. Annotated versions from March are in AMS Records, box 26, folder 71.

and intellectual and institutional prestige—from Harvard, the IAS, and beyond—to secure the Eisenhart committee's projected budget of $23,000.[131]

The Eisenhart committee anticipated the greatest expense to be preparing and printing proceedings, at a cost of $10 per printed page and an estimated 800 pages in sum (though the committee hoped for 1,000 pages should funds permit). They estimated an equal outlay for the costs of hosting an estimated 300 foreign mathematicians as guests (as well as entertaining all those in attendance), with a hope of being able to contribute toward travel expenses from further donations. A final category of expenses, only slightly smaller than the first two, encompassed the considerable expected cost of reaching foreign mathematicians and coordinating the event.[132]

Eisenhart's committee expected that the Congress's estimated 600 participants would be able to support $10 each in dues, with another $5 each from an estimated 200 accompanying "associate members." Library sales would make a small dent in the *Proceedings'* cost, and a pledge of $7,500 from the Carnegie Corporation underwrote the remaining costs of publication. This left a $7,500 hole in the balance sheet, which is why on April 15, 1937, Morse found his way to the office of the Rockefeller Foundation's Warren Weaver, cap in hand.[133] Trained as a mathematician, Weaver would become best known for directing the Rockefeller Foundation's tremendous resources to reshape the field of experimental biology, including the area of

[131] In terms of buying power, this represents about $380,000 in 2016 dollars. From http://www.bls.gov/data/inflation_calculator.htm.
[132] For a more detailed examination of the Congress's initial plans, from which this discussion is adapted, see Michael J. Barany, "Remunerative Combinatorics: Mathematicians and their Sponsors in the Mid-Twentieth Century," in Brendan Larvor, ed., *Mathematical Cultures: The London Meetings 2012-2014* (Basel: Birkhäuser, 2016), 329-346.
[133] Their exchange is documented in Warren Weaver's officer diary, excerpted in RF Projects: U.S., box 125, folder 1545.

molecular biology whose name he coined.[134] The Rockefeller Foundation's interventions in international mathematics predated Weaver's 1932 arrival as director of its Division of Natural Sciences, but from 1932 into the early postwar years his targeted financial interventions shaped nearly every aspect of American's emerging leadership in what soon became a global discipline.

Despite some tentative successes beginning in the 1920s at securing corporate patronage, American mathematicians relied heavily on their academic institutions for day-to-day support and on large philanthropies—especially the Carnegie Corporation and Rockefeller Foundation—for any special or discipline-wide endeavors. Routine funding could, through well-timed lecture invitations and short-term appointments, be converted to support exceptional undertakings, but the ICM's organizers were in no position to rely on these sorts of tricks for their core operations. Mathematics, Morse pled at his meeting with Weaver, was in "the unique position of … having no natural sources of support," and Weaver agreed.[135] The Rockefeller Foundation soon joined the Carnegie Corporation as a leading benefactor for the 1940 Congress.

Events in Europe, however, meant that their pledges would have to wait. On the heels of the German invasion of Poland, the council of the American Mathematical Society suspended planning for the 1940 Congress and appointed Morse to chair a new Emergency Committee for the Congress whose first tasks included convincing donors to allow the American Mathematical Society to keep their contributions in trust until conditions permitted a renewed effort.[136]

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[134] Mina Rees, "Warren Weaver, 1894-1978," *Biographical Memoirs of the National Academy of Sciences* (Washington: National Academy of Sciences, 1987); Lily E. Kay, *The Molecular Vision of Life: Caltech, the Rockefeller Foundation, and the Rise of the New Biology* (New York: Oxford University Press, 1993).

[135] Weaver's agreement is evident in his formal appropriation request to the Foundation for its November docket, which echoes Morse's claim nearly verbatim. RF Projects: U.S., box 125, folder 1545.

[136] Minutes of September 6, 1939, AMS Records, box 15, folder 17.

The war's onset also accompanied what would ultimately be a much larger undertaking initially underwritten by the Carnegie Corporation and Rockefeller Foundation, the launch of an American substitute for the *Zentralblatt* under the title *Mathematical Reviews*.[137] More than the International Congresses, which the Americans had learned the hard way not to regard as settled achievements before they came to pass, American mathematicians indexed their international status in the realm of publication. Their inability to supplant that *Jahrbuch* in the 1920s was evidence, for many, of a national community that continued to lack a sure footing. Following the 1938 crisis at the *Zentralblatt* and a series of domestic debates over the viability and propriety of creating an American replacement, the Americans arranged for the *Zentralblatt*'s founding editor-in-chief Otto Neugebauer once again to create a leading international abstracting journal, this time on American shores and with an American bankroll.[138] As the prospect of a new abstracting journal grew, American mathematicians asserted with brimming confidence that this was finally their moment on the international stage.

In a December 1938 memorandum, AMS Secretary Richardson observed that "a majority of the referees for the Zentralblatt are residents of North America […] not only because many of the Europeans have moved over here, but also because the obviously best material from which to select referees is to be found growing up on this side of the Atlantic." This was evidence, Richardson claimed, that "the center of gravity of mathematics has obviously moved into this

[137] See esp. Siegmund-Schultze, "Scientific Control," 321-324. On the effort's philanthropic financing, see the grant reports in AMS Records, box 15, folders 33 and 49. A July 1940 report to the Carnegie Corporation in box 15, folder 33, observed "In view of the uncertain political future of the world it is not possible to extract from this record any very clear prediction of the financial future."

[138] The goal to re-create the *Zentralblatt* was explicit in the Americans' discussions. Indeed, R. G. D. Richardson proposed directly to Ferdinand Springer that rather than re-found the journal it might be possible simply to transfer editorial control of the existing *Zentralblatt* to the Americans. See Richardson to Springer, 5 Dec 1938, AMS Records, box 26, folder 124.

country."[139] Warren Weaver, who freely appropriated his petitioners' colorful formulations (including Morse's claim above about "natural sources of support"), offered a memorandum of his own for the Rockefeller Foundation dated 18 May, 1939, asserting that "During the last twenty years the center of gravity of world-wide activity in mathematics has definitely moved from Europe to the United States" and that the journal would respond "to the present challenge to sponsor and protect on this continent certain cultural interests which have previously been located in Europe."[140] In a similar vein, Marshall Stone wrote on 2 May, 1939, that Americans' efforts to launch *Mathematical Reviews* reflected "the desire to assume for the future of mathematical abstracting a responsibility commensurate with America's great and growing mathematical importance."[141] As institutions strained and crumbled in Europe, Americans positioned themselves to fill the void.

A War of Mathematics

Across Europe and the United States, the Second World War affected mathematics teaching, training, and research at nearly every level, transforming institutions as it went. Many of those active at the time remarked on the field-changing transformations the war had wrought for mathematicians' research, organization, and funding.[142] These included changes to existing channels of revenue from states, academic institutions and philanthropies, as well as the advent

[139] R. G. D. Richardson, "Memorandum Regarding a Mathematical Abstracting Journal," December 1938, in RF Projects: U.S., box 125, folder 1549. Richardson recycled his "center of gravity" language in later missives, e.g. a 25 Apr 1939 "Dear Colleagues" letter quoted in Parshall, "A New Era," 295.
[140] Warren Weaver, "International Mathematical Review Journal," 18 May 1939, RF Projects: U.S., box 126, folder 1551.
[141] Stone to Hasse, 2 May 1939, quoted in Reingold, "Refugee Mathematicians," 331-332.
[142] E.g., in the United States, Marshall H. Stone, "Science and Statecraft," *Science* 105, no. 2733 (16 May 1947): 507-510; Mina Rees, "The Mathematical Sciences and World War II," *The American Mathematical Monthly* 87, no. 8 (1980): 607-621.

of sponsors largely or wholly new to mathematicians. Postwar mathematics differed from its interwar predecessor in the scope and scale of participation and activity, both in its traditional centers and on its widening periphery. This, in turn, reconfigured the problems, methods, and means of communication mathematicians shared and developed.

In a front-page article in November, 1941, on "Chemists and the National Defense" in *Chemical and Engineering News*, Harvard President and chairman of the National Defense Research Committee James Bryant Conant observed that "This is a physicist's war rather than a chemist's."[143] AMS Secretary Richardson offered his own version of Conant's claim in a January 1942 letter, alleging that Conant remarked to his counterpart, Frank Jewett, of the National Academy of Sciences, that "The last was a war of chemistry but this one is a war of physics." To that, Jewett, trained as an electrical engineer, was said to reply "It may be a war of physics but the physicists say it is a war of mathematics."[144]

Some in Europe had tried, with similar language, to carve out a role for mathematicians during World War I. Now American mathematicians deployed the same terms to court new military and civilian patrons in the first half of the 1940s while simultaneously trumpeting the importance of mathematics and mathematicians to scientists, policy makers, and the general public. As Marshall Stone wrote in July, 1940, "If mathematics is to be brought to bear upon our defense problems in full measure, we shall have to organize and conduct propaganda to this end."[145] Latching onto the notions of "total war" and the "manpower problem," these figures aggressively sought the ear of military officials and any, like Conant and Jewett, with influence

[143] James B. Conant, "Chemists and the National Defense," *Chemical and Engineering News* 19, no. 22 (25 Nov 1941): 1237-1238, on 1237. See also David Kaiser, "History: From blackboards to bombs," *Nature* 523 (30 Jul 2015): 523-525.
[144] As recounted by R. G. D. Richardson to Dunham Jackson, January 19, 1942, Morse Papers, box 9, "Jackson, Dunham" folder.
[145] July 21 1940, Stone to Dunham Jackson, Morse Papers, box 13, "Stone, Marshall H." folder.

over wartime purse strings.[146] With surveys, reports, letters, editorials, and radio interviews, these mathematicians insisted that not just the war but the technical patrimony of the nation owed its present and future to their profession.[147]

Stone's call for organized propaganda played out, by and large, through a 1940 joint War Preparedness Committee of the American Mathematical Society and Mathematical Association of America, the two leading American professional organizations of mathematicians. On the United States' entry into armed conflict in 1942, the committee became the War Policy Committee, and in September 1945 it transformed again into a postwar Policy Committee for Mathematics.[148] The War Preparedness Committee's initial chair was Marston Morse, whom we have already seen to be a consummate insider of American mathematics, and who would soon assume the presidency of the American Mathematical Society, as well. The committee, which received substantial financial support from the Rockefeller Foundation for its activities, gathered information about mathematicians' current and potential participation in the war effort and advocated for the mathematicians' professional interests.

Among the many outgrowths of this effort was the creation in 1943 of an Applied Mathematics Panel at the Office of Scientific Research and Development, a civilian organization

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[146] Cf. 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.
[147] See Parshall, "A New Era," 22-27. Transcripts of radio broadcasts frequently appear in the collected papers of leading mathematicians. See e.g. Morse Papers, box 7, "Hart, William L. (Hart Committee)" folder; box 13, "Stone, Marshall H." folder; box 15, "War" folder. For a recording, see Bennington P. Gill, "The Role of Science in War," WNYC radio broadcast, 9 May 1942, at http://www.wnyc.org/story/bennington-p-gill/ (accessed 2016).
[148] See Kline report, October 30, 1945, RF Projects: U.S., box 127, folder 1561.

established in 1942 to support American military operations.[149] The panel included several grandees of the American Mathematical Society, including Marston Morse, Griffith Evans, and Oswald Veblen, who would play central roles in organizing the renewed International Congress of Mathematicians in 1950. It was also, by no means incidentally, directed by none other than Warren Weaver. At the suggestion of panelist Richard Courant, Weaver invited Mina Rees to join as a technical aide, and her work on the Applied Mathematics Panel set the template for Rees's later work at the Office of Naval Research, through which she channeled funding for both pure and applied mathematics after the war.[150]

The Applied Mathematics Panel and Office of Naval Research primarily sponsored research through a contract model. Academic mathematicians organized into research centers on broad topics like dynamics, differential equations, or statistics, with a small number of established researchers and a varyingly-sized and rotating cast of post-doctoral and doctoral student assistants. These would be assigned contracts centered on particular research questions formulated or refined by expert mathematicians on the Applied Mathematics Panel or consulting for the Office of Naval Research. Letters and reports from the War Policy Committee stressed the value of dividing labor between a small group of elite mathematicians capable of formulating problems for research and a much larger group of competent working mathematicians who could solve those mathematical problems.[151]

At the same time, leading mathematicians personally assumed military roles as advisors and technical experts, often arranging leaves from their academic posts in order to do so. In this

[149] Rees, "The Mathematical Sciences"; Amy Shell-Gellasch, "Mina Rees and the Funding of the Mathematical Sciences," *The American Mathematical Monthly* 109, no. 10 (2002): 873-889, on 875-876.
[150] Shell-Gellasch, "Mina Rees," 876-885.
[151] See reports in RF Projects: U.S., box 127, folder 1561.

respect, American mathematicians followed a pattern that was already well-worn by those of other belligerent nations. Morse, again, sat near one extreme. During the Great War, he had served with the Ambulance Service in France and reflected at the time that "Although the ambulance work is great, still I think I could be of more use in some branch of the service where some mathematical training is necessary."[152] Now a prominent member of the mathematical profession, he arranged to serve as a technical expert for the U.S. Army's Ordnance Department and compiled, by his own count, some eighty reports on various aspects of bomb deployment. Here, he followed the World War I path of his IAS colleague Oswald Veblen, who (along with Norbert Wiener, among other future leaders of the discipline) had worked extensively on munitions at the U.S. Army's Aberdeen Proving Ground.[153] Morse also consulted directly for the National Defense Research Committee that preceded the Office of Scientific Research and Development. For his efforts, the War Department cited him for "outstanding meritorious service […] of great value to every branch of our Armed Forces and to Allied Nations."[154] Such service furnished Morse and those who played similar roles with privileged access to military and civilian government authorities by the war's end.

For the other principal belligerents on each side of the conflict, similar patterns of mass involvement by mathematicians in the war effort obtained.[155] Mathematicians angled to insert themselves into applicable areas of the war effort, especially those involving ballistics and several branches of engineering. Officers from national mathematical societies courted military

[152] Morse to Birkhoff, 1 Dec 1917, Birkhoff Papers, HUG 4213.2, box 4, quoted in Archibald, Dumbaugh, and Kent, "A Mobilized Community," 239.
[153] See Archibald, Dumbaugh, and Kent, "A Mobilized Community," 241-244.
[154] Marston Morse - War Work, 1940-1946," IAS Faculty Files, box 24A, folder 8.
[155] Siegmund-Schultze, "Military Work" offers a range of comparisons and references. On Germany, see also Volker R. Remmert, "Mathematicians at War, Power Struggles in Nazi Germany's Mathematical Community: Gustav Doetsch and Wilhelm Süss," *Revue d'histoire des mathématiques* 5 (1999): 7-59.

patronage, offering technical advice and, most importantly, coordinated access to technical personnel. Colleges and universities transformed into training grounds for officers and technical personnel who needed to learn some basic areas of mathematics, and to learn them quickly. Many more resources were available to mathematicians, but the supply was not limitless, and there were struggles over access and authority. In countries that escaped the worst of the war's devastation, mathematicians retained and built on much of the access to power and resources they had garnered during the war, while in those on the front lines the hangover of war had less to offer the corps of mathematicians.

In occupied areas the story could be quite different. Many mathematicians left as refugees, and a great number of those stayed permanently abroad, transforming their adopted mathematical communities and supplying a ready-made store of connections to and familiarity with their former mathematical homes. A similar pattern held for mathematicians from Germany whose ethnicity or politics made staying untenable—Neugebauer was just one among many prominent German figures who reshaped mathematical communities across their entire itineraries.[156] Though the German invasion of Poland prompted the Americans to cancel the 1940 Congress, Polish refugee mathematician Alfred Tarski capitalized on the work that had already been done in preparation for the Congress to advance his case for a permanent visa to reside in the United

156 Reingold, "Refugee Mathematicians," 316-319; Reinhard Siegmund-Schultze, *Mathematicians fleeing from Nazi Germany: individual fates and global impact* (Princeton: Princeton University Press, 2008); Siegmund-Schultze, *Rockefeller*; June Barrow-Green, Della Fenster, Joachim Schwermer, and Rienhard Siegmund-Schultze, eds., *Emigration of Mathematicians and Transmission of Mathematics: Historical Lessons and Consequences of the Third Reich*, Mathematisches Forschungsinstitut Oberwolfach Report 51/2011; Sanford L. Segal, "War, Refugees, and the Creation of an International Mathematical Community," in Parshall and Rice, eds., *Mathematics Unbound,* 359-380. On the effects of German refugee mathematicians passing through Denmark, specifically, see Henrik Kragh Sørensen, "Confluences of agendas: Emigrant mathematicians in transit in Denmark, 1933-1945," *Historia Mathematica* 41 (2014): 157-187.

States. In December, 1939, Tarski presented to the American Consulate in Cuba a folder full of letters collected over the previous month from leading American and European mathematicians and philosophers, including Bertrand Russell, Ernest Nagel, Rudolf Carnap, Willard Van Orman Quine, and Solomon Lefschetz.[157] California Institute of Technology professor Eric Temple Bell, a prolific mathematician and math history popularizer, declared "Tarski stands a fair chance of being remembered as one of the men who has made a substantial advance beyond Aristotle in logic, one of the few such in the past two thousand years." Marshall Stone observed that "He comes to us now in the prime of his life." William Graustein wrote on International Congress of Mathematicians letterhead to certify that Tarski was among the sixteen foreign mathematicians already invited for plenary addresses, and Haskell Curry and J. R. Kline also cited the ICM invitation to make the case for Tarski's suitability for immigration.

Refugees' personal and professional connections proved crucial, as officials of bodies like the Rockefeller Foundation with resources to assist in relocation often lacked the detailed knowledge of European scientific communities to enable them to identify and prioritize potential refugees. Hermann Weyl, for instance, sent Weaver a list of younger French mathematicians "who may be helped by an invitation to this country." On learning that Bourbaki collaborator André Weil had been advocating on behalf of his fellow Bourbaki mathematicians, Weyl wrote a follow-up letter to Alvin Johnson of New York's New School for Social Research (which was jointly coordinating "rescue action" with the Rockefeller Foundation) to explain the somewhat cryptic Bourbaki enterprise and urge that Bourbaki membership "should hardly play a decisive role in the selection" of evacuees.[158] Harald Bohr, particularly active advocate for German

157 Tarski Papers, carton 1, folder 12.

158 Weyl to Johnson, 22 March 1941, with attached annotated lists, IAS General Files, box 22, folder 13. Weil took a sour view of his own time as a Rockefeller refugee, writing to Weaver that

colleagues, remarked as early as 1933 to Abraham Flexner, who had just launched the Institute for Advanced Study in Princeton, that "In these later months I have had rather much to do, trying together with friends in different countries to find possibilities for the many German scientists, the basis of whose very existence has been taken away by the absurd and dreadful movement in Germany, which the leaders wish to call a 'revolution' in order to get away from any kind of humanity and justice."[159] Bohr's aid to American resettlement efforts won him the praise and admiration of influential Americans, leading to a concerted effort to secure his own relocation after the German occupation of Denmark, with the Rockefeller Foundation ultimately underwriting his salary at the University of Stockholm for the tail end of the war.[160]

Other mathematicians sought shelter from the effects of war as by adopting a low profile in provincial institutions. The Bourbaki collaborators who had put down roots in the 1930s at the University of Strasbourg, for instance, moved with their faculty after the German invasion to Clermont-Ferrand.[161] Some could make a passable existence in the open during the occupation. Others went into hiding as the occupation intensified. If high-profile Americans could afford to promote a war of mathematics, some unlucky French mathematicians hoped just to survive the war in order to return to mathematics in calmer times.

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he was disappointed in every one of his expectations for his American sojourn. Weil to Weaver, July 1943, in Michèle Audin, *Correspondance entre Henri Cartan et André Weil (1928-1991)* (Paris: Société Mathématique de France, 2011), 513. It should be said that Weil took a sour view of a great many things during his long and distinguished mathematical career.

159 Bohr to Flexner, 9 Aug 1933, IAS Member Files, box 12a, "Bohr, Harald" folder.

160 RF Projects: Sweden, box 6, folder 59. See also "Copies of Cables," 12 Oct 1943 – 29 Oct 1943; Veblen to Bush, 16 Oct 1943; Hanson to Tunberg, 29 Oct 1943, IAS Mathematics, box 3, folder 3. American mathematicians first advocated relocating Bohr to the United States, possibly under an OSRD or other government contract connected to the war effort, e.g. Strauss to Courant, 13 Oct 1943; Aydelotte to Weyl, 15 Oct 1943; Courant to Aydelotte, 16 Oct 1943, IAS Mathematics, box 3, folder 3.

161 See Anne-Sandrine Paumier, *Laurent Schwartz (1915-2002) et la vie collective des mathématiques* (PhD Dissertation, Université Pierre et Marie Curie, 2014), 40-42.

In these circumstances, one's capture during military service need not spell the end of one's research. Jean Leray wrote in January 1941 from a German Prisoner of War Camp to Hans Lewy at Berkeley to say that "this new situation" afforded "plenty of comfort to pursue my scientific research" before then expressing an urgent desire for Lewy and his U.S. contacts to send him recent offprints on partial differential equations and other fields of interest.[162] André Weil was himself a prisoner of war, though he was captured by conscience rather than the enemy and served time for refusing military service. The period was among the most intellectually productive he had yet experienced, and he mused to his wife Eveline that "My mathematics work is proceeding beyond my wildest hopes, and I am even a bit worried – if it's only in prison that I work so well, will I have to arrange to spend two or three months locked up every year?"[163] Part of Weil's success owed to his robust connections to the outside world. Personal visits from his wife, parents, and sister kept him stocked with bibliographic materials and his robust correspondence with Henri Cartan (among other mathematicians) helped him stay apprised of his field.[164]

War thus affected elite mathematicians highly unevenly, with some deriving great personal and professional benefits and others paying dearly. This range of experience shaped the variety of responses to the war's end and to efforts to form anew a functioning international community of mathematicians. Postwar mathematicians were imbricated and implicated in geopolitics and statecraft, and they knew it. This created new opportunities for cooperation, but also new venues for conflict. Such conflicts played out on scales large and small, implicating

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[162] Leray to Lewy, 19 Jan 1941, Lewy Papers, carton 1, folder 4.
[163] André to Eveline Weil, 7 Apr 1940, quoted in André Weil, *Apprenticeship of a Mathematician*, trans. Jennifer Gage (Basel: Springer, 1991), 146.
[164] Michèle Audin, "La guerre des recensions: Autour d'une note d'André Weil en 1940," *Mathematische Semesterberichte* 59 (2012): 243-260.

mathematicians institutions and theories alike. They played out as struggles to claim the discipline's future, but also as efforts to define the discipline's past: its triumphs, its failures, its hopeful starts and missed opportunities.

Table 1: Attendance at pre-World War I and interwar International Congresses.

	Prewar Congresses				
Host	**Zürich**	**Paris**	**Heidelberg**	**Rome**	**Cambridge**
Year	1897	1900	1904	1908	1912
Western Europe					
France	23 (6)	**89**	24 (5)	63 (29)	39 (6)
Italy	20 (5)	23	12 (2)	**190 (23)**	35 (6)
Germany	41 (12)	24	**173 (31)**	120 (54)	53 (17)
Switzerland	**60 (8)**	7	12 (1)	16 (2)	8 (2)
Great Britain	3		7 (1)		**221 (49)**
England		10		22 (11)	
Scotland		2			
Netherlands	3	2	6	3 (1)	9 (1)
Spain	1	3	1	5	25 (2)
Belgium	3	11	2	4	5
Portugal	1	3			2 (1)
Luxembourg		1			
Scandinavia					
Sweden and Norway			8 (1)		
Sweden	6	6		5 (1)	12 (2)
Norway		2		2 (2)	3 (1)
Denmark	3	3	13 (8)	3 (2)	4 (2)
Finland	4 (1)				
Central and Eastern Europe					
Austria-Hungary	17 (3)		25 (3)	51 (23)	
Austria		8			20 (3)
Hungary		1			16 (3)
Greece	17 (3)	1	1	3	4 (1)
Russia	12 (1)	14	30 (4)	19 (6)	30 (10)
Romania		4	2	6	4 (1)
Bulgaria			1	1	1
Serbia		2		1	1
North America	6 (1)				
United States		19	15 (4)	16 (11)	60 (27)
Canada		1	1	1	5

Prewar Congresses, continued

Host	Zürich	Paris	Heidelberg	Rome	Cambridge
Year	1897	1900	1904	1908	1912
Latin America					
Argentina		1	1		5
Mexico		1		1	2
Brazil		1			1
Peru		2			
Chile					1
Asia and Pacific					
Japan		1	2		3
India					3
Africa and Middle East					
Egypt				1	2
Turkey		1			
Algeria		1			
Tunisia				2	

Interwar Congresses

Host	Strasbourg	Toronto	Bologna	Zürich	Oslo
Year	1920	1924	1928	1932	1936
Western Europe					
France	**80 (32)**	24 (3) [18]	56 (35)	69 (20)	28 (13)
Italy	5 (2)	11 (1) [3]	**336 (76)**	64 (17)	5 (2)
Germany			76 (30)	118 (24)	35 (2)
Switzerland	14 (3)	4 [1]	29 (19)	**144 (41)**	20 (11)
Great Britain		58 (20) [13]			48 (18)
England	9 (2)		47 (17)	37 (12)	
Netherlands	5 (1)	4 (2)	9 (6)	16 (1)	15 (4)
Spain	10	3 [1]	11 (1)	10	8
Belgium	10 (4)	6 [2]	10 (4)	7 (1)	9 (3)
Portugal	3 (3)	2 [1]		2 (1)	
Ireland		2 (1)	2	4 (2)	6 (3)
Malta		1 (1)			

Interwar Congresses, continued

Host	Strasbourg	Toronto	Bologna	Zürich	Oslo
Year	1920	1924	1928	1932	1936
Scandinavia					
Sweden	1	3 [2]	4 (3)	5 (2)	26 (10)
Norway	4 (2)	5 [1]	8 (4)	9 (5)	**59 (25)**
Denmark	3 (1)	3 [1]	9 (5)	6 (2)	22 (5)
Finland			2	3	8 (1)
Iceland					1
Central and Eastern Europe					
Austria			9 (1)	10	10 (2)
Hungary			22 (9)	12 (3)	5 (1)
Greece	6 (1)	[1]	8 (2)	5	4
Russia	1 (1)	4 [10]	27 (3)	10 (2)	11
Romania	6 (1)	1 [1]	11 (8)	7 (5)	9 (6)
Poland	2	2 [1]	31 (10)	20 (2)	25 (4)
Czechoslovakia	12	3	15 (7)	12 (1)	10 (3)
Bulgaria			5	3 (1)	2
Yugoslavia		1	4 (1)	4	2 (3)
Ukraine		1 [2]	10 (3)		
Serbia	1		1		
Latvia			1	1 (1)	3 (1)
Georgia		1			
Lithuania			1 (1)		
Estonia					1
North America					
United States	11 (4)	191 (64) [15]	52 (24)	66 (36)	86 (56)
Canada	1	**107 (7) [4]**	4 (3)	2	7 (6)
Latin America					
Argentina	6	2 (1)	7		
Mexico	2	1		1	
Brazil	1		2		
Peru		[1]			
Cuba		[1]			
Guatemala			1 (2)		

Interwar Congresses, continued

Host	Strasbourg	Toronto	Bologna	Zürich	Oslo
Year	1920	1924	1928	1932	1936
Asia and Pacific					
Japan	2	[1]	11 (2)	3 (1)	4
India	2	2	5 (2)	2	3
China				3 (3)	1
Australia	1	[1]			1
Hong Kong		1			
Philippines	1				
Samoa		1			
Africa and Middle East					
Egypt	1	[1]	2	5 (2)	3
Palestine			6	2	5 (1)
Turkey			2 (2)	2	
South Africa				2 (1)	2 (1)
Iran				1	2 (1)
Algeria					1

Notes: Numbers are transcribed from the national attendance tables in the respective Congress proceedings, except for the 1900 Paris Congress, where I have counted participants manually according to parenthetical national identifications in the member roster (combining German regions and omitting the two "deceased" members). I have sorted entries within approximate geographic regions roughly according to attendance in order to give a better sense of variation across regions and times. I have indicated registered associate or family members, when listed in the respective proceedings, using parentheses. Corresponding members tabulated in the 1924 Toronto proceedings are listed in square brackets. Host delegations are in bold. I have listed numbers for both "Great Britain" and "United Kingdom" under "Great Britain" and have translated "Holland" as "Netherlands" where it appears. All except the 1936 Oslo proceedings list "Russia," and I have put the "U.S.S.R." number from the 1936 proceedings along with the "Russia" numbers.

Chapter 2: A Widely Anticipated Invention

A Derivative Innovation

"The invention of distributions took place in Paris, at the start of November 1944," recounted Laurent Schwartz in his autobiography, half a century later.[1] He would call it "the most beautiful night of my life."[2] Just over two months prior, the city had been under German control, but the occupiers were in retreat and the end of the war in France seemed at hand. Apart from a brief spell in the company of many from the Bourbaki group in Clermont-Ferrand, Schwartz was too preoccupied with matters of life and limb during the German occupation to pursue much mathematical research.[3] With the occupation's end, Schwartz could return to mathematics—if not fully, then at least with the attention required to follow some new lines of inquiry. He lingered, in particular, on a problem from the theory of partial differential equations he had first encountered as a student at the École Normale Supérieure in 1935, and which appeared newly relevant in view of the trickle of unpublished work from fellow French mathematicians that Schwartz could now access. He would recall his November breakthrough as "a click coming about on the occasion of a particular circumstance [that] sometimes unites the tentative paths" of investigation. Numerous partial "predecessors" and "antecedents" merged into a genuine "discovery."[4]

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[1] Laurent Schwartz, *A Mathematician Grappling with His Century*, trans. Leila Schneps (Basel: Birkhäuser, 2001), 209; Laurent Schwartz, *Un mathématicien aux prises avec le siècle* (Paris: Odile Jacob, 1997), 223. I have departed from Schneps's translation in some places and cite pages for Schwartz's French edition in brackets below.

[2] Ibid., 232 [246].

[3] Ibid., 167 [176].

[4] Ibid., 209 [223]. Schwartz moved freely between the language of "discovery" and "invention" in accounting for the origins of distributions. I will follow his usage by not dwelling on the distinction between these two terms here, despite the distinction's importance in twentieth-century philosophy of mathematics. Cf., e.g., Wittgenstein's famous remark "Der Mathematiker

But wars do not end when the last occupying soldier takes leave of disputed soil, and discoveries are not made in single beautiful nights, sudden clicks and epiphanies.[5] Schwartz recounted the partial results, the famous battles, that led to his singular invention. At Versailles at the close of the Great War, French Prime Minister Georges Clemenceau famously marked the transition from winning the war to the more difficult task of winning the peace. In Paris at the close of the Second World War, Schwartz too would embark on the difficult work of winning a place for his creation in mathematics. His task, like Clemenceau's, involved navigating interests, reckoning debts and causes, and reconciling histories.

From his earliest writings and advocacy about distributions, Schwartz struck a delicate balance between asserting his theory's novelty and claiming for his theory a wide range of antecedents. In effect, to establish distributions' relevance he put his priority in question. His claims to originality thus hinged on broader claims, both implicit and explicit, about the relationships between mathematical ideas and about the nature of mathematical theories. These allowed Schwartz to depict his theory of distributions as at once new and familiar, wholly original and widely anticipated. His theory's early adopters, advocates, and even critics built

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ist ein Erfinder, kein Entdecker." ("The mathematician is an inventor, not a discoverer.") Ludwig Wittgenstein, *Remarks on the Foundations of Mathematics*, trans. G. E. M. Anscombe, ed. G. H. von Wright, R. Rhees, and G. E. M. Anscombe (Oxford: Basil Blackwell, 1954), remark I: 167.

[5] This view of discoveries has been well developed in historical and sociological analyses of the natural sciences. See especially Harold Garfinkel, Michael Lynch, and Eric Livingston, "The Work of a Discovering Science Construed With Materials from the Optically Discovered Pulsar," *Philosophy of Social Sciences* 11, no. 2 (1981): 131-158; Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987), esp. 126-133, 268; Michael Lynch, "Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science," in Andrew Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992), 215-265, on 247-256. See also Augustine Brannigan, *The Social Basis of Scientific Discoveries* (Cambridge: Cambridge University Press, 1981), which frames discovery in relation to the norms and expectations of a scientific community. On a discovery in mathematics viewed through retrospective accounts, see Andrew Pickering and Adam Stephanides, "Constructing Quaternions: On the Analysis of Conceptual Practice," in Pickering, ed., *Science as Practice and Culture*, 139-167, esp. 142-144.

upon such hybrid presentations to shape their personal and collective understandings of distributions. For them, the theory's contested history held lessons about both the theory itself and its famous progenitor.[6]

This imbrication of theory and historiography is evident across the extant literature on Schwartz and distributions, from both mathematicians and historians.[7] A central concern in this literature has been the relation between distributions and the many predecessors and antecedents that came to be associated with it, as well as the biographical relationships between Schwartz and others who trod similar conceptual terrain. The first book-length historical study of the theory of distributions was dedicated almost entirely to the conceptual and genealogical relationships among the wide range of ideas and techniques that distributions' proponents and critics came to associate with the theory in its early years.[8] For Schwartz and defenders like his fellow Bourbaki collaborator Jean Dieudonné, recounting the range of such antecedents demonstrated the

[6] Cf. Peter Galison, "Re-Reading the Past from the End of Physics: Maxwell's Equations in Retrospect," in Loren Graham, Wolf Lepenies, and Peter Weingart, eds., *Functions and Uses of Disciplinary Histories* (Dordrecht: D. Reidel, 1983), 35-51; Richard Staley, *Einstein's Generation: the Origins of the Relativity Revolution* (Chicago: University of Chicago Press, 2008), part IV. The broader claim that communities invent pasts to suit their presents has long been a historiographical staple, e.g. Eric Hobsbawm and Terence Ranger, eds., *The Invention of Tradition* (Cambridge: Cambridge University Press, 1983); Herbert Butterfield, *The Whig Interpretation of History* (London: G. Bell and Sons, 1931).

[7] On the relationship between theory and historiography in modern mathematics, cf. David Aubin and Amy Dahan Dalmedico, "Writing the History of Dynamical Systems and Chaos: *Longue Durée* and Revolution, Disciplines and Cultures," *Historia Mathematica* 29 (2002): 273-339.

[8] Jesper Lützen, *The Prehistory of the Theory of Distributions* (New York: Springer, 1982). See also John Synowiec, "Distributions: The Evolution of a Mathematical Theory," *Historia Mathematica* 10 (1983): 149-183. Schwartz had a personal role in shaping both of these narratives. See Synowiec to Schwartz, 5 Apr 1977, and Schwartz to Synowieg [sic], 4 May 1977, Schwartz Papers IX.B.I.1.1.102; Lützen to Schwartz, 9 May 1977, Schwartz to Lützen, 14 Jun 1977 and 20 Dec 1978, Schwartz Papers IX.B.I.1.1.64; Lützen personal communication, 9 Nov 2012.

originality and significance of Schwartz's innovation.[9] For skeptics and critics, all of those apparently equivalent approaches exogenous to Schwartz's own furnished evidence that the theory was merely derivative—consisting of old ideas in new clothing and reflecting differing national or personal styles of mathematics.[10]

Two recent studies of distributions' reception and incorporation in different communities extend the relationship of historiography to theory still further beyond the questions of priority that have motivated so many analyses. In the first, Klaus-Heinrich Peters examined interacting interpretations of the theory among mathematicians and theoretical physicists to suggest a relationship between the consistency principles of mathematical rigor and physicists' emergent commitment to phenomenological criteria in formulating and evaluating physical theories.[11] For Peters, a particular way of justifying a new mathematical formalism, based on the particular form of rigor that made Schwartz's theory appear simultaneously relevant and innovative, had far-

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[9] Schwartz, *Mathematician Grappling*, ch. 6; Jean Dieudonné, Review of Lützen, *Prehistory*, *The American Mathematical Monthly* 91, no. 6 (1984): 374-379; Colette Anné, Jean-Pierre Bourguignon, and Claude Viterbo, eds. *Supplément au numéro 98 de la Gazette des mathématiciens*, 2003. See also Peter Lax, "The Reception of the Theory of Distributions," *The Mathematical Intelligencer* 26, no. 1 (2004): 52. For Lax, it was primarily Schwartz's uneven reception that proved his originality.

[10] E.g. Salomon Bochner, Review of Laurent Schwartz, *Théorie des distributions*, vols. 1 and 2. *Bulletin of the American Mathematical Society* 58, no. 1 (1952): 78-85; Jean-Michel Kantor, "Mathematics East and West, Theory and Practice: The Example of Distributions," *The Mathematical Intelligencer* 26, no. 1 (2004): 39-46; Adolf P. Yushkevich, "Some remarks on the history of the theory of generalized solutions for partial differential equations and generalized functions," *Istoriko-matematicheskie issledovaniia* (1991): 256-266, translated as an appendix to Kantor, "Mathematics East and West," on 46-50; S. Kutateladze, "Some Comments on Sobolev and Schwartz," *The Mathematical Intelligencer* 26, no. 1 (2004): 51.

[11] Klaus-Heinrich Peters, "Mathematische und phänomenologische Strenge: Distributionen in der Quantenmechanik und -feldtheorie," in Karl-Heinz Scholte and Martina Schneider, eds., *Mathematics meets physics: A contribution to their interaction in the 19th and the first half of the 20th century* (Frankfurt am Main: Verlag Harri Deutsch, 2011), 373-393; Peters, *Der Zusammenhang von Mathematik und Physik am Beispiel der Geschichte der Distributionen: Eine historische Untersuchung über die Grundlagen der Physik im Grenzbereich zu Mathematik, Philosophie und Kunst* (Dissertation zur Erlangung des Doktorgrades des Fachbereichs Mathematik der Universität Hamburg, 2004).

reaching consequences for the epistemology of mid-century physics. From a perspective that is more sociological than epistemological, Anne-Sandrine Paumier interpreted the theory's multiple mathematical and historical presentations as evidence of new postwar forms of collective engagement in French and international mathematics.[12] Situating the theory's reception in the context of Schwartz's biography and the changing modes of collective life in French mathematics, with particular attention to the twentieth-century rise of the mathematical seminar, Paumier treated narratives and assessments of discovery, invention, interaction, and significance as active interventions in mathematicians' communal memory and activity.

These analyses make clear that Schwartz and his contemporaries were not just inventing a theory and a tradition but a community, a philosophy, and a future. In the analysis that follows, I begin by unpacking the invention narrative from Schwartz's autobiography, identifying the different uses and implications of Schwartz's claims about the relationships amongst mathematical theories. Looking back on Schwartz's own retrospective account throws into relief what were by then well-worn historiographical positions on the origin and significance of distributions and the conceptual terrain in which Schwartz intervened in the mid-1940s. I then pick up the story from the earliest surviving published and archival records of Schwartz's theory and advocacy, showing how Schwartz framed distributions and the theory's antecedents for a variety of audiences between 1945 and 1949. Placing these presentations in the infrastructural, institutional, and philosophical contexts of mathematics in this period, I elaborate a relationship between Schwartz's historical claims and the means by which mathematicians in Europe and North America first encountered the theory of distributions. Here, making a theory was

[12] Anne-Sandrine Paumier, *Laurent Schwartz (1915-2002) et la vie collective des mathématiques* (PhD Dissertation, Université Pierre et Marie Curie, 2014), esp. § 0.1, ch. 2. My account in this chapter is indebted to Paumier's insightful reconstruction and analysis of the invention and invention-narratives of distributions.

inseparable from making a living, making a name, making history, and making sense. These constructions' circumstantial details explain Schwartz's and his theory's rapid early recognition in terms that illuminate how distributions' advocates productively comingled different situated pasts, presents, and futures at the dawn of postwar international mathematics.

Distinctions Among Differences

While mathematicians in different times and places since the advent of the calculus have accorded a considerable variety of precise meanings to the mathematical notions of functions and derivatives, nearly all have acknowledged an essential mismatch between them.[13] Modern definitions of functions have roots in seventeenth-century analyses of the relations between variable quantities derived principally from abstracted kinematic processes and geometrical figures.[14] For instance, a function could represent (kinematically) the course of a falling object or a point on a rolling wheel, or (geometrically) the static path traced by either. René Descartes and his contemporaries established many of the principles that Isaac Newton and Gottfried Wilhelm Leibniz, in their respective formulations of what European mathematicians ultimately reconciled into a coherent infinitesimal calculus, would place at the foundation of the systematic study of variables and their rates of change over abstract time and space.[15] By the middle of the

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[13] J.R. Ravetz, "Vibrating Strings and Arbitrary Functions," in *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi on his Seventieth Birthday, 11th March 1961* (London: Routledge, 1961), 71-88; A. P. Youschkevitch, "The Concept of Function up to the Middle of the 19th Century," *Archive for History of Exact Sciences* 16, no. 1 (1976): 37-85.

[14] Henk J. M. Bos, *Redefining Geometrical Exactness: Descartes' Transformation of the Modern Concept of Construction* (New York: Springer, 2001); Michael Mahoney, "The Mathematical Realm of Nature," in Daniel Garber and Michael Ayers, eds., *The Cambridge History of Seventeenth-Century Philosophy* (Cambridge: Cambridge University Press, 1988), 702-755.

[15] Niccolò Guicciardini, *The Development of Newtonian Calculus in Britain, 1700-1800* (Cambridge: Cambridge University Press, 1989); Henk J. M. Bos, "Differentials, Higher-Order Differentials and the Derivative in the Leibnizian Calculus," *Archive for History of Exact*

eighteenth century, Leonhard Euler had successfully promoted among elite European mathematicians a conception of functions as relations between independent and dependent variable quantities, such as could be represented on a Cartesian graph. Derivatives, here, marked the rate of change of a dependent variable with respect to a specified independent variable. In a kinematic framework, if that independent variable corresponded to the passage of time and the dependent variable represented the position of an object, then the derivative was the object's velocity. Geometrically, the variables could be orthogonal axes of a plotted curve and the derivative would be the slope of the curve's tangent at a corresponding point along the independent variable's axis.

Since derivatives reflected a function's variable rate of change, and thus themselves related a dependent variable quantity (the rate of change) to an independent variable, they too were functions, and so had their own derivatives (which were themselves functions, and so on).[16] This furnished the template for most subsequent interpretations of these terms, with the precise meanings of variables, rates of change, and other relevant terms continually reinterpreted according to intellectual and practical needs and disciplinary norms. History's various interpreters of functions and derivatives dwelled, in particular, on the relative foundational statuses of different forms of representation, ranging from graphs and diagrams to explicit or implicit formulae to different kinds of symbolic expressions of abstract relations.[17] As

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Sciences 14 (1974): 1-90; Domenico Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz* (Oxford: Oxford University Press, 1997).

[16] Note that this chain of interpretations works only with Eulerian functions. Prior to his formulation in terms of independent and dependent variables, derivatives were generally seen as categorically different from the derived entity: fluxions were not fluents in disguise, and Leibnizian differentials differed in dimension from the geometric objects of Leibniz's calculus. See esp. Bos, "Higher-Order Differentials," §1.

[17] E.g. Michael J. Barany, "God, King, and Geometry: Revisiting the Introduction to Cauchy's *Cours d'Analyse*," *Historia Mathematica* 38 no. 3 (2011): 368-388.

mathematicians offered new definitions of the central terms of the calculus, this history of adaptation and reinterpretation was itself an important feature of their rhetorical and conceptual interventions: by the nineteenth century, mathematicians recognized the calculus as a subject whose changing fundamental notions manifested the changing state of mathematics itself. Sometimes this history was a measure of mathematicians' progress. Other times, as for Émile Picard at the opening of the 1920 International Congress of Mathematicians, "analyzing the subtlest recesses of the idea of a function" represented the cloistered predilections of "incorrigible idealists," whose attitude Picard found untenable after the Great War.[18] For this reason, to attempt to specify further the precise meaning of functions and derivatives at this point in the story of distributions would be to put the cart before the horse.[19] Schwartz argued that mathematicians should change their understandings of these terms in part based on claims about how those terms had already changed over their history as past mathematicians called aspects of past interpretations into question.

Most historical construals of derivatives could not be applied to everything that counted as a function, since functions (in nearly every definition) could have abrupt changes or discontinuities that made nonsense of the gradual variation that defined derivatives. Conversely, any of the few historical definitions of functions which were guaranteed always to have formal derivatives, like Lagrange's analytic definition of functions as formal power series, involved severely limiting what counted as a function in ways many contemporaries found untenable.[20]

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[18] Émile Picard, "Séance d'Ouverture du Congrès: Allocution de M. Émile Picard," *1920 ICM Proceedings*, xxvi-xxix, on xxviii-xxix.
[19] Likewise, one should not teach analytic geometry to someone with a sore throat.
[20] See e.g. Craig G. Fraser, "Lagrange's Analytical Mathematics, Its Cartesian Origins and Reception in Comte's Positive Philosophy," *Studies in History and Philosophy of Science* 21, no. 2 (1990): 243-256; Joan Richards, "Historical Mathematics in the French Eighteenth Century," *Isis* 97 (2006): 700-713, on 710.

From the eighteenth century to Schwartz's time, mathematicians broadly considered and taught the calculus of derivatives—whether understood in terms of geometric curves, algebraic formulae, or abstract sets—as a science of smoothly varying quantities, not the sudden impulses and abrupt transformations to which many of those same mathematicians routinely attempted to apply it nonetheless. Schwartz was far from the first to attempt to endow the calculus and its associated theory of differential equations with a mathematical construct better suited than functions for the theory's central operation of differentiation. As others have documented, such efforts date to the first century of Newton's and Leibniz's calculus, and many since then have believed the problem suitably resolved in one form or another.[21] In November 1944, Schwartz, too, believed himself to have settled the matter once and for all.

Anticipation and Priority

Challenges to the novelty and priority of Schwartz's theory came quickly on the heels of his claims themselves, and mathematicians debated the matter long after the rudiments of his claims had become routine and widespread elements of mathematical teaching and exposition.[22] Historians and sociologists of mathematics and science have turned repeatedly to priority disputes, showing how scholarly communities use questions of priority to negotiate communal norms and philosophical principles in ways that range far beyond ascertaining the origin and proper attribution of a particular development.[23] By the time Schwartz wrote his autobiography,

[21] See esp. Lützen, *Prehistory.*
[22] See Paumier, *Laurent Schwartz,* ch. 2.
[23] E.g. Bertoloni Meli, *Equivalence and Priority*; A. Rupert Hall, *Philosophers at War: The Quarrel Between Newton and Leibniz* (Cambridge: Cambridge University Press, 1980); I. Grattan-Guinness, "Bolzano, Cauchy and the 'New Analysis' of the Early Nineteenth Century," *Archive for History of Exact Sciences* 6, no. 5 (1970): 372-400; Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review* 22,

after decades of such challenges, his account of the invention of distributions had developed into an elaborate defense of both his priority and his epistemic innocence. This involved, on the one hand, situating his innovation in a long history of attempts to rectify the problem of functions in the theory of differential equations while amplifying his own formulation's novelty and unique elegance and comprehensiveness. Here, a history of antecedents showed the nature of the innovation for which Schwartz asserted priority. At the same time, Schwartz propounded a narrative of invention predicated on his ignorance of most of that history and his due acknowledgement of his forerunners upon learning of their efforts. His claim to priority thus also hinged on his biographical independence from the historical developments for which his innovation was a culmination and a capstone.

The chapter on the invention of distributions in Schwartz's autobiography was, for this reason, narratively out of joint with the rest of the work, which more neatly fit a memoir genre of thematic digressions anchored to a chronological life story. In his invention chapter, Schwartz instead rotated among two histories and a philosophy—the history of antecedents to his theory, his personal history of discovery or invention, and philosophical reflections and analogies about the nature of mathematical and scientific discovery that were to relate the two histories. Weaving these together, Schwartz marshaled a litany of precursors and inspirations for distributions. He joined to each such antecedent a profession of how it related to and differed from his theory, when he learned of it, and how he responded to this knowledge.

To preface his specific discussion of distributions' antecedents, Schwartz proposed two analogies to explain the significance of his invention. The first was the history of the integral

no. 6 (1957): 635-659; Michael D. Gordin, "The Textbook Case of a Priority Dispute: D. I. Mendeleev, Lothar Meyer, and the Periodic System," in Jessica Riskin and Mario Biagioli, eds., *Nature Engaged: Science in Practice from the Renaissance to the Present* (New York: Palgrave Macmillan, 2012), 59-82.

calculus. As Schwartz told it, integration (the converse of differentiation) could be traced at least as far as Archimedes. Newton and Leibniz "unified" a panoply of related notions into "a grand global theory which dominates all of mathematics." However, in this telling, the final word on integration, in its full generality, would wait for Henri Lebesgue at the start of the twentieth century, though Bernhard Riemann found a simpler and less general version in the nineteenth.[24] The French modern offered the definitive refinement of a notion with a long history of refinements by finding the most abstract and therefore most generally applicable representation of the essential operation of integration.[25] Lebesgue, in building his definition for the integral on the relatively new set-theoretic theory of measures, had captured the fundamental nature of integration and had thereby made it possible for mathematicians rigorously and consistently to integrate any so-called measurable function.

Schwartz's history of integration was one where functions themselves changed little, and mathematicians successively changed the definition of integration so that it applied to more and more functions, until it was mathematically applicable to every function a mathematician might care to integrate.[26] Schwartz's second analogy, by contrast, involved a relatively stable collection

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[24] Schwartz, *Mathematician Grappling*, 209-210 [224].

[25] See Thomas Hawkins, *Lebesgue's Theory of Integration: Its Origins and Development* (Madison: University of Wisconsin Press, 1970); Loren Graham and Jean-Michel Kantor, *Naming Infinity: A True Story of Religious Mysticism and Mathematical Creativity* (Cambridge: Belknap, 2009), chs. 2-3.

[26] This excludes, for instance, functions whose curves enclose non-finite areas, or those based on sets that are "non-measurable." Part of the integral's history, like the corresponding history of the derivative, has been the negotiation of which sorts of mathematical entities might reasonably be expected to be integrable. The so-called Dirichlet function, with the value one at irrational points and zero at rational points on a specified interval, is an example of a function newly considered reasonable with the advent of set theory, and to which Lebesgue's integral applied but Riemann's did not. Schwartz could safely presume his mathematical readers to have pedagogically familiar examples like this in mind with his brief allusion to Lebesgue and Riemann. On mathematicians' recalibration of what makes a reasonable example or expectation, see Imre Lakatos, *Proofs and Refutations: The Logic of Mathematical Discovery,* ed. John

of operations (those of polynomial arithmetic and algebra) that applied to successively expanding classes of objects (here, systems of numbers).[27] Every question about the addition, subtraction, multiplication, or division of rational numbers—those representable as fractions of integers—could be answered with a rational number. But in order to consider square roots for every rational number one needed real and then complex numbers as well. Miraculously, complex numbers were the end of the line: not just square roots but every operation of polynomial arithmetic could be settled with an answer among the complex numbers.[28] In the special circumstances where a problem was soluble using only the narrower groups of real or rational numbers, the shift to complex arithmetic made little difference: real or rational solutions could still be found within the complex framework and then carried back to their narrower contexts with a suitable change of perspective.

To drive home the analogy between the advent of complex numbers and that of distributions, Schwartz attributed to Italian logician Giuseppe Peano a 1912 suggestion that "There must exist a notion of generalized functions which are to functions what the real numbers are to the rationals," offering for differentiation what the passage to real numbers did for square roots.[29] The quote, as far as I have been able to determine, is apocryphal. Peano devoted a

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Worrall and Elie Zahar (Cambridge: Cambridge University Press, 1976), especially the discussions of functions and measures in the two appendices; David Bloor, "Polyhedra and the Abominations of Leviticus," *British Journal for the History of Science* 11, no. 3 (1978): 245-272.

[27] Schwartz, *Mathematician Grappling*, 212 [226].

[28] Though Schwartz presented this sequence from integers to complex numbers as a historical development, his framing owed more to contemporary understandings of the relationship amongst such number systems. Complex numbers remained controversial well into the nineteenth century, and mathematicians found in them the closure Schwartz described only gradually and for a variety of reasons. See Gert Schubring, *Conflicts between Generalization, Rigor, and Intuition: Number Concepts Underlying the Development of Analysis in 17-19th Century France and Germany* (New York: Springer, 2005).

[29] Schwartz, *Mathematician Grappling*, p. 212 [226]. In Schwartz's version of the analogy, complex rather than real numbers represent the relation of distributions to ordinary functions.

considerable amount of writing in the early twentieth century to the history, theory, and semiology of numbers and functions, including an extended 1912 article on differentiation.[30] The quote itself, however, appears to be a misrecollection of a concluding remark from a short 1913 article about integration.[31] Here, the documentary record (or lack thereof) bears witness to some important features of Schwartz's mathematical memory. First, even when related with the apparent confidence of a direct quotation, Schwartz made historical claims largely on the basis of recalled associations rather than direct and documented research. Second, such relatively precise claims were nonetheless important legitimating features of Schwartz's rhetoric—and the appearance of precision was more important than its documentary basis. From the late nineteenth century, mathematical societies and institutions regularly arranged for famous members and forebears to be commemorated with authoritative collected editions of their works, a practice itself derived from an older "life and letters" biographical tradition.[32] Such collections were then and remain today fixtures of mathematics libraries, and mathematicians regularly turned to them as sources of problems, techniques, and especially motivations in their teaching, exposition, and research. Mathematicians' habits of use for collected works meant that attributions like Schwartz's were normal features of mathematical presentation in a variety of contexts.

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[30] Giuseppe Peano, "Derivata e Differenziale," *Atti della Reale Accad. delle Scienze di Torino* 48 (1912): 47-69, (*Opere Scelte*, Rome: Edizioni Cremonese, 1957, vol 1, #159, 369-388).

[31] Giuseppe Peano, "Resto nelle formule di quadratura espresso con un integrale definito?" *Atti della Reale Accademia dei Lincei* 22 (1913): 562-569. I owe this attribution to Clark McGranery and Gabriele Greco.

[32] Emmylou Haffner (Bergische Universität Wuppertal) is currently preparing a systematic study of the nineteenth-century production of Riemann's collected works that uses editors' correspondence to characterize the intricate interpretative processes such editions required. See also Michael Cahn, "*Opera Omnia:* The Production of Cultural Authority" and Hans-Jörg Rheinberger, "Writing Works: A Reaction to Michael Cahn's Paper," in Karine Chemla, ed., *History of Science, History of Text* (Dordrecht: Springer, 2004), 81-94 and 95-103.

In Schwartz's autobiography, the first group of antecedents to follow the analogies of integration and complex numbers were those that proved the interest and importance of generalized functions for the study of differential equations. Foremost were turn-of-the-century engineer Oliver Heaviside's symbolic calculus and Paul Dirac's related formulations in mathematical physics from the mid-1920s.[33] A calculation in Heaviside's notation showed how differential equations could be solved "instantaneously by magical means" but without rigorous mathematical justification. Dirac's innovations were similarly incredible: "physicists lived in a fantastic universe which they know how to manipulate admirably, and practically without fault, without ever being able to justify anything."[34] Both physicists' reputed sense of magic and fantasy and Schwartz's narrow conception of what counted as justification—something beyond an admirable method simply working without fault—were important features of Schwartz's presentation of distributions. Schwartz recorded that the Dirac function "nauseated" him as a student in 1935, but remained "in a corner of my memory" until the fortuitous "night of distributions' discovery."[35] His narrative of Dirac and Heaviside referred as well to interwar and early wartime work by his compatriot André Weil that introduced him to the calculus of convolutions, as well as to interwar work on the Laplace transform by three others that justified the Heaviside calculus without addressing Dirac's potent formalism.

The next group of Schwartz's antecedents offered, in his account, mathematically rigorous remedies for the study of partial differential equations that in their own ways fell short of Schwartz's synthesis. They thus merely "would constitute a part of the theory of

[33] Schwartz, *Mathematician Grappling*, 212-218 [227-232]. See Jesper Lützen, "Heaviside's Operational Calculus and the Attempts to Rigorise it," *Archive for History of Exact Sciences* 21, no. 2 (1979): 161-200.
[34] Schwartz, *Mathematician Grappling*, 217 [231].
[35] Ibid., 218 [231].

distributions."[36] With the exception of Jean Leray's use of integration by parts to find solutions to some differential equations, which figured in a course Schwartz took from him in 1934-1935, Schwartz professed complete ignorance of these other works prior to distributions' advent. The figures here, principally Salomon Bochner, Norbert Wiener, Sergei Sobolev, and Torsten Carleman, would appear prominently in debates over Schwartz's priority from the 1950s onward. With the benefit of hindsight, Schwartz thus attempted the delicate task of assigning credit without conceding influence or priority.

A final collection of antecedents represented what Schwartz identified as direct influences on his eventual formulation. While these antecedents came in the form of concepts, theories, and articles, Schwartz's recollections made these inseparable from the mathematicians who had advanced them. For the previous collection of antecedents—where Schwartz disclaimed any prior knowledge—he linked biographical and conceptual narratives in order to acknowledge that antecedent's conceptual relevance while explaining how he himself could not have been aware of that relevance at the time of his invention. When acknowledging influence with this next set of antecedents, conversely, Schwartz used biographical connections to explain relationships between others' formulations and his own while stressing conceptual distinctions to preserve his claims to priority and originality.

In the following overview, I have attempted to preserve Schwartz's promiscuous mix of memoir and function theory. This mixture, which leaves much unexplained about Schwartz's biography and theory alike, derives from Schwartz's decades-long project of self-justification, a project whose primary audience was mathematicians of his personal acquaintance. If the mathematical claims here leave you cold, fear not: you shall meet distributions on more

[36] Ibid., 219 [233].

approachable terms many times yet, as this chapter contrasts Schwartz's autobiographical account with his earliest efforts to explain and promote his theory. Focus, for now, on how Schwartz mingled personal struggles and conceptual obstacles, helpful colleagues and pliant formulations, chance encounters and inexorable conclusions.

The first of Schwartz's directly influencing antecedents, in this narrative, was the theory of finite parts that he learned at the École Normale Supérieure from his great uncle Jacques Hadamard. In Schwartz's telling, Hadamard's theory helped him present so-called fundamental solutions to differential equations by supplying a formalism for analyzing certain divergent integrals. Hadamard's approach, which from an early stage Schwartz made a point of justifying in the idiom of distributions, was here characterized as "an inextricable muddle" in need of clear-sighted rectification.[37] Next came Swiss mathematician Georges de Rham, whom Schwartz met in autumn 1942 while a doctoral student in Clermont-Ferrand, and who would become among the earliest adopters of Schwartz's approach. On that 1942 visit, de Rham introduced Schwartz to his own theory of currents, which he swiftly reformulated in terms of distributions once Schwartz sent him his proposed definition for the latter. Schwartz attributed his measure-theoretic interpretation of the Dirac function in part to conversations about topology and measure theory shared with de Rham over hot chocolate in a Clermont-Ferrand tearoom.[38] On his arrival in 1940 in Clermont-Ferrand, Schwartz had fallen in with the group of collaborators of Bourbaki who had relocated there from Strasbourg with the onset of war. These mathematicians, especially Jean Dieudonné, encouraged Schwartz to study topological vector spaces, which would offer the primary theoretical framework for distributions.[39]

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[37] Ibid., 224-225 [238].
[38] Ibid., 225-226, 239 [238-240, 251].
[39] Ibid., 226-228 [240-241]. Paumier, *Laurent Schwartz*, § 1.3.4-1.4.3.

The last of the direct influences, what Schwartz termed the "final trigger," was a short 1944 article by Gustave Choquet and Jacques Deny on differential equations involving harmonic and polyharmonic mass distributions.[40] Schwartz saw a pre-publication version of the article and solved a problem derived from one of Choquet and Deny's principal results. This solution, which the latter authors deemed "very elegant," appeared in a five-page note immediately following Choquet and Deny's piece in the *Bulletin de la Société Mathématique de France*.[41] It required a notion of generalized solutions to partial differential equations involving limits of conventional solutions in a manner Schwartz later associated with Bochner's approach, alongside convolutions of such solutions with compactly-supported smooth functions. This latter feature would be a centerpiece of Schwartz's early development of the theory of distributions, and compactly-supported smooth functions figured crucially even as Schwartz jettisoned his convolution-centric initial formulation.

The *Bulletin*'s editors received Schwartz's manuscript on October 31, 1944. If Schwartz is to be believed, his breakthrough came sometime in the subsequent fortnight, though the precise date is lost to history. Expanding on his *Bulletin* note, Schwartz began to interpret generalized solutions to partial differential equations as convolution operators from the topological vector space of compactly supported smooth functions to that of all smooth functions. Rather than look for ordinary functions that satisfied a differential equation, he proposed to treat solutions to such equations as ways to transform one kind of ordinary function into another by means of convolution. He could then determine properties of such solutions by examining how

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[40] Schwartz, *Mathematician Grappling*, 228-229 [241-243]. Paumier, *Laurent Schwartz,* § 1.4.2.
[41] Gustave Choquet and Jacques Deny, "Sur quelques propriétés de moyenne caractéristiques des fonctions harmoniques et polyharmoniques," *Bulletin de la Société Mathématique de France* 72 (1944): 118-140; Laurent Schwartz, "Sur certaines familles non fondamentales de fonctions continues," *Bulletin de la Société Mathématique de France* 72 (1944): 141-145.

the terms of the differential equation in question altered their corresponding transformations. This, Schwartz recollected in his autobiography, gave an immediate justification for the Dirac function and its derivatives.[42] Schwartz had more trouble finding a consistent interpretation of multiplication by smooth functions for convolution operators, and the result "left a bitter taste."[43] The last straw, in Schwartz's telling, was his inability to make sense of the Fourier transform, an indispensable feature of the theory of differential equations, in his new framework.

So Schwartz had to reinvent distributions three months later, by which time he had relocated to Grenoble. Schwartz had spent the latter part of the war in hiding near the city under an assumed name—Laurent-Marie Sélimartin, chosen because its cursive signature was nearly indistinguishable from his usual way of signing his legal name Laurent Moïse Schwartz (and so offering protection in case of an unthinking slip).[44] After the city was liberated, Schwartz petitioned for a position in the local faculty of sciences and was eventually appointed to replace a member who was a prisoner of war in Germany, though the appointment was not made until after Schwartz had moved with his young family temporarily to Paris.[45] In place of convolution operators, Schwartz proposed the topological dual to the space of compactly supported smooth functions, calling the dual space's elements distributions. From there, Schwartz developed notions of multiplication, convolution, Fourier transforms, and much else—not without difficulty, but very much more to his satisfaction.

Schwartz's autobiographical account was one in which mathematical theories did not come fully formed. Instead, they were the result of learning and labor, contemplation and interaction. Schwartz's emphasis on partial successes "percolating" (as he repeatedly put it) and

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[42] Schwartz, *Mathematician Grappling*, 230 [243-244].
[43] Ibid., 233 [247].
[44] Ibid., 190-191 [200-201].
[45] Ibid., 202, 211 [213, 225].

then finally emerging as a unified achievement suited his historiographical self-image. In this telling, it did not detract from his accomplishment that so much of his theory could be located in prior work by others. To the contrary, this wellspring of predecessors was necessary to represent Schwartz's theory as a genuine breakthrough.

The Propaganda Tract

Schwartz came to resolve questions over priority by making his distributions not a departure but a culmination, and his defenders came to adopt this historiographical posture with particular resolve. Many aspects of this narrative emerged only gradually over the course of years or decades of formal and informal presentations of distributions—in articles, reviews, textbooks, lectures, dinnertime conversations, memoirs, obituaries, and more.[46] But other aspects are identifiable in the earliest public presentations of the theory, when the theory's acceptance (much less acclaim) was far from assured, and well before its history had the chance to be controversial. These features became responses to (and resources for) Schwartz's many interlocutors after 1945, but in 1945 they were something different. At the birth of Schwartz's theory, his historical

[46] On narratives' role in establishing norms of professional scientific sociality, see Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge: Harvard University Press, 1988), ch. 3. On lore and memorialization in mathematics, see Caroline Ehrhardt, "How Mathematicians Remember," *International Social Science Journal* 203-204 (2011): 104-120; Liliane Beaulieu, "Bourbaki's Art of Memory," *Osiris* 14 (1999): 219-251; Amir Alexander, *Duel at Dawn: Heroes, Martyrs, and the Rise of Modern Mathematics* (Cambridge: Harvard University Press, 2010); Henrik Kragh Sørensen, "Reading Mittag-Leffler's Biography of Abel as an Act of Mathematical Self-Fashioning," in Benjamin Wardhaugh, ed., *The History of the History of Mathematics: Case Studies for the Seventeenth, Eighteenth and Nineteenth Centuries* (Oxford, Peter Lang, 2012), 115-144; Rebekah Higgitt, *Recreating Newton: Newtonian Biography and the Making of Nineteenth-Century History of Science* (London: Pickering and Chatto, 2007). On narrative and genre in scientific texts, including their role in consolidating historical and historiographical claims, see Charles Bazerman, *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science* (Madison: University of Wisconsin Press, 1988); Peter Dear, ed., *The Literary Structure of Scientific Argument: Historical Studies* (Philadelphia: University of Pennsylvania Press, 1991).

posture was in some respects a familiar kind of idealized narration offered to justify an inchoate program of research. In other respects, what would become an ensemble of historical claims about *his* mathematical theory began as an intervention about mathematical theories as such—about their relation to interpretive contexts, their relevance to allied domains of knowledge, and their proper aims and meanings.

The earliest surviving articulation of the formulation of the theory that Schwartz traced to February 1945 is an eighteen-page article in the *Annales de l'université de Grenoble*.[47] In his autobiography, Schwartz called this his "propaganda tract."[48] Though the article itself was not widely read, it represented Schwartz's first concrete presentation of a collection of principles and explanations that would figure centrally in his early efforts to promote his theory of distributions. As part of the 1945 annual volume for the university, the article itself did not appear until 1946. In fact, Schwartz was able to indicate in the past tense in his first footnote that the article furnished the template for his debut lectures on distributions, delivered as his January-April 1946 Cours Peccot, a prestigious Paris lecture series organized by the Collège de France that had recognized promising young mathematicians since the turn of the century.[49] The article also indicates the likely scope and emphases of Schwartz's four-hour presentation during a July 1946 Bourbaki Congress in Strasbourg, though Schwartz surely developed some points of more specialist interest from the theory of topological vector spaces as well.[50] Other mathematicians in

[47] Laurent Schwartz, "Généralisation de la notion de fonction, de dérivation, de transformation de Fourier et applications mathématiques et physiques," *Annales de l'université de Grenoble* 21 (1945): 57-74.
[48] Schwartz, *Mathematician Grappling*, 239 [251].
[49] See Schwartz, *Mathematician Grappling*, 240 [252-253]. The Collège de France archives contain a poster for Schwartz's course listing the course title and the dates of his lectures. Collège de France: Cours Peccot, 3 AFF 16.
[50] See Paumier, *Laurent Schwartz*, 59. The Congress and Schwartz's presentation are mentioned near the end of a letter from Henri Cartan to André Weil from Michèle Audin, ed.,

Europe, the United States, and beyond encountered the article in subsequent years through personally circulated offprints and summaries in widely-circulated review journals.[51] In later years, Schwartz and others adapted many of the article's claims and narrative features for a wide variety of audiences. Such broader contexts of circulation and adaptation make the 1945 article important to the history of distributions to a degree that far outstrips its own likely readership. Rich with examples but short on detailed proofs, the article's introduction and five body sections included a motivation and definition for distributions, discussions of differentiation and integration, and claims about the algebraic and topological features of the topological vector space of distributions.

In many ways, the direct and implied historical claims Schwartz made are typical of his period's mathematical and scientific writing. His few direct citations to prior publications assign credit and establish an intellectual lineage. His portrayals of precedents and mathematical problems frame his intervention and help him assert his new formulations' relevance. Such rhetorical features often functioned simultaneously in multiple registers, with different meanings and implications for different audiences. In these respects, Schwartz's writing was an example of the normal work of mathematical exposition in his time and place and shares features with scientific writing in a much wider range of contexts. By detailing what these features meant in

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Correspondance entre Henri Cartan et André Weil (1928-1991) (Paris: Société Mathématique de France, 2011), 118, and Audin offers further details about the Congress on p. 530. Distributions were sufficiently new to receive scare-quotes in Cartan's letter, which also noted Schwartz's "great progress" with the theory over the past year. Cartan mentioned a few points of particular interest that were discussed briefly in the 1945 article but likely considered at greater length for the Bourbaki audience.

[51] Precise evidence regarding these forms of circulation can be difficult to obtain for any particular article. I note below at relevant points what can be determined about the specific 1945 article.

specific interpretive contexts, I here use typical aspects of Schwartz's exposition to examine what was distinctive about his project and circumstances.

At the same time, Schwartz made claims about his theory's past, present, and future in ways that are not typical of modern scientific writing writ large. On the one hand, his historical and quasi-historical discussions helped him make specific mathematical and metamathematical arguments. Conversely, his specific mathematical and metamathematical claims also shaped the meanings and implications of his historical and quasi-historical statements. In particular, Schwartz wrote at a time when leading mathematicians (including those closest to Schwartz) paid special attention to the relative interpretive fixity or ambiguity of formal expressions and to mathematical arguments' sensitive dependence on their precise interpretive frameworks. Certain aspects of Schwartz's own attention to these concerns, I contend, help explain his own theory's crucial interpretive flexibility, and in turn his theory's rapid international recognition.

"Since the introduction of the symbolic calculus," Schwartz's 1945 article began, "physicists have commonly made use of certain notions or certain formulas for which the success is indisputable, while not being mathematically justified."[52] Physicists and their calculations, not mathematicians and their proofs, consume Schwartz's page-long overture. Though the latter part of his article's title promised "mathematical and physical applications," Schwartz put all of the explicit physical applications in this brief introduction. They appeared, moreover, as already-realized successes, the results of a "fecund […] formalism" ubiquitous in the physics of electricity, waves, and other phenomena.

That formalism rested on successive derivatives of the so-called Dirac function, whose value was 0 when its variable x did not equal 0 and infinity when x was 0, and whose definite

[52] Laurent Schwartz, "Généralisation [1945]," 57. I will omit further page citations for the remainder of this section's close reading of Schwartz's short article.

integral was 1. Such a paradoxical entity would seem impossible to depict, but mathematicians and physicists had no shortage of means for doing so. Graphically, they sometimes illustrated the function with a tall and thin rectangle of unit area situated along the positive *y* axis or a similarly-placed tall and narrow bell-shaped curve. Gesturally, they might run a hand horizontally at an even pace, then rapidly raise it to illustrate an infinite ascent before rapidly returning to the earlier horizontal trajectory.[53] Most commonly, however, the Dirac function's users depicted it as Schwartz did in his article, by way of a collection of conventional symbolic calculations or stipulations that governed its use—most importantly its value of 0 except at its one critical point and the value of its definite integral (written in a conventional notation for integrals of functions). If it lacked, as Schwartz asserted, a "mathematical" justification, it was at the same time readily justified by images, gestures, and symbolic practice, any of which might in practice satisfy a mathematician or mathematical physicist.

That is, the Dirac function's lack of mathematical justification *despite* its usefulness was not a pre-given rationale or state of affairs, but rather something Schwartz had to assert, in part by ruling its many manifest justifications to be non-mathematical. Rather, Schwartz's mathematics rested on rigorously formulated language—on conceptual definitions rather than the symbolic uses to which physicists routinely put the Dirac function. "Such an 'abuse of language,'" wrote Schwartz, "is despite everything incompatible with the habitual notion of function and derivation!" He announced the goal of his article to be a summary of a forthcoming monograph that "will provide a complete justification to the preceding language," referring to a textbook that

[53] Though I know of no surviving videographic evidence for such gestural practices, they can be reasonably inferred from photographs of and anecdotes about mathematical presentations from the period alongside observed practices of depiction in the present.

would take him the next half decade to complete.[54] Schwartz then claimed this language's many uses as a result, rather than simply a motivation, of his project of justification. "Thus rehabilitated," the language of the Dirac function would be useful not just in electrical calculations and mathematical physics but in many areas of mathematical analysis.

This rehabilitation required, Schwartz continued, reasoning with "elements … more general than functions." To explain what this meant, Schwartz next described the Dirac function as a mass distribution, which defined a measure that could be used to take a rigorous Stieltjes integral in lieu of the *ad hoc* symbolic integral expression Schwartz attributed to physicists.[55] Here, Schwartz relied on the fact that the Dirac function, when used by mathematicians or physicists, almost never appeared in isolation. Instead, its users deployed it as an aid to analyzing other functions or differential equations, and common conventions of calculation dictated how they were to resolve a calculation involving the Dirac function or any of its derivatives. Most immediately, when the Dirac function appeared multiplied by another function within an integral expression, the end result of the integral was simply the value of that other function at the point 0. Denoting the Dirac function as the Greek letter δ and the other function as the letter φ, this could be represented as the expression: $\int \delta(x)\varphi(x)dx = \varphi(0)$.

Where an ordinary integral amounted to an evenly-weighted sum of all the values of a function, the Dirac function indicated that instead all the weight would be concentrated at the single point where the Dirac function leapt to infinity—thus replacing a weighted sum with a single value of the function in question. With the Stieltjes integral, Schwartz asserted that this

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[54] Schwartz's long-delayed monograph—always a year from delivery—became a running joke among his Bourbaki companions. Paumier, *Laurent Schwartz,* 61-62.
[55] On Stieltjes, see Gerrit van Dijk, "Thomas Joannes Stieltjes: Honorary Doctor of Leiden University," *The Mathematical Intelligencer* 16, no. 1 (1994): 52-53.

kind of operation—re-weighting an integral to isolate particular values of a function—already had a familiar mathematical formulation with a sound justification in the field of measure theory. With this interpretation, every function entailed a measure that could be used to re-weight the integral of another function. But some measures (like the Dirac function, or any other weighting that singled out individual points) did not correspond to a conventional function. In fact, in this view, the whole apparatus of integration was no longer necessary: the Dirac function became simply a rule to replace a given function with its value at the specified point.

Next, Schwartz introduced a doublet consisting of an infinitely large positive-valued weight placed infinitesimally close to an infinitely large negative-valued weight. Interpreting these weights as he had the Dirac function, not as values of an ordinary function but as rules for operating on some given auxiliary functions, Schwartz demonstrated that this doublet corresponded to an output of the value of the function's derivative at the specified point, rather than the value of the function itself. Such an operation did not correspond to a Stieltjes integral, much less to an integral with an ordinary function. But Schwartz instructed his readers to interpret the doublet instead as a linear functional—yet another abstraction from the theory of functions and measures that described any rule whatsoever that gave some numerical output when applied to a given function, subject to some conditions for mathematical consistency.[56]

Schwartz then formally defined the space of smooth compactly-supported functions of n variables and defined n-dimensional distributions as continuous linear functionals (denoted with the letter T) that applied to such functions (denoted φ). When the distribution was a measure

[56] A detailed internalist history of the modern theory of functions, covering a wide range of alternative interpretations of functions and their generalizations, is Garrett Birkhoff and Erwin Kreyszig, "The Establishment of Functional Analysis," *Historia Mathematica* 11 (1984): 258-321.

(denoted μ) or a function (denoted f), the numerical output of the linear functional was determined by the Stieltjes integral of φ weighted by the function or measure—in symbolic notation $f(\varphi) = \int f(x)\varphi(x)dx$ or $\mu(\varphi) = \int \varphi(x)d\mu$. Under the right conditions, Schwartz stressed, distributions could be measures and measures in turn could be functions. But there were also examples of distributions that were not equivalent to measures and measures that were not equivalent to functions, and thus distributions were more general than measures which were in turn more general than functions. This unfolding of generalizations, culminating in distributions, amounted to both a historical claim and a claim about the nature of mathematical objects. Such objects, this narrative implied, could be understood in progressively unfolding contexts of increasing mathematical power and generality. Here, distributions were both a chronological and a mathematical point of arrival in a march of ever more comprehensive comprehensions of the fundamental concept of a mathematical function.

Up to the point where he defined distributions, Schwartz explained his series of generalizations for functions under the guise of justifying familiar formalisms from mathematical physics. But his article promised to generalize not just functions but derivatives as well. To define derivatives for distributions that were not interpretable as conventionally differentiable functions, Schwartz followed his justification-through-generalization in reverse. Any operation applied to distributions that happened to be functions had to give the same result as the corresponding conventional operation from integral and differential calculus, just like any operation on complex numbers that happened to have no imaginary part had to give the same result as the corresponding calculation considered only among the real numbers. Thus, the distributional derivative of a differentiable function was to match its conventional derivative. Schwartz then defined operations for arbitrary distributions (which did not, in general, have conventional derivatives) by translating a usual calculus presentation into one intelligible for linear functionals and then declaring that to be the general definition for distributions.

This approach rested on a kind of reasoning by metaphor made compelling through a combination of formal and informal wordplay. Arbitrary distributions were like functions, Schwartz implied, so operations with the one should look and feel like those with the other, using similar formalisms and terminology while calling upon similar intuitions. To define differentiation, Schwartz began with a conventionally differentiable function *f* in *n* variables and its derivative in the direction of the *i*th variable x_i (denoted with an apostrophe and a subscript variable). As a linear functional, this gave the expression:

$$f'_{x_i}(\varphi) = \iint \dots \int f'_{x_i}(x_1, \dots, x_n)\varphi(x_1, \dots, x_n)\, dx_1 \dots dx_n.$$

Then, Schwartz asserted, "there is no difficulty to integrate by parts," applying a routine technique from the conventional calculus to rewrite the expression as:

$$f'_{x_i}(\varphi) = -\iint \dots \int f(x_1, \dots, x_n)\varphi'_{x_i}(x_1, \dots, x_n)\, dx_1 \dots dx_n,$$

or, in the idiom of linear functionals, $f'_{x_i}(\varphi) = f(-\varphi'_{x_i})$. The subtle but important difference between the two integral expressions involved removing the derivative from the function *f* and, along with the introduction of a minus sign, applying the derivative instead to the function φ. Rendered in the compact formalism of linear functionals, stripped of the visual trappings and technical provisos of integration, the transformation was striking. Schwartz used this formal sleight-of-hand to define the derivative of a distribution as $T'_{x_i}(\varphi) = T(-\varphi'_{x_i})$. The left side of this equation had no prior definition, but the right side appeared mathematically sound because the functions denoted φ were presumed differentiable. To prove the definition's worth, he considered the Dirac function and its derivatives, piecewise-differentiable functions, and

Hadamard's "finite parts," each of which lacked a full justification in the ordinary calculus but could be succinctly captured in a brief explanation as distributions.

Integration by parts began, on the fifth page of Schwartz's article, as a familiar computational technique for evaluating integrals of conventionally differentiable functions. By characterizing that technique instead as an operation in the abstract calculus of functionals, within half a page Schwartz could declare that integration by parts "permits the generalization of the notion of a derivative" by applying the formal operation of integration by parts to distributions for which a conventional calculus interpretation was impossible. To accomplish this, Schwartz elided the significant conceptual distinctions between the superficially similar equations for functions and distributions, justifying the latter by portraying it as the same as the former. That is, Schwartz defined distributional derivatives by turning integration by parts from a technique into a metaphor. This only worked by stripping both of their mathematical justifications and conditions of validity and interpreting the expressions instead through their formal appearances—at root, this was wordplay.

In the theory of distributions, integration amounted to finding consistent antiderivatives, and again Schwartz presented distributions as the "obligatory" consequence of conventional but insufficiently general rules. Without referring to his 1944 paper or Choquet and Deny's, Schwartz illustrated a consequence of this for partial differential equations in terms of harmonic or polyharmonic functions. Schwartz devoted the subsequent section to different ways to combine distributions algebraically, including by convolution, and continued to refer to the Dirac function and to the calculus of ordinary functions. A final section treated distributions' topological features, particularly the convergence of different kinds of sequences of distributions and associated conclusions regarding Fourier series (while promising a future treatment of Fourier and Laplace transforms).

Two conclusions from the latter part of Schwartz's article were especially important. The first, offered without proof, stipulated that in every bounded region a distribution was a finite derivative of a continuous function. Schwartz later called this the "one absolutely essential property of distributions."[57] It meant, in particular, that distributions offered the most conservative possible generalization of functions such that every such generalized object was differentiable. That is, the space of distributions was sufficient to give every function a derivative, and since every distribution was ultimately a finite-order derivative of a function each distribution was also necessary to include in the space. Schwartz portrayed this as confirmation that his theory was the optimal culmination of efforts to rectify differentiation. The second conclusion was that while distributions were not functions they were nonetheless arbitrarily close to ordinary functions—in the terms of topological vector spaces, smooth functions were dense in the space of distributions. Schwartz did sketch a demonstration of this conclusion based on convolution with approximations to a unit impulse, a technique evident in his 1944 paper that would be central to mathematicians' future work with the theory.

Direct citations of other works in Schwartz's brief article were sparse but telling.[58] The first, on the title page, advertised Schwartz's own upcoming Cours Peccot. The next referred to a "classic theorem of F[rigyes] Riesz" and cited some pages from a monograph published in Warsaw by Stefan Banach in 1932. This helped justify Schwartz's interpretation of functionals as generalized functions. Several pages later, Schwartz referred to another 1932 publication, this

[57] Schwartz, *Mathematician Grappling*, 233 [246].

[58] I do not know of any relevant quantitative bibliometric studies, but my own unsystematic qualitative survey suggests that the number of Schwartz's citations was not unusual for mathematical articles of this period, length, and genre.

one by Jacques Hadamard on his theory of finite parts. After another interval of five pages, Schwartz cited André Weil's 1940 "general study of the composition [convolution] product." Five pages after that, Schwartz called attention almost in passing to Salomon Bochner's generalized Fourier transform in a Leipzig text of 1932, while deeming Bochner's formalism "much less manageable" than Schwartz's own.

The preponderance of texts from 1932 owed in part to coincidence and in part to a combination of personal and geopolitical forces. Among the latter, the period between the wars was a time of significant upheaval in international mathematical publishing, with political and military events in the latter part of the 1930s and early 1940s particularly affecting publication and circulation in Europe.[59] In France from 1940-1944, war and occupation saw many mathematics library collections lost or damaged, while making it difficult to acquire new literature.[60] Schwartz himself began his military service in October 1937, a few months after graduating from the École Normale Supérieure, and was again cut off from the mathematical literature while in hiding during the latter half of the German occupation.[61] The 1932 volumes were among the most modern and up-to-date expository monographs available to Schwartz during the formative years of his longest sustained study of mathematics to date. Independent of how he encountered them, the volumes' 1932 dates also signaled that Schwartz's work was

[59] Reinhard Siegmund-Schultze, "The Emancipation of Mathematical Research Publishing in the United States from German Dominance (1878-1945)," *Historia Mathematica* 24 (1997): 135-166, on 140-156.

[60] On the library of the Institut Henri Poincaré, see Reinhard Siegmund-Schultze, "The Institute [sic] Henri Poincaré and mathematics in France between the wars," in Beaulieu, ed., *Regards,* 247-283, on 281. See also the account of American mathematicians' work with the Rockefeller-funded Committee to Aid Devastated Libraries in Kline to Bok, 17 Jul 1947, AMS Records, box 32, folder 103. The urgent task of rebuilding libraries after the war appears regularly in postwar reconstruction discussions.

[61] Schwartz, *Mathematician Grappling*, 125, 228 [137, 241].

current without being so new as to be tentative, that it was firmly supported by antecedents without being reducible to them.

The most prominent antecedent for distributions in both the 1945 article and Schwartz's 1997 autobiography was the Dirac function, along with its derivatives. These provided both straightforward illustrations and justifications of basic features of the theory and a ready-made case for the theory's usefulness and relevance. Also as in 1997, Hadamard's finite parts gave Schwartz in 1945 a striking example of the explanatory power of distributions. Schwartz credited Weil, but did not otherwise signal a role for the Bourbaki collaborators who played such a central role in his training and his approach to differential equations and topological vector spaces. Choquet and Deny appeared only between the lines in 1945, and de Rham was nowhere to be found. The latter likely became much more significant to Schwartz's story after identifying himself as an early convert to his cause.[62] Schwartz deflected Bochner in passing with a posture he would assume repeatedly (and at greater length) over the years. If Schwartz knew of the other ostensible forebears for his theory they were not in evidence in the 1945 article. Crucially, apart from Bochner's formalism, Schwartz assembled his corpus of antecedents from theoretical applications of distributions rather than alternatives to the theory. In 1945, distributions emerged from a fount of need rather than a fount of innovation.

Making Senses of Distributions

The litany of antecedents from Schwartz's 1945 article fit together under the umbrella of distributions in a way that hinged on a dual meaning of the word "sense" ("sens" in French). The first meaning came in references to "the usual sense" of terms, as in "the function's … derivative

[62] Ibid., 239 [251].

in the usual sense of the word." Here, "sense" indicated a meaning, a way of understanding and using a term that might elsewhere be understood and used differently. From the late nineteenth century to the mid-twentieth century a significant number of mathematicians and philosophers devoted their attention to the relationships between mathematics and the linguistic means and metamathematical frameworks by and through which it was articulated.[63] In mathematics and mathematical logic, for instance, figures like Rudolf Carnap of the Vienna Circle found a particularly rich setting for their considerations of the possibility of a stable, context-independent observation language for empirical science and rigorous scientific thought.[64] Such efforts shaped the practice and rhetoric of most mathematicians—perhaps not as much or as fundamentally as the metamathematicians would have aimed to do, but in ways that were undeniably significant and lasting. One readily observed consequence in many areas of mathematical writing was a proliferation of formulations, like Schwartz's first usage of "sense," that specified a precise frame or context for mathematical terms which often had competing definitions or varying interpretations across different schools and areas of research. Even within particular lines of inquiry, mathematicians often re-specified the contexts of and interpretations of key terms in search of more robust or fruitful foundations for particular theoretical aims.

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[63] Leo Corry, *Modern Algebra and the Rise of Mathematical Structures*, 2nd edn. (Basel: Birkhäuser, 2004 [1996]); Ivor Grattan-Guinness, *The Search for Mathematical Roots, 1870-1940: Logics, Set Theories and the Foundations of Mathematics from Cantor through Russell to Gödel* (Princeton: Princeton University Press, 2000); Herbert Mehrtens, *Moderne – Sprache – Mathematik: eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme* (Frankfurt am Main: Suhrkamp, 1990).

[64] E.g. Rudolf Carnap, *Der logische Aufbau der Welt* (Berlin: Weltkreis, 1928). Carnap's project continues to animate investigations in the philosophy of science. Of particular note with respect to the argument I make here is Emily R. Grosholz, *Representation and Productive Ambiguity in Mathematics and the Sciences* (Oxford: Oxford University Press, 2007), 16-24, 33-60, 126-139, 227-284.

Some terminological coincidences were accidental, but more often mathematicians used them deliberately in order to advance mathematical or philosophical arguments or to suggest helpful analogies. Schwartz built a theory of derivatives and Fourier transforms that differed from the so-called "usual" contexts of these terms, principally in calculus and mathematical analysis. But Schwartz kept these terms in his theory to indicate a kind of equivalence, or at least homology, between the "usual" senses of the terms and the senses his theory gave them. By renaming the context of derivatives rather than the operation of differentiation, Schwartz signaled that the essence of differentiation would be unchanged in his theory. This was far from an obvious assertion about a theory wherein many of the most common heuristics about differentiation and its relation to rates of change, curves, and analytic expressions ceased to apply in general. Conversely, by replacing "functions" with the term "distributions," Schwartz suggested a more substantive conceptual break—one with which the many who later translated distributions as "generalized functions" would disagree in part. The term "distributions" was itself borrowed from probability, physics, and measure theory, and Schwartz's distributions shared many conceptual and practical features with the corresponding distributions in these other settings. To emphasize Schwartz's particular context for "distributions," especially in situations where other formulations of distributions might apply, mathematicians came to refer to "Schwartz distributions" or "distributions in the sense of Laurent Schwartz."

Schwartz's second usage of "sense" in the 1945 paper came in references to statements either "having a sense" or "not having a sense" (less literally, "making sense" or "not making sense"). Here, "sense" indicated that a formulation was mathematically sound: it was wholly justifiable, if not necessarily wholly justified in the work at hand. For example, Schwartz asserted that "here, [the derivatives] dA/dy and dB/dx always have a sense qua distributions" and later that “the product has a sense if the factors are arbitrary distributions.” This usage of sense was more particular to Schwartz and to others writing about mathematical foundations. It was an emphatic term, stressing the presence or absence of a coherent basis for an expression that might otherwise be assumed valid simply by virtue of its use in a mathematical argument.

The ambition to guarantee that every mathematical notion had a sense was a crucial feature of a wide range of metamathematical projects dating to the turn of the twentieth century.[65] For Schwartz, the most influential formulation of this ambition came from the collaborators of Bourbaki, who played a formative role in Schwartz’s higher mathematical education and soon adopted him into their number. For them, “having a sense” was almost invariably a promissory claim and expository ideal rather than a settled conclusion. The Bourbaki group’s central project was a series of textbooks titled *Elements of Mathematic* (singular).[66] The introduction to the first volume, on “Set Theory,” included instructions about language that were to guide the group’s larger work. Bourbaki’s texts were to be “formalized,” which meant that they could in principle be expressed in a simple language with “a syntax consisting of a small number of unbreakable rules.” However even basic mathematics, for instance “ordinary algebraic calculation,” was rarely so codified in practice. Instead, Bourbaki

[65] It is possible to read similar goals into earlier mathematical debates as well, although the philosophical stakes and motivations could be quite different. See, e.g., Ravetz, “Vibrating Strings”; Barany, “God, King, and Geometry.”

[66] See Leo Corry, “Writing the ultimate mathematical textbook: Nicolas Bourbaki’s *Élements de mathématique*,” in Eleanor Robson and Jacqueline Stedall, eds., *The Oxford Handbook of the History of Mathematics* (Oxford: Oxford University Press, 2009), 565-588. The title is typically translated in English as *Elements of Mathematics* (with an *s*). Since the singular/plural distinction was important to Bourbaki, I refer in this dissertation to the *Elements of Mathematic* (without the *s*) except when citing a specific translation that renders it otherwise. On the singularity of mathematics for Bourbaki, see, e.g., Nicolas Bourbaki, “The Architecture of Mathematics,” trans. Arnold Dresden, *The American Mathematical Monthly* 57, no. 4 (April 1950): 221-232, first published in F. Le Lionnais, ed., *Les grands courants de la pensée mathématique* (Marseille: Cahiers du Sud, 1948). I also follow the convention of referring to “Bourbaki” as the singular author of the collective texts published under that name.

explained: "In general [the mathematician] is content to bring the exposition to a point where his experience and mathematical flair tell him that translation into formal language would be no more than an exercise of patience (though doubtless a very tedious one)."[67]

Such experience and flair allow the mathematician to introduce what Bourbaki identified as "abuses of language" that assume the justifiability of a claim without rigorously performing it.[68] For some, this showed Bourbaki's "slightly contemptuous" view of conventional terminology while preserving the group's ability to use the more familiar terms.[69] But for Bourbaki this was precisely the point: mathematicians should not eschew conventional terms but must always pay attention to the sense (in Schwartz's first usage) in which they made sense (in Schwartz's second usage). Bourbaki's collaborators reveled in puns, parodies, and invented histories.[70] In their intense, jocular meetings, they demanded great extremes of interpretive versatility across fields of mathematics and a confident command of the relevant nuances of language.[71] They did not oppose conventional terminology, but they ardently rejected its *naïve* or *unconsidered* use. This rejection was as much a social and cultural norm for the Bourbaki group as it was an intellectual or philosophical one.

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[67] Nicolas Bourbaki, *Elements of Mathematics: Theory of Sets* (Paris: Hermann, 1968), 7-8. N. Bourbaki, *Éléments de Mathématique, Livre 1: Théorie des Ensembles,* ch. 1-2 (Paris: Hermann, 1954), 1-2. As with Schwartz's autobiography, I refer below to the English pagination with the French pagination in brackets.
[68] Ibid., vi, 11 [iii, 6].
[69] Paul R. Halmos, "Nicolas Bourbaki," *Scientific American* 196 (1957): 88-99, on 93-94.
[70] Beaulieu, "Bourbaki's Art of Memory." On play as an aspect of mathematical communication and reasoning, see Reviel Netz, *Ludic Proof: Greek Mathematics and the Alexandrian Aesthetic* (Cambridge: Cambridge University Press, 2009); Apostolos Doxiadis and Barry Mazur, eds., *Circles Disturbed: The Interplay of Mathematics and Narrative* (Princeton: Princeton University Press, 2012).
[71] See, e.g., Jean A. Dieudonné, "The Work of Nicholas [sic] Bourbaki," trans. Linda Bennison, *The American Mathematical Monthly* 77, no. 2 (1970): 134-145; Liliane Beaulieu, *Bourbaki: Une histoire du groupe de mathématiciens français et de ses travaux (1934-1944)* (PhD Dissertation, Université de Montréal, 1989); Maurice Mashaal, *Bourbaki: Une société secrète de mathématiciens* (Paris: Éditions Pour la Science, 2002).

Crucially, the abuses of language that Bourbaki admitted into mathematical writing could be quite consistent with a valid exposition. In fact, such abuses were necessary in order to avoid becoming mired in tedious verification. Moreover, by justifying mathematicians' invocations of terms with pre-existing contextual meanings, Bourbaki's stance toward abuses of language signaled the heuristic, pedagogic, and intellectual value of mathematicians' inevitable linguistic promiscuity. A term's unlicensed or senseless meanings could nonetheless aid a savvy mathematician to generate and advance valid claims, provided that mathematician was aware of the abuse of language and treated such terms accordingly. When Schwartz, borrowing Bourbaki's turn of phrase, referred to the Dirac function as an "abuse of language" in the introduction to his 1945 article, he was not condemning it but rather calling attention to its need for a sense—a sense given to it at long last by distributions.

Making sense, here, was quite compatible with seeming bizarre or magical. What mattered was the consensual stipulation of a valid mathematical context, not the immediate or obvious perspicuity of that context or its rigorous justification. Sense-making rested on a great deal of often-dense and difficult mathematical work, but in practice it was enough to know that the work had been done without going to the trouble of repeating it in every instance. Giving something a sense (like "the sense of distributions") was thus itself a sort of abuse of language—an invocation promising a justification whose validity typically rested more on habit and experience than a full tedious exercise of patience. A full explication of what a term or expression meant as a statement about distributions could be onerous, and was rarely necessary to make a credible claim to sensibility. With practice, distributions' users found it easy to manipulate terms subject to the premises and implications of Schwartz's theory, and could confidently recognize when and how it was appropriate to do so. To those not versed in the theory's substantial theoretical underpinnings, results sometimes seemed to come more from wordplay than rigorous mastery.

In his autobiography, Schwartz recounted an illustration of this phenomenon that, like the Peano quote discussed above, is likely apocryphal in its details but appears to be based closely on documentable events. In 1937-1938, Ralph P. Boas and some Princeton companions started a dinner-table game of devising mathematical methods for catching lions.[72] Their pseudonymous 1938 article in the *American Mathematical Monthly*, titled "A Contribution to the Mathematical Theory of Big Game Hunting," sparked a vigorous genre of self-parody among mathematicians. For instance, from the original article:

> THE METHOD OF INVERSIVE GEOMETRY. We place a *spherical* cage in the desert, enter it, and lock it. We perform an inversion with respect to the cage. The lion is then in the interior of the cage, and we are outside.[73]

In Schwartz's recollection, the parody originated with another Princeton stalwart and Boas co-conspirator, John Tukey (whose name Schwartz spelled as "Tuckey"), who supposedly published a much longer list of methods in the *Duke Mathematical Journal*. Schwartz recited a version of the proof by spherical inversion, adding some additional mathematical stipulations not present in the original piece, and then claimed the article to include a further method from the theory of distributions:

> Anything in the desert is a lion, but in the sense of distributions. For example, a stone is a lion in the sense of distributions. So it suffices to regularize it by a C^∞ [smooth] function

[72] Gerald L. Alexanderson and Dale H. Mugler, eds., *Lion Hunting & Other Mathematical Pursuits: A collection of mathematics, verse and stories by Ralph P. Boas, Jr.* (Washington, DC: Mathematical Association of America, 1995), 9-10, 26.
[73] H. Petard, "A Contribution to the Mathematical Theory of Big Game Hunting," *The American Mathematical Monthly*, 45, no. 7 (1938): 446-447, on 446.

> with compact support, and it becomes a real lion, which furthermore is infinitely differentiable![74]

There were, indeed, many published variations on the original 1938 article (which appeared before Schwartz's theory was even invented, much less known in the United States), and it is likely that many more variations circulated informally as self-published parody offprints.[75] None of the published variations, however, appeared in the *Duke Mathematical Journal*, nor did any of those I have seen include a method for distributions. So Schwartz's attribution, if not simply a creative misrecollection, probably originated in a mock-offprint. In Schwartz's variation on lion-hunting, distributions offered mathematicians a sense in which everything was what one wanted it to be, just one step removed from the real thing. Applied to functions and their derivatives, the effect could be profound. Applied to lions, sheer nonsense.

Lion Hunting was an effective parody because this kind of "abuse of language" could be found in virtually any kind of mathematics. When mathematicians sought to understand work outside of their dedicated specialisms they really did encounter explanations that could appear like the outrageous proofs from the article—perhaps containing familiar terms but using them to strange and seemingly unaccountable ends. Schwartz promoted his theory in a context where mathematicians plied familiar and novel formal manipulations in ways that made sense only when accorded a proper interpretive framework. Importantly, in this context it was not just common but often necessary for mathematicians to move between senses of an object, arguing for instance that those senses were mathematically equivalent while allowing complementary insights and technical transformations, or that one sense entailed another as a generalization or specification. Such a proliferation of senses, and of arguments found in the intersection of

[74] Schwartz, *Mathematician Grappling*, 247 [259-260].

[75] Seven published versions are reprinted in Alexanderson and Mugler, eds., *Lion Hunting*.

meanings from different contexts, made for an uncanny prevalence of wordplay in a discipline vaunted for its linguistic precision. The lion-hunting parody lampooned a modern mathematics that appeared to many like an exercise in making sense from licensed puns.

A Ritual Phrase

The slippage licensed by the language of senses appeared to some to be particularly pronounced among physicists. Those critical of physicists' unthinking use of a theory that rescued their methods observed that Schwartz's terminology of justification and rehabilitation became a kind of habitual incantation, a conjuring trick to rectify any differential equation.[76] Looking back from 1983, an early student of Schwartz's paraphrased one version of the incantation: "it may seem bizarre, but it can be justified thanks to the theory of distributions." In fact, he recounted, "a good many limited their knowledge of the theory of distributions to the ritual phrase 'd'après Schwartz, ça a un sens.'"[77]

"According to Schwartz, it has a sense" was a perversely refined distillation of what Schwartz claimed in 1945 to offer the community of physicists, with whom he tended to lump electrical engineers ("électriciens," in French) who used the Heaviside calculus in his early presentations. But where Schwartz offered a magic formula with a mathematical recipe, these profane users took just the formula and substituted Schwartz's personal authority and charisma

[76] On tricks in mathematics, see Michael Harris, *Mathematics Without Apologies: Portrait of a Problematic Vocation* (Princeton: Princeton University Press, 2014), ch. 8. Tricks have a curious place in mathematicians' value systems. Mathematicians do not necessarily consider them deceptive or disingenuous, and often associate them with a certain kind of cleverness or insight, but at the same time tricks can be viewed with suspicion or as barriers to comprehensive understanding.

[77] Bernard Malgrange, "Laurent Schwartz et la théorie des distributions," in M. R. Herman and B. Teissier, eds., *Colloque en l'honneur de Laurent Schwartz,* Palaiseau, 30 May 1983, *Astérisque* 131, no. 1 (1985): 25-33, republished in Anné, Bouguignon, and Viterbo, eds., *Supplément,* 67-74, on 67-68.

for Schwartz's technical argument. Schwartz himself encouraged this approach. He relished the spotlight his theory brought him in the wake of the Cours Peccot, and readily stepped into the role of physicists' redeemer. And even those who might have wished for a detailed technical justification would have found it hard to come by, especially through the written literature: before his 1950-1951 textbook, the bulk of the theory's formal development remained a promissory note alone.

The 1945 article and its subsequent development in Schwartz's 1946 Cours Peccot and that summer's Bourbaki Congress were, despite the 1945 article's overture, squarely aimed at mathematicians.[78] Schwartz's next major public forum, however, was a lecture on December 4, 1946, at the Society of Radio-electricians, subsequently published in the *Annales des Télécommunications*.[79] The presentation recapitulated many features of the 1945 article in a somewhat more didactic manner, systematically introducing terms and examples while eschewing some explanations that Schwartz may have deemed too particular to mathematicians. The lecture also spelled out more explicitly how Schwartz intended non-mathematicians to see and use his theory by the end of 1946.

Schwartz began the published version of his lecture by referring to his 1945 paper and to a 1947 follow-up article still forthcoming at the time of his lecture that examined Fourier transformations in more detail. He also, by then, could cite one of the book series in which his in-progress textbook would appear, the Publications de l'Institut Mathématique de Strasbourg.[80]

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[78] Schwartz claimed in his autobiography that about half the audience of the Cours Peccot consisted of physicsts and electrical engineers. Schwartz, *Mathematician Grappling*, 240 [253].
[79] Laurent Schwartz, "Généralisation de la notion de fonction et de dérivation: Théorie des distributions," *Annales des Télécommunications* 3, no. 4 (1948): 135-140.
[80] The first volume of Schwartz's textbook would become volume IX of the Publications de l'Institut de Mathématique de l'Université de Strasbourg (to which Schwartz referred with an abbreviated name in 1948) and volume 1091 of the Paris publisher Hermann's series Actualités

His theory, he announced, “permits the complete justification of certain processes in symbolic calculus (electricity) and wave mechanics” where “one boldly uses the ‘Dirac function’” and its derivatives. But, Schwartz stressed, the theory was “autonomous” and was “not limited to justifying certain formulas.”[81]

To simplify the exposition, Schwartz announced his intention to consider only functions of a single real variable, although he briefly introduced a second variable in the latter half of his presentation in order to discuss partial differential equations. As in 1945, Schwartz explained how to generalize functions to measures in order to justify the Dirac function, and how in turn to interpret measures as functionals using the Stieltjes integral. Unlike in 1945, Schwartz spelled out in greater detail the definitive properties of measures and functionals and their relation to functions, along with rationales for some of the mathematical conditions he introduced. Rather than simply refer to Riesz’s “classic theorem,” Schwartz summarized the conclusion of that “celebrated theorem” for his audience. Following the 1945 template, Schwartz next interpreted a dipole as a functional that was not a measure and then defined distributions without some of his 1945 formalities.

Tellingly, and uniquely among his presentations of his theory in this period, Schwartz did not then define the derivative of a distribution by analogy to integration by parts. Instead, he began with the difference quotient defining ordinary differentiation for conventional functions in one variable. Interpreting this in terms of translations—replacing, for example, $f(x)$ with $f(x+h)$—Schwartz then constructed a corresponding difference quotient for linear functionals and manipulated that into his famous expression $T'(\varphi) = -T(\varphi')$. Only as an afterthought did

Scientifiques et Industrielles, the latter of which was the same series in which Bourbaki’s textbooks appeared. I discuss Bourbaki’s and Schwartz’s relationship with Hermann in chapter 5.
[81] Schwartz, “Généralisation [1948],” 135.

Schwartz add that “the formula is, in fact, that of integration by parts.”[82] Here was the difference between explaining his definition to mathematicians who, at a minimum, regularly taught the technique of integration by parts to calculus students, and explaining it to those in whose professions and scholarship the technique could not be assumed to be so central.[83]

Just as telling as Schwartz’s downplaying of integration by parts was the collection of mathematical assertions he kept comparatively undiminished from his 1945 article, claiming them here for physics. He continued to stress that the framework of distributions was the minimal generalization of conventional functions for which every entity had a derivative.[84] He made note of harmonic and polyharmonic distributions, and asserted that discontinuous solutions to partial differential equations had “great physical interest” amply demonstrated in applications predating the theory of distributions.[85] These included elementary solutions to differential equations and Hadamard’s finite parts, which Schwartz claimed necessarily used distributions “at least in a camouflaged manner.”[86] Schwartz also claimed convolution and its associated differential equations for “fundamental operations of the symbolic calculus,” which “became very easy” with the advent of distributions.[87] Where Schwartz signaled an intention to develop

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[82] Ibid., p. 136.
[83] The details of undergraduate mathematical instruction as an aspect of professional service varied across different countries and kinds of institutions in this period, but the integral calculus (of which integration by parts was a central technique) was an effectively universal element of undergraduate science and engineering curricula, and increasingly present in secondary education in the developed world as well. See Luciana Zuccheri and Verena Zudini, “History of Teaching Calculus,” in Alexander Karp and Gert Schubring, eds., *Handbook on the History of Mathematics Education* (New York: Springer, 2014), 493-513.
[84] Schwartz, “Généralisation [1948],” 137.
[85] Ibid., 138.
[86] Ibid.
[87] Ibid., 138-139.

the Fourier and Laplace transforms in 1945, by the time his later article appeared he could go into some detail about both.[88]

At the same time, Schwartz laid special emphasis on his theory's consequences for the routine formal manipulations required for physical or electrodynamical calculations. Thus, he claimed that distributions "eliminate difficulties from the usual analysis" involving "special precautions" for differentiating integral expressions under the integral sign and convergent series term by term.[89] By way of conclusion, Schwartz conceded that mathematically unjustified expressions "do not at all embarrass an electrician [electrical engineer] who knows nothing of distributions" but insisted all the same that "the systematic manipulation of distributions permits evaluation with greater ease and fewer chances to commit errors." Despite its "complicated appearance," Schwartz's framework "is in reality very simple and demands just a little mathematical knowledge."[90]

But simple could also mean superfluous, and for many it was enough to know that distributions justified what they were already doing. Analyzing how the apparent audience of Schwartz's *Annales des Télécommunications* article reacted in subsequent years to Schwartz's claims, Paumier concluded that "it is not the theory of distributions that justifies the physicists' calculus so much as that the references given by Schwartz justified his theory in the sense that they gave it an obvious application."[91] Indeed, for all of Schwartz's claims that his theory would improve not just electrical engineers' and physicists' theoretical foundations but also their technical practices, there is little evidence that this came to pass in practice. However Schwartz's advocacy for his theory's use in physics did more than give him a claim to an application.

[88] Ibid., 139-140.
[89] Ibid., 137.
[90] Ibid., 140.
[91] Paumier, *Laurent Schwartz*, 150-154, quote on 154.

Beginning in the 1950s (and very much relying on claims established in the latter 1940s), Schwartz propounded an image of his theory as simple, fundamental, and meaningful for non-mathematicians in a way that let him claim some of his largest and most prolific audiences of students and researchers in both mathematics and physics. Thanks to his and others' efforts to adapt the theory of distributions, Schwartz's claim about the theory's broader relevance transformed from a dubious exaggeration to a self-fulfilling prophecy.

International Recognition

While Schwartz had a small number of early interlocutors outside of France, he and his theory only began to achieve substantial recognition from foreign mathematicians in the summer of 1947. Schwartz was among the first beneficiaries of the Rockefeller Foundation's postwar interventions to rebuild a European and trans-Atlantic scientific community.[92] While the officers of the Foundation's Division of Natural Sciences focused their postwar efforts in France primarily on biology, especially after 1949, they offered substantial support to mathematicians both directly and in collaboration with the French government.[93] Directly, they built on their interwar involvement in founding the Institut Henri Poincaré by funding foreign visitors both to

[92] See John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge: MIT Press, 2006), ch. 4; Doris Zallen, "The Rockefeller Foundation and French Research," *Cahiers pour l'histoire du CNRS* 5, 1989; Doris Zallen, "Louis Rapkine and the Restoration of French Science after the War," *French Historical Studies* 17, no. 1 (1991): 6-37. For Laurent Schwartz in particular, see Paumier, *Laurent Schwartz*, ch. 3; Anne-Sandrine Paumier, "Laurent Schwartz (1915-2002) et le colloque d'analyse harmonique de Nancy, 15-22 juin 1947," *Gazette des mathématiciens* 147 (2016): 39-51.

[93] For the direct effect of the focus on biology on mathematics funding, see Gerard Pomerat to Maurice Frechet, 16 Feb 1949, RF Projects: France, box 11, folder 118. On the Rockefeller Foundation's postwar biology programs themselves, see Jean-Paul Gaudillière, *Inventer la biomédecine: la France, l'Amérique et la production des saviors du vivant, 1945-1965* (Paris: La Découverte, 2002); Jan Sapp, *Beyond the Gene: Cytoplasmic inheritance and the struggle for authority in genetics* (New York: Oxford University Press, 1987), ch. 6.

that institute and to other sites of mathematical research, and by partially underwriting some further meeting and publication projects.[94]

The Foundation's chief collaboration with the French government was in the form of a partnership with the Centre National de la Recherche Scientifique to purchase laboratory equipment and to conduct international colloquia centered around specific topics of contemporary research.[95] Rockefeller's Warren Weaver intended the colloquia to maximize informal interaction and scientific contacts and to spread the benefits of foreign visitors and research investment to provincial universities rather than concentrating them in Paris, propounding an American model of competitive regional, national, and international scientific organization. The Foundation funded the travel of American and non-French European participants to these conferences, while the CNRS supported domestic participants and observers and local organizational costs.[96]

One of an initial pair of Rockefeller colloquia in mathematics, part of the first half dozen colloquia in all areas of the Rockefeller-CNRS collaboration, was a meeting from 15 to 22 June, 1947, on the topic of Harmonic Analysis at the Faculty of Sciences of Nancy.[97] Schwartz had joined the faculty at Nancy in 1945. The faculty's dean, Jean Delsarte, was a founding collaborator of Bourbaki and the Rockefeller-CNRS colloquium's principal local organizer. The

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[94] These are documented principally in RF Projects: France, box 11; RF Paris Field Office, box 8, folder 55. The latter folder includes documentation of the Foundation's postwar support for the "small number of exceedingly brilliant young French mathematicians" of the "Bourbaki Group" in Nancy from 1948-1951, primarily for typing, copying, and meeting expenses. On the interwar foundation of the IHP, see Siegmund-Schultze, "The Institute Henri Poincaré."

[95] On the CNRS's context in late interwar French politics and early Fourth Republican governance, see Philip Nord, *France's New Deal: From the Thirties to the Postwar Era* (Princeton: Princeton University Press, 2010), 178-179, 207-208.

[96] Krige, *American Hegemony*, 95-96, 110. Paumier, *Laurent Schwartz*, 111-114.

[97] A table of conference topics is in Zallen, "Louis Rapkine," 31-32. The second mathematics colloquium was on Algebraic Topology. On the choice of subject for the colloquium, see Paumier, *Laurent Schwartz*, 120-123.

meeting's roster featured prominent researchers at a range of career stages from Paris, Nancy, the United States, Scandinavia, Switzerland, and Britain, including Harald Bohr of Denmark, Torsten Carleman and Arne Beurling of Sweden, and the American Norbert Wiener.[98] As a local presenter, Schwartz helped Delsarte and his Paris co-organizer Szolem Mandelbrojt (another founding Bourbaki collaborator) with a number of arrangements. This included circulating background articles to participants in advance of the meeting, among which numbered Schwartz's own 1945 article on distributions.[99]

Paumier has characterized the 1947 colloquium as a "springboard" for Schwartz and his theory both in France and internationally.[100] Her assessment, amply justified by the surviving documents from that period, substantiates an impression that has been widely shared among those who have written about Schwartz's career, including Schwartz himself.[101] The subject of Schwartz's presentation, Fourier transforms of distributions, was the crux of Schwartz's autobiographical depiction of the final stage of realizing his theory. In his depiction, the challenge of finding a workable Fourier transform forced him to adopt a framework of linear functionals in place of convolution operators.[102]

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[98] See Paumier, *Laurent Schwartz*, 119-124. Wiener's 1930 article "Generalized Harmonic Analysis" (*Acta Mathematica* 55, no. 1: 117-258) was a major touchstone of the modern theory of Harmonic Analysis.
[99] Paumier, *Laurent Schwartz,* 124-125.
[100] This claim, running from the Nancy colloquium to Schwartz's 1950 Fields Medal, will be fully argued and documented in Anne-Sandrine Paumier, Michael J. Barany, and Jesper Lützen, "From Nancy to Copenhagen to the World: The internationalization of Laurent Schwartz and his theory of distributions," in preparation, based in part on the authors' presentations at the conference "Le centenaire de Laurent Schwartz: analyses et temoignages," École Polytechnique, Paris, 3 Nov 2015.
[101] Paumier, *Laurent Schwartz*, § 3.3. Paumier's account includes a survey of a considerable range of mathematicians' post-hoc presentations of Schwartz and the 1947 colloquium.
[102] Paumier (131-132) identifies a further direction in Schwartz's work immediately following the colloquium that went beyond Fourier transforms of distributions and first appeared in print as Laurent Schwartz, "Sur une propriété de synthèse spectrale dans les groupes non compacts,"

The intellectual culmination signified by Schwartz's Nancy presentation coincided, in his account, with his breakthrough on the international stage. Though his autobiographical characterizations of the Nancy colloquium were ambiguous and somewhat contradictory, it is clear that Schwartz credited it for his introduction to "a vaster world."[103] Impressed with Schwartz's performance in Nancy, Danish colleagues Harald Bohr and Børge Jessen wrote to him the ensuing July 14 to invite him to lecture in Copenhagen on his "extraordinary theory of distributions." Underscoring their belief in the theory's significance "both in pure and applied mathematics," Bohr and Jessen made a point of having the trip co-sponsored by their Mathematical Institute at the University of Copenhagen and the corresponding institute at Copenhagen's Technical University (to which they referred in the letter as their Ecole polytechnique). For Copenhagen's "rather heterogeneous" mathematical community, the Danish hosts urged Schwartz to prepare an "exposition as elementary as possible" beginning with an introductory lecture "for a somewhat larger audience."[104] Schwartz made the trip from October 27 to November 2, 1947, and extended his voyage by two days to lecture across the narrow channel in Lund, Sweden, where he was hosted by Lars Gårding and also met Marcel Riesz.[105] The Nancy colloquium also helped precipitate invitations for Schwartz to London and Oxford in 1948 on the recommendation of London-based but French-trained colloquium participant Michel Loève.[106]

Comtes-Rendus de l'Académie des Sciences 223 (1946): 68-70.

[103] Schwartz, *Mathematician Grappling*, 240, 291, 301 [253, 301, 309], quote on 301 [309].

[104] Bohr and Jessen to Schwartz (in English), 14 Jul 1947, Jessen Papers, box 22, "Schwartz" folder. See also Paumier, *Laurent Schwartz*, 133-134.

[105] Paumier, *Laurent Schwartz,* 134. Schwartz, *A Mathematician Grappling*, 224 [238].

[106] Schwartz, *A Mathematician Grappling,* 302 [310].

To Schwartz's "agreeable surprise," in preparation for the October visit, Bohr had made a personal study of Schwartz's 1945 article and turned it into a course at his university.[107] Bohr also began preaching the gospel of distributions to foreign visitors to Denmark in advance of Schwartz's visit. Earlier that summer, Warren Weaver deputized New York University's Richard Courant on the latter's U.S. Navy-sponsored tour of Western Europe to assess the situation and to advise him confidentially on the Rockefeller Foundation's potential interventions in German scientific reconstruction.[108] Courant visited Bohr in Denmark from 24-27 July at the latter's summer home in Fynshav—Bohr's "first 'international' guest after the war"—during which the two discussed Bohr making a trip in the reverse direction the following spring.[109] On 11 August, Courant wrote directly to Schwartz to share that Bohr had told him "with enthusiasm about your work on differentiation and integration" which promised "strikingly general and illuminating" formulations for his group's work in New York, and offered to reimburse Schwartz the expense of sending him several photostatic copies of his papers.[110]

[107] Laurent to Marie-Hélène Schwartz, 5 Nov 1947, Schwartz family archives, reproduced in Claudine Schwartz, "Autor des premiers travaux de Laurent Schwartz sur les distributions," *Gazette des mathématiciens* 113 (Jul 2007): 112-118, on 114. See also Paumier, *Laurent Schwartz*, 134.

[108] Courant described his mission to Bohr in Courant to Bohr, 19 May 1947, Courant Papers, box 30, folder 15. Courant specified "The main point is the question whether it would be advisable to provide fellowships for young Germans to study abroad, for example in Scandinavia or even in America" but he would also be investigating "library problems," for instance. Courant notes the funding source in Courant to Bohr, 30 Apr 1947, in the same folder. The travel diary for the trip is in Courant Papers, box 25, folder 13. Courant was joined in his reconnaissance trip by Natascha Artin, who joined Courant's NYU research group when her husband, prominent Austrian émigré mathematician Emil Artin, took a position at Princeton University in 1946. On Natascha Artin, see Louis Nirenberg, "In Memoriam: Natascha Artin Brunswick, Editor Emeritus," *Theory of Probability and its Applications* 47, no. 2 (2003): 189.

[109] See page 29 of Courant's 1947 travel diary, Courant Papers, box 25, folder 13. There is also an undated "Note concerning a conversation with Harald Bohr" in box 30, folder 15, that appears to date to this visit. Quotation from Bohr to Courant, 13 Apr 1947, Courant Papers, box 30, folder 15.

[110] Courant to Schwartz, 11 Aug 1947, Courant Papers, box 79, folder 18.

While in Denmark and Sweden, Schwartz also heard rumors that his theory had reached Steinhaus and Stone in the United States, and theoretical physicist Jan Weyssenhoff in Krakow.[111] At the same moment, the formal invitation for Bohr's projected spring visit to the United States, jointly sponsored by Courant's institute and Oswald Veblen's Institute for Advanced Study, reached him in Copenhagen. Fully in the thrall of Schwartz's visit, Bohr accepted the invitation and listed among his possible lecture topics "the generalization of the notion of differentiability of Laurent Schwartz."[112] He added "As to the latter we have just now the great pleasure of having Schwartz among us and are all very enthusiastic as well about his theory and its wide scope as about his most sympathetic and inspiring personality."[113] Bohr confided to Asger Aaboe soon thereafter that "I intend to propagandize strongly for [Schwartz's] eminent contribution to the classical and integral calculus in the United States."[114] Bohr's lectures in Copenhagen and then New York on Schwartz's theory were likely the first expositions of Schwartz's theory outside of France and Western Europe, respectively.[115] Following Bohr's New York presentation, Courant wrote to Schwartz to say that "Our mutual

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[111] Laurent to Marie-Hélène Schwartz, 5 Nov 1947, Schwartz family archives, reproduced in Schwartz, "Autor des premiers travaux."

[112] Bohr to Courant, 1 Nov 1947, and Courant to Bohr, 27 Oct 1947, Courant Papers, box 30, folder 15. Peter Lax (personal communication, 27 Mar 2014) recalls Bohr introducing Courant's group to distributions on that visit. Jessen, like Bohr, overflowed with enthusiasm regarding the theory of distributions and Schwartz's manner of presenting it in a letter to Schwartz, who found his Danish adulation a touch excessive. Laurent to Marie-Hélène Schwartz, 5 Nov 1947, Schwartz family archives, reproduced in Schwartz, "Autor des premiers travaux." On the emergence of New York University as a leading center of applied mathematics under Courant, see Alma Steingart, *Conditional Inequalities: American Pure and Applied Mathematics, 1940-1975* (PhD Dissertation, Massachusetts Institute of Technology, 2013), ch. 3.

[113] Bohr to Courant, 1 Nov 1947, Courant Papers, box 30, folder 15.

[114] Bohr to Aaboe, Nov 1947, translated in Lützen, *Prehistory,* 160.

[115] Bohr's older brother Niels played a similar role before the war in bringing Lise Meitner's model of fission across the Atlantic. See Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, 1986), 261-271. For details of Harald Bohr's itinerary, see also Bohr to Veblen, 1 Nov 1948; Bohr to Veblen, 31 Jan 1948, IAS Mathematics, box 3, folder 3.

friend Harald Bohr gave here a most inspiring lecture the other day about your beautiful theory of differentiation, and independently of this we talked about you and your work." Courant added that he hoped to meet Schwartz soon as well as bring him to New York as a visiting lecturer for "at least a few months," as "Harald was very enthusiastic about the idea of your visiting this country, and he thinks much mutual advantage could come of it."[116] Beyond formal presentations, Bohr took every occasion to propagandize behind the scenes. On a side trip to Montreal, he regaled mathematicians gathered at the apartment of William Lloyd Garrison Williams, a Quaker born and trained in the United States who made his career in Canada at McGill University, with an account of Schwartz's theory.[117]

An early hint of Schwartz's formulation, however, had already in 1947 reached most American sites of mathematical research (and many beyond the United States) in the form of a brief entry on Schwartz's 1945 article in the American Mathematical Society's new abstracting journal *Mathematical Reviews.* Schwartz's article was assigned to Harvard-trained Cornell mathematician Harry Pollard, who signaled that Schwartz proposed new definitions for "functions" and "derivatives" (both enclosed in quotation marks in the review) in order to justify the Dirac function and its derivatives.[118] Pollard's ambivalent summary, wherein the term "distributions" did not appear, nonetheless signaled at least to those interested in harmonic analysis where they might look for a new approach to the calculus of harmonic functions and Fourier transforms.

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[116] Courant to Schwartz, 26 Apr 1947, Courant Papers, box 79, folder 18.
[117] Kurt Ramskov, *Matematikeren Harald Bohr* (Aarhus: Institut for de Eksakte Videnskabers Historie, Aarhus Universitet, 1995), 365. I thank Jesper Lützen for this reference.
[118] H. Pollard, MR0018259 (8,264e), 1947, accessed on MathSciNet.

It would take another two years for the CNRS to publish the proceedings of the Nancy colloquium.[119] However Schwartz published a revised and somewhat elaborated version of his chapter from the proceedings in the 1947-1948 volume of the *Annales de l'université de Grenoble*, the same publication that received his 1945 article.[120] This revised article began with a summary of the 1945 piece and then introduced the spaces of spherical test functions and their corresponding spherical distributions, denoted *S* and *S'* respectively, which offered "the domain *par excellence* for harmonic analysis."[121] In the 1947 article, to a greater extent than in the CNRS proceedings, Schwartz stressed the formal similarity between conventional Fourier analysis and what he proposed for distributions. A note in that article also acknowledged the "great kinship"

[119] Laurent Schwartz, "Théorie des distributions et transformation de Fourier," *Colloques Internationaux du Centre National de la Recherche Scientifique, Analyse Harmonique: Nancy, 15-22 Juin 1947* (Paris: CNRS/Gauthier-Villars, 1949), 1-8. The Princeton University Library did not receive a copy until 1951, but Salomon Bochner checked that copy out the day it returned from the bindery.

[120] Laurent Schwartz, "Théorie des distributions et transformation de Fourier," *Annales de l'université de Grenoble* 23 (1947-1948): 7-24. The article was received by the *Annales* on September 3, 1947, and I will refer to it below by that date. The extent of verbatim correspondence and the nature and location of the changes between the CNRS and *Annales* versions of the article are consistent with the *Annales* article being a revised version of the CNRS one.

[121] Schwartz, "Transformation de Fourier [1947]," 13. In his two-volume 1950-1951 textbook (which, as in the *Annales des Télécommunications*, Schwartz forecast under the series Publications de l'Institut Mathématique de Strasbourg) Schwartz substituted the adjective "tempered" for "spherical," emphasizing sub-exponential growth in place of extensibility to the Riemann sphere, while retaining the notation of *S* and *S',* a terminological switch Irving Segal noted in his review of the volume in *Mathematical Reviews,* MR0041345 (12,833d). See Lützen, *Prehistory*, 158; Paumier, Barany, and Lützen, "From Nancy to Copenhagen to the World." By the mid-1950s the "S" had come for stand for "Schwartz." See, e.g., R. E. Edwards, "On Factor Functions," *Pacific Journal of Mathematics* 5 (1955): 367-378. Robert Edmund Edwards had corresponded with Schwartz as early as 1952 (Schwartz Papers IX.B.I.1.1.32), and may well have met him while a lecturer at Birkbeck College, London, when Schwartz traveled to London in 1948 (at which point Schwartz still referred to *S* with "spherical").

between Schwartz's approach and that from Bochner's 1932 work, albeit "under a different formalism."[122]

The same year the CNRS proceedings appeared, the 1947 article was reviewed in both *Mathematical Reviews* and the *Zentralblatt für Mathematik,* the latter of which had been revived in Berlin in 1947.[123] For *Mathematical Reviews*, Irving Segal of the University of Chicago briefly noted Schwartz's definition of spherical distributions and some of its claimed applications and closed by observing that "A somewhat similar generalization of Fourier transform theory is due to Bochner," citing the latter's 1932 work.[124] For the *Zentralblatt*, meanwhile, Freiburg mathematician Gustav Doetsch offered a two-and-a-half page review—exceptionally long and detailed by the *Zentralblatt*'s standards—praising both Schwartz's 1947 and 1945 papers as offering a "new way" that replaced "classical analysis" and made difficulties surrounding the Dirac function, for instance, "disappear completely."[125] Doetsch, who directed a significant share of wartime mathematical research from his post in the German Aviation Ministry, was at the time of this review still suspended from the University of Freiburg as a result of the institution's denazification program.[126] Though the documentary record on Doetsch's Nazism is unclear,

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[122] Schwartz, "Transformation de Fourier [1947]," 10.
[123] The revived *Zentralblatt* maintained editorial headquarters in East Berlin while publishing from West Berlin, even after the Berlin Wall was erected, despite the attendant logistical complications. See zbmath.org/about (accessed February 2015). Cf. the similar arrangement for the *Chemisches Zentralblatt* discussed in Michael D. Gordin, *Scientific Babel: How Science Was Done Before and After Global English* (Chicago: University of Chicago Press, 2015), 280-283.
[124] I.E. Segal, MR0025615 (10,36a), 1949, accessed on MathSciNet.
[125] G. Doetsch, Zbl 0030.12601, *Zentralblatt für Mathematik* 30 (3 Jan 1949): 126-129, accessed on zbMATH. It is likely that Harald Bohr referred to Doetsch's review when preparing his remarks on Schwartz, discussed in the introduction and chapter 5, since he misattributes Schwartz's "propaganda tract" to 1948, the date given at the top of Doetsch's joint review of the papers dated 1945 and 1947-1948.
[126] Volker R. Remmert, "Mathematicians at War, Power Struggles in Nazi Germany's Mathematical Community: Gustav Doetsch and Wilhelm Süss," *Revue d'histoire des mathématiques* 5 (1999): 7-59, on 37-43, 49. Doetsch was reinstated as professor in 1951.

there was no doubt of his zeal for mathematical applications.[127] With distributions rhetorically poised on the intersection of pure mathematics, physics, and engineering, that zeal helped Doetsch become one of Schwartz's most ardent German promoters in the early postwar years.

Between Segal's and Doetsch's reviews one can interpolate two meanings of anticipation that by 1949 governed mathematicians' reception and presentation of the theory of distributions. For Segal, it was important that Schwartz's theory was anticipated in that it brought together and perhaps even improved upon its many antecedents. For Doetsch, as for Jessen and Bohr in Copenhagen, distributions were a cause for anticipation—that is, hope and excitement (to which one might add the converses of anxiety or fear). At an uncertain time for international mathematics after the Second World War, amidst the first furtive efforts to rebuild, Schwartz was a member of a younger generation of mathematicians offering something that seemed to break with the ungrounded past in favor of a well-founded future. In Doetsch's words, distributions "gave citizenship" to the Dirac function and other concepts that lacked meaning in classical analysis. His word choice here cannot have been accidental: citizenship for unmoored concepts meant something particular in a defeated and divided Germany whose inhabitants faced their own questions of citizenship. If the Dirac function could come at last into port, perhaps so too could the world's mathematicians.

A Theory from Zero

Schwartz's first trip by airplane took him in the summer of 1949 from Paris to London, to Ireland, to Iceland, to Greenland, to Labrador, to Montreal, Canada.[128] From there, he joined some Canadian mathematicians on a cross-continental train to Vancouver, "just about" learning

[127] Ibid.
[128] Schwartz, *Mathematician Grappling,* 305-306 [314].

colloquial English along the way.[129] In Vancouver, he was among four speakers to headline the second quadrennial meeting of the Canadian Mathematical Congress and to teach a course of eight lectures on research topics at its associated summer seminar for university staff and graduate students from August 16 to September 10, 1949.[130] The gathering was part of a concerted postwar effort by mathematicians in Canada to develop a robust domestic research scene that took advantage of the Canadians' proximity to the United States without relying upon it.[131] The Vancouver event was the Congress's first meeting outside of the leading centers of Canadian mathematics in Toronto and Montreal. It was meant "to combat the isolation caused by our great distances […] and to create pride in the work being carried out in our country."[132]

The Canadian mathematicians' sense of comparative isolation, both from each other and (with some significant exceptions) from a broader international community, gave many a motive to cultivate disciplinary and institutional ties to interested non-mathematicians, including physicists and actuaries. These connections had strong intellectual and financial consequences for the Canadian Mathematical Congress.[133] In the latter category, the Congress was notable in winning financial support from "almost all the Canadian life assurance companies and chartered

[129] Ibid., 306 [315].
[130] *Comptes Rendus du Deuxième Congrès Canadien de Mathématiques / Proceedings of the Second Canadian Mathematical Congress, Vancouver, 1949* (Toronto: University of Toronto Press, 1951).
[131] On the founding and early history of the Canadian Mathematical Congress, which later became the Canadian Mathematical Society, see E. S. Keeping, "Twenty-one Years of the Canadian Mathematical Congress," in Peter Fillmore, ed.,, *Canadian Mathematical Society / Société mathématique du Canada, 1945-1995, Volume / Tome I: Mathematics in Canada / Les mathématiques au Canada* (Ottawa: Canadian Mathematical Society, 1995), 45-90.
[132] *Comptes Rendus / Proceedings*, ix-x.
[133] E.g. Gillson to James, 6 Dec 1946; James to Gillson, 13 Mar 1944; Gillson to James, 2 Mar 1944, with enclosed "Notes for a Discussion on the Development of Applied Mathematics at McGill University"; Williams to James, 7 Jun 1944; Williams to James 21 Feb 1944; Mathematics Department to James, 5 Jan 1944, McGill Mathematics Subject File.

banks, and a large number of industrial and engineering companies."[134] Among the intellectual effects of such arrangements, the Congress's seminars and lectures reflected a particular focus on mathematical physics and related areas of applied mathematics. These areas were an explicit theme of the 1949 meeting at which Schwartz spoke. Though the Vancouver seminars took place in English, the Canadian mathematicians also navigated their country's official French-English bilingualism with a sensitivity toward linguistic inclusion.

Direct documentation for Schwartz's invitation does not appear to have survived, but a few facts can be inferred. In all likelihood, McGill's Williams received Schwartz's name from Harald Bohr after the latter's 1948 side-trip to Montreal, and Bohr's advocacy also most likely accounts for Børge Jessen's participation in the Congress.[135] Even after issuing the invitation, Williams knew precious little about Schwartz, and wrote fruitlessly to request biographical details from the equally-clueless officers of the American Mathematical Society, who could only refer the Canadians to André Weil for further information.[136] As they learned more about Schwartz, the Canadian organizers must have been very pleased. Here was a charismatic young French mathematician who positioned his theory at the intersection of abstract mathematics and theoretical physics, a perfect fit for a national mathematical community just beginning to assert a distinctive role for itself in the broader postwar discipline.

That summer in Vancouver, the Dirac function's most famous user, English mathematical physicist Paul Dirac himself, first met the function's supposed redeemer. The two bonded over

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[134] *Comptes Rendus / Proceedings*, xxvii. W. L. G. Williams was a particularly active seeker of such support, and would use this experience to shape the American Mathematical Society's financial positioning.

[135] Williams initially invited Bohr himself, who declined. See Bohr to Coxeter, 20 Oct 1948, Coxeter Papers, box 3, file 6.

[136] Williams to Kline, 2 Aug 1949, and Kline to James, 9 Aug 1949, AMS Records, box 36, folder 4.

fishing and hiking, recalled Schwartz, and Dirac found Schwartz's lectures at least sufficiently engaging to remain awake for their duration.[137] In addition to Schwartz and Dirac, Polish mathematician Antoni Zygmund of the University of Chicago and Indian mathematical physicist Homi J. Bhabha of the Tata Institute of Fundamental Research rounded out the "Lecture Series on Research Topics."[138] Each of these dignitaries except Dirac also gave a lecture, printed in the proceedings, on recent mathematical research from his home country. Dirac, whose home country and its mathematical scene were presumed to be more familiar, lectured instead on "The relation of Classical to Quantum Mechanics."

Schwartz based his eight-lecture contribution to the summer seminar on his 1948 *Annales des Télécommunications* article, an English translation of which was appended to the typed notes that accompanied his lectures. Where the *Télécommunications* article claimed the comparative simplicity of the mathematical background required for the theory of distributions, Schwartz's lecture notes declared that "it is possible to start the theory from zero."[139] The theory, Schwartz claimed, offered "for young students and engineers" a "considerable simplification, without requiring great mathematical insight."[140] In the four years between Schwartz's debut article on distributions and his Vancouver seminar, he developed a way to frame an abstract and potentially

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[137] Schwartz, *A Mathematician Grappling*, p. 309 [317]. Schwartz's reported interactions are not typical of most others' experiences of Dirac, who was famously shy. See Graham Farmelo, *The Strangest Man: The Hidden Life of Paul Dirac, Mystic of the Atom* (New York, Basic Books, 2009).

[138] *Comptes Rendus / Proceedings*, x. Zygmund previously lectured at McGill University on Williams's invitation. Williams to James, 7 June 1944, McGill Mathematics Subject File.

[139] Laurent Schwartz, *Theory of Distributions [CMC],* 1949, Courant Papers, box 79, folder 19, 1. Like his references to "abuses of language," to "start from zero" was another of Schwartz's methodological Bourbakisms, present as early as the group's 1934 plans for their textbook. See Beaulieu, *Bourbaki,* I: 150.

[140] Schwartz, *Theory of Distributions [CMC]*, 2.

difficult intervention from the theory of topological vector spaces as something any student might learn and, indeed, find useful.

This transformation, the seeds of which can be seen in the introduction to Schwartz's 1945 article, was one consequence of the particular relationship between distributions and their antecedents Schwartz advanced in those years. Because the theory was, in this view, a culmination of efforts in areas as diverse as atomic physics, electrical engineering, topology, and differential equations, its advocates could claim the mantle of any of these according to audience and intention. It was precisely because Schwartz never presented distributions "from zero" that he could set his theory simultaneously at the foundation and leading edge of his discipline. At the Rockefeller-CNRS colloquium, Schwartz worked at the forefront of elite research in harmonic analysis. In Vancouver, he simplified and clarified an old and established formalism. At both, such a range of antecedent-inflected framings made it possible for Schwartz to present a theory of broad interest and promise. From both, Schwartz won acolytes and launched international ventures that redounded to his career and his theory alike.

I have argued that Schwartz's polyvocal presentation of distributions—at once simple and theoretically sophisticated, an intervention in pure mathematics, physics, and engineering—held together because of the particular kind of sense-making Schwartz demanded of his audiences. Although he claimed to offer something new, his theory could have such a broad and rapid appeal because it amounted to a reframing of several distinct ideas and practices that had come before. Schwartz suggested some changes to mathematicians' conceptual approach and symbolic practices for differentiation, but the novelty of his intervention was not so much to change mathematicians' or physicists' approaches as to place them in a new context. That context and its justification could be the objects of research and deliberation in their own right, but they were not necessary for distributions' potential users to take an interest in the theory, to believe it valuable, and to consider it to have rehabilitated a collection of familiar notions and techniques whose need for rehabilitation was itself among Schwartz's rhetorical achievements. By the time of the Vancouver Congress, Schwartz's long-awaited textbook on distributions remained on the horizon. In those early years, the theory spread on the promise alone of rigorous foundations.

The theory of distributions, as a new theory, represented a particular moment in the history of mathematics and its relation to language and philosophy. As a theory (with associated techniques) whose principal feature was that it offered a new sense or context to other theories and techniques, it could thrive only insofar as sense and context had become over the preceding half-century primary preoccupations for mathematical researchers. Here, the wide body of antecedents Schwartz claimed for his theory stood as evidence of its profundity—it underwrote more by being deeper, more fundamental. Distributions arrived as a culmination, rather than as a departure or a new beginning, even though most of what the theory underwrote was obscure to Schwartz until after he claimed to have devised his theory. Though initially devised to ground an interpretation of a narrow phenomenon in the theory of partial differential equations, Schwartz's innovation became something that might unite a great range of theories and phenomena.

It was a theory for the postwar world. Mathematicians and their nations were just emerging from decades of dissensus and disillusionment. The next two chapters will show how leading mathematicians marked the start of their postwar period with earnest and far-reaching (if not always far-achieving) institutional efforts to assert a unity of principle and purpose. As a unifying theory that brought many elements of mathematics together, distributions were of a piece with new intercontinental measures to bring communities of mathematicians together. The theory was a widely anticipated invention, not because mathematicians saw it coming (Peano notwithstanding), but because it could be seen to excavate a breadth of coherence in seemingly incoherent divisions of theory and practice. Divisions were not always and inevitably what they seemed; for mathematics and mathematicians, all one needed was a new sense, a change in perspective.

Chapter 3: The Closed Circuit of Open Ideals

Reconciling Internationalisms

Internationalists across the sciences emerged from the Second World War determined not to repeat the mistakes their predecessors made after the previous World War. But they disagreed, sometimes vehemently, about what those mistakes had been and how best to avoid them. Their competing visions for international science thus reflected a range of conflicting values and priorities. Some argued, for example, for a gradual return to cross-border cooperation based in Europe without forming any new organizations to speak for all mathematicians until *all* mathematicians (or at least all the ones that mattered) could be a part of it. Others thought this was precisely backward: mathematicians had to organize formally with politically-neutral (by which many meant American) leadership as soon as possible, and use those organizations to extend mathematicians' global integration. This chapter examines how mathematicians reconciled some of these differences, avoided having to reconcile others, and joined together just enough to lay the basis for new intercontinental institutions in the Second World War's wake.

I argue that a central feature of this tandem of reconciliation and avoidance involved the fundamental rhetorical terms with which they posed the problems of international mathematics. As indicated in the Introduction, mathematicians sought a "truly international" system with virtual unanimity, but what they meant by those words varied considerably. Working within the ambiguities of that potent adjectival phrase, they angled to win allies and secure resources, to defend their prerogatives and marginalize their rivals. Between 1945 and 1949, internationalist mathematicians attempted to rally their colleagues to a compelling vision of their discipline's future in the absence of a clear articulation of that future's technical or institutional underpinnings. Their efforts turned on appropriating the past, recasting the present, and laying claim to an inchoate but inspiring future.

If the ambitions expressed in this period on behalf of international science were staggeringly grand, their detailed working out in the corridors of diplomatic and institutional authority took place at an altogether smaller scale. This chapter elaborates the dynamics and consequences of the narrow networks within which intercontinental mathematics first emerged through the intertwined organizational histories of the 1950 International Congress of Mathematicians and the postwar International Mathematical Union. It joins to these organizational histories an account of the period's new links between North and South American mathematicians—on full display in the ICM and IMU—that derived from those same networks of mathematical elites. As at the close of the Great War, a narrow cohort of closely-linked individuals from the last war's victors set the terms and the stakes for science's post-World War II order. Absent the robust intercontinental infrastructures they aimed to build, these planners and visionaries relied on the limited domestic and multinational infrastructures at their disposal, and on their personal relationships built over years of training and work at the top of their professions. These practical exigencies meant that, by and large, postwar disciplinary leaders were male and white, ensconced in secure posts at well-resourced scholarly institutions and well-connected to other elites within their disciplines and their sponsoring bodies.[1] Even those partial exceptions to this demographic generalization had to compensate by playing yet more to type in

[1] On masculinity in scientific work and professionalization, see Erika Lorraine Milam and Robert A. Nye, "An Introduction to *Scientific Masculinities*," *Osiris* 30 (2015): 1-14, on 8-12. On class and gender in mathematical theory and institutions, cf. Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003), ch. 4; Mary Terrall, "Metaphysics, Mathematics, and the Gendering of Science in Eighteenth-Century France," in William Clark, Jan Golinski, and Simon Schaffer, eds., *The Sciences in Enlightened Europe* (Chicago: University of Chicago Press, 1999), 246-271.

other areas.[2] Postwar mathematical elites marshalled assent and managed dissent through opportunistic interventions enabled by and in response to personal, political, and economic circumstances far beyond their immediate control. They traded and debated expansively inclusive ideals from within an exclusive closed circuit of access and control.

The leading men of American mathematics at the center of this story were tightly joined by their interactions through a variety of academic, philanthropic, and governmental institutions and advisory posts, especially as part of the recent war effort.[3] When faculty colleagues Marston Morse and Oswald Veblen, as representatives of the International Congress of Mathematicians and the hosting American Mathematical Society, asked the Rockefeller Foundation's Warren Weaver for funding, it mattered that they served under him on the wartime Applied Mathematics Panel, and that their respective institutions were connected by substantial funding and advisory

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[2] The most significant such partial exception for mid-century mathematicians was Mina Rees, who worked in a variety of crucial roles for the wartime and postwar organization of elite mathematics, though her work was concentrated on projects within the United States and was mostly peripheral to the specific international undertakings recounted here. As a woman, she relied especially on her educational and personal connections to elite scholars and government and philanthropic officials. See Amy Shell-Gellasch, "Mina Rees and the Funding of the Mathematical Sciences," *The American Mathematical Monthly* 109, no. 10 (2002): 873-889; Amy Shell-Gellasch, *In Service to Mathematics: the Life and Work of Mina Rees* (Boston: Docent Press, 2011). Rees is discussed at several points in Margaret W. Rossiter, *Women Scientists in America*, 3 vols., Vol. 2: *Before Affirmative Action, 1940-1972* (Baltimore: The Johns Hopkins University Press, 1995). See also Judy Green and Jeanne LaDuke, "Rees, Mina S." in *Pioneering Women in American Mathematics: The Pre-1940 PhD's* (Providence: American Mathematical Society, 2008), online supplementary material (www.ams.org, updated 2011).

[3] On the lasting effects of World War II for the funding and institutional organization of American and (less directly) international mathematics, see Alma Steingart, *Conditional Inequalities: American Pure and Applied Mathematics, 1940-1975* (PhD Dissertation, Massachusetts Institute of Technology, 2013); Michael J. Barany, "Remunerative Combinatorics: Mathematicians and their Sponsors in the Mid-Twentieth Century," in Brendan Larvor, ed., *Mathematical Cultures: The London Meetings 2012-2014* (Basel: Birkhäuser, 2016), 329-346.

relationships and a moderate commute by road or rail.[4] It mattered as well that they had many shared experiences in the tumultuous politics of interwar international science, with strong and mutually reinforced opinions about what had worked and what had failed. Their backroom politics and accommodations, while informed by their individual biographical entanglements, responded at the same time to much wider currents in their discipline and in postwar international science and politics that sustained for many a consequential conceit of common ground and shared endeavor.[5]

A small cadre of elite mathematicians tied to a small collection of institutions in the American Northeast thus made itself responsible for an international community of mathematicians, albeit a community whose form and extent differed significantly from what they had envisioned in the war's twilight.[6] Those differences, too, are instructive. Disciplinary hegemony was harder-won for the American mathematical elite than many of them thought it would be, and it afforded them less power to shape international mathematics than they might have desired. Their postwar internationalisms guided their pursuit of an intercontinental discipline, but that pursuit, in turn, tested and altered their varying internationalisms. The Congress's organizers cast themselves as the privileged guardians of a politically neutral

[4] On the Applied Mathematics Panel, under the civilian Office of Scientific Research and Development, see Shell-Gellasch, "Mina Rees," 875-879; Steingart, *Conditional Inequalities,* ch. 1. The OSRD's dramatic consequences for the organization of research in the United States are well known to historians of physics. See, e.g., Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), 297-306; Dan Kevles, "Cold War and Hot Physics: Science, Security, and the American State, 1945-56," *Historical Studies in the Physical and Biological Sciences* 20, no. 2 (1990): 239-264.

[5] Cf. Ronald E. Doel, Dieter Hoffmann, and Nikolai Krementsov, "National States and International Science: A Comparative History of International Congresses in Hitler's Germany, Stalin's Russia, and Cold War United States," *Osiris* 20 (2005): 49-76, esp. 66-76.

[6] On the broader conditions, course, and consequences of postwar American scientific hegemony, see John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge: MIT Press, 2006).

international mathematics, but had to adapt their ambitions and their actions to the far-from-neutral contexts of postwar science and politics.[7] Their story offers a different view of the well-known postwar ascent of American scientific leadership in the international arena—one driven by money and migration, but also by complex personal, practical, and political negotiations in which shifting narratives and ideologies proved decisive. It calls attention to the pliable assumptions and unequal effects of such leadership and prompts a reconsideration of the nature of American and scientific internationalism in the early Cold War.

A Few Discreet Inquiries

Just two months after the end of hostilities in the Pacific Theater, on October 17, 1945, Marston Morse wrote as chairman to the members of the Emergency Committee of the International Congress of Mathematicians to reopen planning for the Congress.[8] The letter's timing was significant. When the American Mathematical Society council suspended the Congress in 1939 and constituted Morse's committee, it resolved only that the new committee should tend the Congress's coffers and await "a more favorable time."[9] In many respects, October, 1945, was manifestly not a favorable time. Morse would soon confront a bevy of logistical, financial, and ideological challenges left in the war's wake. But in his view, the most significant obstacle of all—the only one of decisive consequence—had been the war itself. Everything else was a matter of details.

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[7] Cf. Bruno J. Strasser, "The Coproduction of Neutral Science and Neutral State in Cold War Europe: Switzerland and International Scientific Cooperation, 1951-69," *Osiris* 24 (2009):165-187.
[8] Morse "Dear Colleagues" letter, 17 Oct 1945, Morse Papers, box 7, "ICM – Emergency Committee" folder.
[9] AMS minutes, September 6, 1939, AMS Records, box 15, folder 17.

The war had, however, left behind one fundamental question that Morse posed near the top of his letter: "should the policy of an open Congress be adopted?"[10] He clarified that the very definition of an "open Congress" would require further discussion, but, however defined, Morse believed that such a Congress's feasibility was a pressing and potentially controversial matter. The question was sufficiently delicate, moreover, that Morse worried whether their foreign colleagues should even know that it had been asked. Before the war, Morse well knew, international scientific organs of all stripes struggled over the inclusion or exclusion of members from the previous war's Central Powers. For many, these were the defining conflicts of what they considered to be the clear failure of interwar international science.

Though Morse himself was not sufficiently established in the early 1920s to have taken a leading part in the initial controversies, their lasting fallout cast a long shadow over his field as he rose through the ranks between the wars. He no doubt had internalized the bitter lesson of the prior generation of American mathematical elites, that the specific matter of German inclusion could spell the life or death of an International Congress. Wartime correspondence and interwar experience led Morse and his American peers to speculate that matters would be worse when the dust settled this time around. If the French could not abide German participation in international mathematics in the 1920s after a devastating but ultimately successful confrontation in the trenches, to take one line of speculation that preoccupied Morse's committee, how would they feel after the last four years of military occupation? Mathematicians fought, bled, and died on all

[10] Morse "Dear Colleagues" letter, 17 Oct 1945, Morse Papers, box 7, "ICM – Emergency Committee" folder. On this question, see also Karen H. Parshall, "Marshall Stone and the Internationalization of the American Mathematical Research Community," *Bulletin of the American Mathematical Society* 46, no. 3 (2009): 459-482, on 470-473.

sides of the war. Could they and would they put aside that worldly animosity for the sake of trading abstract theorems?

Faced with such a weighty question, Morse did what American institutional elites of this period did: he solicited the views of a few distinguished men with a few discreet inquiries. This approach, overwhelmingly evident in bureaucratic and personal archives as well as in archive-based secondary scholarship on this period, reflects many of the facts of scientific life just noted: those in positions of authority retained robust connections to those who helped them reach and maintain those positions; most organizations relied on those prior connections to supplement or replace wholly internal consultative or decision-making apparatus; and even if inclined to cast a wider net institutional elites often lacked the infrastructural means by which to do so. Though the concentration of mundane institutional authority in such elites has been less studied, sociologists of science have examined how scientific communities establish elites who play important mediating roles both within those communities and between those communities and their relevant publics.[11] This creates the conditions for concentrating authority and prestige through self-reinforcing cycles that accrue greater power to those who already have the most.[12]

Even within the relatively small community of American mathematicians at mid-century, the movers and shakers of the national American Mathematical Society (which also sported

[11] Michael Mulkay, "The Mediating Role of the Scientific Elite," *Social Studies of Science* 6, no. 3/4 (1976): 445-470.
[12] See Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (New York: Free Press, 1977); Robert K. Merton, "The Matthew Effect in Science," *Science* 159, no. 3810 (1968): 56-63; Robert K. Merton, "The Matthew Effect in Science, II: Cumulative Advantage and the Symbolism of Intellectual Property," *Isis* 79, no. 4 (1988): 606-623; Margaret W. Rossiter, "The ~~Matthew~~ [sic] Matilda Effect in Science," *Social Studies of Science* 23, no. 2 (1993): 325-341.

regional sections) formed a narrow group.[13] The vast majority of committee members for the 1950 Congress worked within a small geographic radius of Princeton or Harvard, and a substantial share of those who did not fit that criterion had other direct ties in work or training to those institutions.[14] This included mathematicians at leading institutions in California (such as the University of California, Stanford, and the California Institute of Technology) and the upper Midwest (especially the Universities of Chicago and Michigan), whose membership in the American mathematical elite necessitated regular written or personal contact with mathematicians in the Northeast.[15] The national American Mathematical Society's regional concentration of authority reflected both the role and demands of personal meetings in the Society's operations and its reliance on existing correspondence networks based on mathematicians' personal relationships. Where the rank-and-file of American mathematics had limited first-hand exposure to Europe, the opposite held for the American elite. The bulk of the Society's early postwar inner circle was American born, but they typically boasted European travel or training. The inner circle's ranks also included a number of interwar and wartime

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[13] On the (un)representativeness of the representatives of national mathematical communities in regional and international organizations, cf. Laura E. Turner and Henrik Kragh Sørensen, "Cultivating the Herb Garden of Scandinavian Mathematics: The Congresses of Scandinavian Mathematicians, 1909-1925," *Centaurus* 55 (2013): 385-411.

[14] Official committee membership is listed in *1950 ICM Proceedings*, 1-4. A comparable picture of the centralization of training and disciplinary organization for theoretical physicists appears in David Kaiser, *Drawing theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005).

[15] Both of these regional powers would grow in influence over American mathematics in the 1950s and 1960s. See, e.g., the institutional histories in Peter Duren, ed., *A Century of Mathematics in America, Part II* (Providence: American Mathematical Society, 1989); Calvin C. Moore, *Mathematics at Berkeley: A History* (Wellesley: A K Peters, 2007). The department in Chicago had been an important hub of American mathematics at the turn of the twentieth century, as well; see Karen Hunger Parshall and David E. Rowe, *The Emergence of the American Mathematical Community 1876-1900: J.J. Sylvester, Felix Klein, and E.H. Moore* (Providence: American Mathematical Society, 1994), 275-426.

émigrés from Europe who had sufficiently high profiles before crossing the Atlantic to land in the United States at a prestigious institution.[16] As they did for any large undertaking, AMS elites divided both formal and informal work for the Congress among interlocking committees drawn from a small pool of established voices permuted according to expertise, taste, and availability.

Perhaps unsurprisingly, Morse's correspondents themselves expressed unanimous support for an open Congress of one form or another, but supposed their view might not be shared abroad. Theophil Hildebrandt, the sitting President of the American Mathematical Society, was emphatic that the Congress should be "completely international," with mathematicians invited from every country "irrespective of their political affiliations." By affirming "that science is not a political affair, but international in character," he wrote, "we make a contribution towards possible permanent peace."[17] Here, Hildebrandt adopted what would become a reliable trope for the American organizers, explicitly opposing the "political" to the "international," as though internationalism were a position free of politics. This opposition relied on a slippage, evident in Hildebrandt's formulation, that joined the "national" and the "political" by identifying one's national identity with one's membership in a national polity (for Hildebrandt, one's "political affiliation"). Conflating "political" considerations (in the sense of national polities) with "political" considerations (in the sense of one's personal and implicitly non-scientific viewpoints), Hildebrandt marked any national considerations as "political" and thus exogenous to science. As we shall see, the Americans regularly denounced or dismissed

[16] See chapter 1 and references therein on European refugees in the United States. On the earlier history of U.S.-European (especially U.S.-German) mathematical exchange, see Parshall and Rowe, *Emergence*, 147-259.

[17] Hildebrandt referred to "all countries" but doubtless intended only those with an established group of pure mathematicians. Hildebrandt to Morse, 21 Nov 1945, Morse Papers, box 7, "ICM – Emergency Committee" folder.

others' motivations as "political" in precisely these semantically ambiguous terms. By setting the "political" opposite both to science and to internationalism, often (as with Hildebrandt) in a single turn of phrase, they presented their versions of internationalism as the inevitable adjuncts to science.

In this vein, avoiding national-political identifications frequently meant eschewing individual-political ones as well. Accordingly, Marshall Stone lent his support for an open Congress with the proviso that personal invitations to the event should be eschewed altogether in favor of general invitations to "an open gathering of mathematicians."[18] Stone's proposal marked one of many more-or-less explicit contrasts between the American organizers' approach and that of the 1920 Strasbourg Congress's secretariat, which issued personal invitations to those its principal organizer Émile Picard characterized as "allies and friends."[19] The Congress's organizers ultimately made an official policy of using general rather than individual invitations, largely to avoid the problem of how best to invite German delegates.[20] Committee members could easily name respectable individual German or Japanese mathematicians, but found it difficult to stomach a wholehearted embrace of those countries' compromised institutions.

To their own confident consensus regarding an open Congress, Morse's correspondents gradually added remarks from their European contacts. Those contacts' ties to American mathematicians meant they were more likely than just any European mathematician to support an open Congress of the kind Morse's committee had begun to advocate, but their responses were encouraging nonetheless. Einar Hille quoted at length the remarks of French mathematician

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[18] Stone to Morse, 23 Oct 1945, Morse Papers, box 7, "ICM – Emergency Committee" folder.
[19] See Olli Lehto, *Mathematics Without Borders: A History of the International Mathematical Union* (New York: Springer, 1998), 33.
[20] ICM Organizing Committee 8 Sep 1948 minutes, Morse Papers, box 7, "ICM 1950 – Organizing Committee" folder.

Jean Dieudonné, affirming a non-specific opposition to Nazis without enmity toward German or Japanese mathematicians and agreeing "that there should be no exclusion on political grounds."[21] At a December 1945 Council meeting of the London Mathematical Society, Louis Mordell raised "An enquiry from the American Math. Soc. as to whether there would be objections to the presence of German mathematicians at the proposed International Congress of 1948" and the Council reported that its members "would have no such objections."[22] Swedish mathematician Torsten Carleman supposed that his Swedish and Norwegian counterparts would approve of an open Congress and promised to test the waters at the next Scandinavian Congress.[23] Perhaps, it appeared, the national grievances that had marred interwar mathematics would give way to postwar mathematicians' scientific relationships and interpersonal magnanimity. Already in the summer of 1946 Morse could put to paper his hope that an open Congress would be possible in 1949 or 1950.[24]

Money, Politics, and Scheduling

The Emergency Committee's plans took on an added urgency in October, 1946, with news from Warren Weaver of the Rockefeller Foundation, which before the postponement had pledged

[21] Hille to the Emergency Committee, 19 Oct 1945, Morse Papers, box 7, "ICM – Emergency Committee" folder.
[22] 13 Dec 1945 Council minutes, LMS Minutes.
[23] Carleman to Morse, 13 Jun 1946, Morse Papers, box 7, "ICM – Emergency Committee" folder. See Turner and Sørensen, "Scandinavian Mathematics," on the early history of the Scandinavian Congresses and their connection to regional and international mathematical organization.
[24] Morse to Belgodere, 25 Jun 1946. Morse Papers, box 7, "ICM – Emergency Committee" folder.

more funds for the Congress than any sponsor save the Carnegie Corporation.[25] Absent word from the American Mathematical Society over the Congress's status, Weaver had taken it upon himself to renew the Foundation's commitment of $7,500 toward the Congress's expenses. The renewal, however, would be contingent on the Congress taking place by the end of 1950, after which Rockefeller's commitment would lapse "rather than rest upon an argument made so long ago," and the Society would be expected to repay the $2,500 already disbursed to it.[26] Alluding (without naming the Rockefeller Foundation) to the closing window for "One of the large pledges" entrusted to the Congress, Morse wrote shortly thereafter to the Emergency Committee's members to enclose a ballot formally endorsing a summer of 1950 date, with new officers and committees to replace the Emergency Committee as soon as possible.[27] Hildebrandt assured Weaver on October 28 that the extended grant would be a great help in supporting "a congress of real international character," a congress he could now tentatively call "the International Mathematical Congress of 1950."[28] The AMS Council approved the date (August 30 through September 6, 1950) and site (Cambridge, Massachusetts) on April 26, 1947, and endorsed the Emergency Committee's dissolution the following December.[29]

Whether intentionally or not, Weaver's deadline had cut short Morse's attempts to lay a firmer foundation for the Congress before its official resumption. Behind the scenes, Morse had

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[25] Organizing Committee minutes of 28 Dec 1938, and 6 Sep 1939, AMS Records, box 15, folders 11 and 17, respectively.

[26] Weaver to Hildebrandt, 22 Oct 1946. Morse Papers, box 7, "ICM – Emergency Committee" folder.

[27] Morse to Emergency Committee, undated, Morse Papers, box 7, "ICM – Emergency Committee" folder.

[28] Hildebrandt to Weaver, 28 Oct 1946, RF Projects: U.S., box 125, folder 1546.

[29] Everett Pitcher, *A History of the Second Fifty Years: American Mathematical Society, 1939-1988* (Providence: American Mathematical Society, 1988), 148. J.R. Kline, "Secretary's Report," *1950 ICM Proceedings,* 121-145, on 121.

been working to assure Soviet cooperation, establish support from the newly-formed United Nations Educational, Scientific and Cultural Organization (UNESCO), and reach a "unanimity of opinion" regarding a proposed revival of an International Mathematical Union, the previous incarnation of which was suspended at the 1932 Zürich Congress and formally dissolved at the 1936 Congress in Oslo. In a rapidly shifting postwar environment in which Morse had already met his share of surprises and setbacks, none of these important ingredients could be assured, and each would offer further hurdles and disappointments to Morse by the time the Congress came to pass. Morse had expected as well to enlist support from the State Department and the National Academy of Sciences toward securing visas for foreign delegates. He could only hope "that a definitive announcement with regard to the Congress might help in clarifying these auxiliary questions."[30]

In fact, on the same day that Weaver penned his letter regarding the Rockefeller Foundation grant, Morse and American Mathematical Society Secretary John Robert Kline had attended a Symposium on International Relations hosted by the National Academy of Sciences in order to probe those very matters.[31] The symposium featured extensive discussions on the situation facing international scientific unions, and Morse and Kline also used the occasion to canvass foreign mathematicians in attendance on their Congress's prospects and timing. Desiring to hold "a truly international Congress" that avoided the "political character" of the interwar Union and Congresses, Morse queried the symposium's other delegates about incorporating former adversary countries. The International Union of Chemistry, he learned, had convened an

[30] Morse to Emergency Committee, undated, Morse Papers, box 7, "ICM – Emergency Committee" folder.
[31] Kline memorandum enclosed in JR Kline to Warren Weaver, 29 Oct 1946, RF Projects: U.S., box 125, folder 1546. This and the subsequent paragraph's quotations and attributions are from Kline's memorandum.

executive meeting with representatives of 21 nations and had a Russian Vice-President, but did not admit German or Japanese participants.[32]

Indications from the French mathematician Gaston Julia and his Czech counterpart Václav Hlavatý were more ambivalent. Julia worried that 1950 would be too late, while Hlavatý worried it might be too soon (at least if all nations were to participate). Julia explained that conditions in Europe meant that a transatlantic voyage could consume more than three months' wages, a cost European governments were unable to subsidize. Americans, on the other hand, could not come to Europe instead because conditions would not support an open Congress there.[33] Russians, Hlavatý explained, could hardly be convinced to come to Czechoslovakia, much less to a Congress further afield. (This would change shortly thereafter, albeit not as Hlavatý may have hoped, with a communist coup d'état in February 1948.) All hoped that German and Japanese mathematicians would be represented, and that those they termed "decent" would receive the needed subsidies for travel. The requirement for permission to enter the United States made them confident, meanwhile, that "notorious Nazis who attempt to attend the Congress" would be unable to do so. It is not clear whether they had particular "notorious Nazis" in mind, nor what kind of problem they imagined the attendance of such figures would present.

In discussions up to that point, in October, 1946, Morse's correspondents based their discussions more on extrapolations from their interwar experience than on the scant available information about postwar conditions in and beyond Europe. Thus, they debated the question of international inclusion as though it were interchangeable with that of the non-exclusion of

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[32] See the next section on American references to "Russian" versus "Soviet" mathematicians.
[33] Incumbency alone did not guarantee that the Americans would host the first postwar Congress. The 1920 Congress, for instance, took place in Strasbourg despite Stockholm's prior selection for the canceled 1916 Congress. See chapter 1.

mathematicians from the losing side of the latest World War. This view was shared, as well, in the initial contacts Morse's confidants made with European counterparts after Morse raised the question of holding an open Congress. Here, however, Morse and Kline encountered European informants who suggested that both inclusion and non-exclusion would be more complicated than they had supposed—that the dynamics that followed the Second World War might not be the same as those that followed the First. The goal of inclusion might falter through non-cooperation, interference by foreign governments, or financial constraints. Non-exclusion, meanwhile, increasingly came to refer to whom the U.S. State Department might deny entry, rather than whom the mathematicians themselves might bar. And State Department cooperation was by no means assured.

From either the angle of inclusion or that of non-exclusion, the problem of Soviet participation began to weigh heavily. Two days after the NAS Symposium, Morse circulated to his committee a draft letter intended for Sergey Vavilov, President of the Soviet Academy of Sciences.[34] The October 24 draft elaborated a litany of concerns that had been voiced at the Symposium, from the difficulties of travel, to the domestic shortage of housing, to the considerations surrounding "Nazi or Fascist" would-be delegates.[35] The draft called the committee's attention to the delicate matter of reassuring the war-aggrieved while maintaining

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[34] Morse to Vavilov, 5 Feb 1947, Morse Papers, box 7, "ICM – Emergency Committee" folder. On Vavilov, whose role in Soviet affairs is often contrasted to that of his ill-starred geneticist brother Nikolai, see Alexei Kojevnikov, "President of Stalin's Academy: The Mask and Responsibility of Sergei Vavilov," *Isis* 87, no. 1 (1996): 18-50.

[35] Morse's view of the domestic housing situation, in particular, may have been shaped by the acute local shortage in Princeton, repeatedly attested in the administrative records of the Institute for Advanced Study which I have examined. A contemporary analysis estimated the nationwide housing shortage in 1945 to be 8-12 million units for a total of some 40 million American households. P. M. Hauser and A. J. Jaffe, "The Extent of the Housing Shortage," *Law and Contemporary Problems* 12, no. 1 (1947): 3-15.

the principle of openness on which they had agreed. As a result of their discussions, Morse revised the letter to assert the principle of openness in terms of not excluding participants based on national criteria. The revised letter focused, however, on the converse challenge of assuring the inclusive participation of as many desirable delegates as possible. There is no reply from any Soviet source in Morse's papers, and it does not appear that he ultimately sent this (or any) version. Rather, the drafting exercise forced Morse and his committee to articulate and then re-articulate their values and priorities in view of new information about challenges from Europe to an open and well-attended Congress.

Reframing the Soviets and the Union

As 1946 drew to a close, Morse met with a fresh illustration of the special problems of assuring Soviet participation. That April, Princeton's Solomon Lefschetz enlisted Warren Weaver to help bring two leading Soviet mathematicians, Lev Pontrjagin and Ivan Vinogradov, to Princeton University for a star-studded conference in honor of the university's bicentennial.[36] Together with the Rockefeller Foundation's Raymond Fosdick, they sought and won State Department support for their invitations. Soviet matters, however, remained beyond their control. United States officials could only reach as far as the Soviet Academy of Sciences, and were unable to guarantee that the invitations would reach their intended recipients once passed into Soviet hands. Lefschetz's first reply from either Soviet mathematician came after the bicentennial's conclusion.

[36] This effort is documented in RF Projects: U.S.S.R., box 1, folder 9. Cf. Reinhard Siegmund-Schultze, *Rockefeller and the Internationalization of Mathematics Between the Two World Wars: Documents and Studies for the Social History of Mathematics in the 20th Century* (Basel: Birkhäuser, 2001), 133, on Lefschetz's interwar conversations with Rockefeller's Warren Weaver over Princeton, Rockefeller, and Soviet mathematicians.

Russian was formally added to the Congress's official languages in 1948, but by then the organizers mostly took for granted that Russian participation was both highly desirable and highly unlikely.[37] In the American Mathematical Society's discussion of Soviet participation, as in most considerations of Soviet mathematics and mathematicians in this period, the national moniker "Russian" was a stand-in for any Soviet mathematician, whether or not Russian-speaking, resident in Russia, or of Great Russian ethnicity. The term occasionally encompassed mathematicians in the Soviet Bloc whether or not they were from or worked in Soviet states. These conflations owed partly to many Americans' ignorance about the Soviet Union and its inhabitants. But to a significant extent the conflations also reflected the consequences of new migration and linguistic policies that tended to collapse or obscure possible distinctions. Regarding migration, early Cold War travel policies in both the United States and in the Soviet sphere of influence effectively collapsed the practical distinctions for Americans of interacting with scholars from various countries of the Soviet Bloc. When it was hard to get any scholar from any communist state to visit, and when the nature of the difficulty did not seem to depend much on the particular state of origin, there was not much value or incentive to paint the region with a fine brush.

Despite the Soviet Union's linguistic heterogeneity, publication policies made Soviet work and its producers appear to outsiders largely as monoglot Russian.[38] Not only did Soviet academics face considerable official pressure to publish only in the Russian language, but (as American mathematicians learned by early 1948) new policies from within the Soviet academic

[37] Minutes of the Organizing Committee of 16 Apr 1948, 29 Apr 1949, and 29 Oct 1949, AMS Records, box 37, folder 56.
[38] See Michael D. Gordin, *Scientific Babel: How Science Was Done Before and After Global English* (Chicago: University of Chicago Press, 2015), 220. Gordin's chapters 8-9 consider the broader American response to Russian-language Soviet publication in the early Cold War.

hierarchy had curtailed for many even the possibility of publishing summaries of their work in other languages.[39] "Most American mathematicians are unable to read Russian and are therefore deprived of some of the most important current mathematical developments which are taking place in Russia," representatives of the American Mathematical Society reported to the U.S. Office of Naval Research in March, 1948, with a characteristic verbal slide from the Russian language to the geographic location of Russia.[40] The remark was part of a plea for a large contract to translate and reprint selected Russian-language mathematical works for both civilian and military use, building on the Society's publishing infrastructure and expertise from its few research journals and its new review journal *Mathematical Reviews*.[41]

That March was a precipitous time for U.S.-Soviet relations. The outbreak of the Condon Affair signaled in the American scientific community the new and dramatic implications of quickly escalating American policies and expectations of anticommunism.[42] Edward Condon was a respected American physicist with a long and distinguished career in academia, industry, and government, including a short period as associate director of the Los Alamos Scientific Laboratory in 1943 during which he clashed with General Leslie Groves over the site's secrecy regime and military control. President Truman nominated Condon to direct the National Bureau of Standards shortly after the end of the war, and in that role he became the first high-profile

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[39] Hille and Kline to Office of Naval Research, 19 Mar 1948, AMS Records, box 34, folder 1.
[40] Ibid.
[41] AMS Records, box 34, folder 1. This contract was shepherded through the Navy by Mina Rees, the lone woman frequently found in high-level negotiations over the financing of postwar mathematics. See Shell-Gellasch, "Mina Rees."
[42] See Jessica Wang, "Science, Security, and the Cold War: The Case of E. U. Condon," *Isis* 83, no. 2 (1992): 238-269 and, for a broader academic context, Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: University of North Carolina Press, 1999); Ellen W. Schrecker, *No Ivory Tower: McCarthyism and the Universities* (Oxford: Oxford University Press, 1986).

scientist targeted by the House Un-American Activities Committee. As an advocate for open international scientific cooperation in a period of public concern over so-called "atomic secrets," he continued to draw political attention and to prompt debates among scientists over through the early 1950s, at which point his case became overshadowed by loyalty and security challenges to the still higher-profile atomic physicist Robert Oppenheimer.[43] In 1946, before HUAC denounced Condon, Morse and his American counterparts worried about reaching Soviet mathematicians. By 1948, they had to worry about whether U.S. or Soviet governments would allow travel to the Congress for even those Soviet mathematicians who were informed and willing to attend. Mathematicians interested in fostering a single, global disciplinary community had more and more reasons to fear a mathematical world cleaved in two.

It was for this reason that accessing Russian writings and mathematicians (including via "governments, national academies, and other international bodies") was near the front of Marshall Stone's rationale for reconstituting an International Mathematical Union as quickly as possible.[44] The precocious Harvard-trained Stone, whose father Harlan had recently become Chief Justice of the United States Supreme Court (where he had been an Associate Justice since 1925), moved easily among the American academic and political elite. As a leading academic mathematician with strong ties to the world of policymakers, Stone had played a central role during World War II in attempting to connect American mathematicians to both military and

[43] On atomic secrets, see David Kaiser, "The Atomic Secret in Red Hands? American Suspicions of Theoretical Physicists During the Early Cold War," *Representations* 90 (2005): 28-60.
[44] Report of the Policy Committee to the AMS Council, 5 May 1947, AMS Records, box 15, folder 112. Extract of 19 Apr 1947, NRC-CISU minutes enclosed in Fleming to Dresden, August 27, 1947, AMS Records, box 32, folder 62. On Stone's efforts toward reconstituting the IMU, see Parshall, "Marshall Stone," 471-477.

civilian arms of the United States war effort.[45] As noted in chapter 1, the joint War Preparedness Committee of the American Mathematical Society and the Mathematical Association of America, which operated with substantial financial support from the Rockefeller Foundation, was a particularly important feature of Stone's and his colleagues' outreach to potential patrons.[46] That committee became the War Policy Committee on the United States' declaration of war, and its efforts to court military and civilian patronage continued under the title of the Policy Committee for Mathematics at the war's conclusion. Through this Policy Committee, Stone now marshaled the American pursuit of a new International Mathematical Union.

Beyond such a union's practical motivations, like facilitating East-West exchanges and drumming up financial and diplomatic backing in the international arena, Stone insisted on a higher purpose "of a psychological rather than a practical order."[47] Namely, a new union "would give concrete expression to the deep-felt desire for international scientific cooperation and would be a step of incalculable importance in restoring to mathematics the international character it enjoyed before the war."[48] With this, Stone did not of course have in mind the factious patchwork of actors and institutions of the interwar mathematics community that had failed to sustain the previous International Mathematical Union. Rather, the "international character" Stone and his committee sought to "restore" was a fiction vigorously touted by Stone and his American colleagues after the Americans took the International Congress's mantle in 1936. The

[45] See chapter 1. Stone's own account of his and fellow mathematicians' efforts is in M. H. Stone, "American Mathematics in the Present War," *Science* 100, no. 2607 (1944): 529-535.
[46] See RF Projects: U.S., folder 1561, box 127.
[47] Report of the Policy Committee to the AMS Council, 5 May 1947, AMS Records, box 15, folder 112. This characterization did not sit well with every member of the AMS Council. See JR Kline to RH Cameron, 29 May 1947, AMS Records, box 32, folder 61.
[48] Report of the Policy Committee to the AMS Council, 5 May 1947, AMS Records, box 15, folder 112.

Americans, in turn, adapted their fiction from the efforts of those like Hardy and Mittag-Leffler to delegitimize Picard and his Strasbourg Congress by claiming that mathematicians' genuine and fundamental internationalism lay elsewhere. The fiction survives in large measure to this day in the "golden age" historiography discussed in chapter 1, though we shall see that it was not universally convincing when the Americans leaned on it most.

Elements of the Americans' fiction of mathematical internationalism were evident when Marston Morse met with Warren Weaver in April, 1937, to plead for Rockefeller Foundation sponsorship, and in Weaver's subsequent appropriation request that November.[49] While Morse alluded to "recent political difficulties" and Weaver explained that "even at the last International Mathematical Congress in Norway some international embarrassments were obvious and unfortunate," they still insisted, as Weaver put it, that mathematics itself was "a wholly non-contentious discipline" with respect to which genuine political strife was necessarily external. Before the Second World War, such an assertion helped mollify American mathematicians still reeling from their failure to host the 1924 Congress, a failure precipitated by the very sorts of contentions that Morse and Weaver now wrote off as trifling embarrassments. After the war, their stance gave rise to a specific and oft-repeated claim that it was in fact the unwillingness of mathematicians to accede to the political (implicitly, national) restrictions of the interwar international scientific community that led to the failure of the first International Mathematical Union in 1932.[50] That is, mathematicians were cast as *too* peaceful and non-political for their time. Implicitly, the postwar order of the United Nations and UNESCO might finally offer an international peace fit for international mathematics.

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[49] RF Projects: U.S., box 125, folder 1545. I discuss this meeting in chapter 1.

[50] See Lehto, *Mathematics Without Borders,* 23-71. Lehto mentions the American reformulation of this history in the 1940s on pp. 30-31.

Stone was the most adamant defender of this version of events. He opined in a 1941 letter to *Science,* for instance, that the Union had dissolved because mathematicians opposed its "political origins and development." On the other hand, he added, mathematicians' "informal but close cooperation" apart from the Union gave them "every reason to be pleased."[51] Stone's view became a ready dogma for those closest to him in the upper ranks of the American Mathematical Society.[52] Habitually ignoring the dramatic consequences of the Second World War and the events that led to it, Stone and his fellow Americans' tunnel vision for the politics of the 1920s and early 1930s helped them propound a picture of international mathematics (and of Americans' place within it) that was distinctly favorable to their postwar aims and endeavors. Thus, when the Carnegie Institution's John Fleming, then the sitting president of the International Council of Scientific Unions, suggested at the 1946 National Academy of Sciences symposium discussed above that the first International Mathematical Union had been disbanded because it seemed unnecessary, Morse was quick to correct him that it was abolished mainly because of its "political character."[53] Kline insisted to an AMS Council member the imperative of American leadership in re-forming the Union so as to avoid "the difficulty with the old Mathematical Union formed after the First World War," that "It was used as the 'football' of politics" and excluded the former Central Powers.[54]

In a grant report to the Rockefeller Foundation, Kline twinned the inclusive internationalisms of the planned Congress and proposed Union, affirming in words paraphrased

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[51] Marshall H. Stone, "International Cooperation," *Science* 94, no. 2432 (8 Aug 1941): 137-138.
[52] See Parshall, "Marshall Stone," 472-473 for discussions of further relevant AMS correspondence.
[53] Kline memo enclosed in Kline to Weaver, 29 Oct 1946, RF Projects: U.S., box 125, folder 1546.
[54] Kline to Cameron, 29 May 1947, AMS Records, box 32, folder 61.

from Stone that both "should be truly international, representing all national and geographical groups."[55] If the old Union supposedly failed to include all nations for the sole reason that its members adopted political criteria of exclusion, then, conversely, the new Union's advocates could claim the virtues of universal inclusion by focusing instead on a comparatively narrow goal of not excluding participants on the basis of political (i.e. national) tests. By papering over persistent obstacles to inclusion that did not originate with Picard and the International Research Council, this conflation came to provide an ideological escape hatch when the Americans came to face insuperable political barriers of their own to universal inclusion—in their case due primarily to the ideological politics of Communism and anticommunism rather than the politics of national identity as such. As evidence simultaneously mounted against the prospect of Soviet participation, the principled avoidance of a particular putative failing of the interwar Union came increasingly to substitute for discussions of the practical challenge of including *all* nations.

International Mathematical Disunion

As efforts to reconstitute an international Union progressed, characterizations of the old Union's demise and prescriptions for the new Union's future continued to turn on such pliable interpretations of internationalism and its associated prerogatives of inclusion and non-exclusion. Stone's European correspondents tended to frame the old Union's failure in terms of insufficient inclusion, and so advocated waiting until broad adherence was assured before launching a new confederation. The work of organizing a new Union, they implied, should therefore be centered in Europe where most of the potential adhering societies were located. While Stone continued to

[55] Kline report on Policy Committee to Rockefeller Foundation, 8 Jul 1947, AMS Records, box 15, folder 112.

push rhetorically for universal inclusion (or at least universal representation), his own rationales and explanations tended to stress political exclusion in the old Union's downfall. Correspondingly, he strenuously argued for a supposedly non-political American leadership to prevent a resumption of "the non-universal character" of the previous Union.[56] It would be American independence from the divisive politics of the interwar Union, for Stone, that offered the best hope for postwar unity. This contrast between Stone and his European counterparts, with their two distinct ideas of when and how to organize a new Union, came to a head in Stone's postwar tangle with the French Mathematical Society.

In the second half of the twentieth century, the International Congresses of Mathematicians and the International Mathematical Union would become increasingly conjoined institutionally. But much of the history of the Congresses up to World War II transpired in a context where an International Mathematical Union was either nonexistent or largely secondary to the quadrennial Congresses whose organization was passed from one national mathematical society to the next. When the interwar Union was in force, the Congresses furnished regular occasions to conduct Union business while Union policies and resources could affect the Congresses' planning and execution. This interlinked history offered a compelling precedent for the American hosts of the upcoming Congress to assume their priority for reconstituting the Union. That same history meant that the old Union's supposed shortcomings could be strongly associated with the groups that hosted contemporaneous Congresses.

As Stone saw it, the first Union failed to be truly international because of the petty political short-sightedness of the French hosts of the 1920 International Congress of

[56] Report of the Policy Committee to the AMS Council, 5 May 1947, AMS Records, box 15, folder 112.

Mathematicians at which it was constituted. By crafting and enacting policies of exclusion, the story went, mathematicians experienced a loss of innocence, a dramatic incursion of political discord in an otherwise non-political discipline. Postwar mathematicians in the United States and their sympathetic counterparts in Europe made their new formations out to be acts of recovery of the innocence supposedly lost between the wars, and so derived a favorable contrast between French-dominated interwar policies and what they hoped would be a postwar regime less tainted by French score-settling. They thus saw themselves as carrying forward a spirit of genuine internationalism that suffered a temporary occlusion, rather than inventing their international system anew. Instead of successors to the interwar period's tumult, they would be restorers of a fin-de-siècle international harmony.

When the postwar incarnation of the French Mathematical Society announced plans to host an informal meeting on the topic of a revived Union in June, 1947, Stone viewed it as a direct threat to the American hegemony he considered so essential in the shadow of 1920.[57] That spring, Stone claimed that he was "fearful that the motive back of the great activity of the French toward the formation of a Union is political" and aimed at "French cultural domination." Conversely, he estimated, the non-exclusive American attitude was broadly shared by the British and Dutch.[58] Americans were needed because the French could not be trusted.

France's geographic proximity to the bulk of the national groups whose adherence to a new Union would be required put the Americans at a considerable disadvantage. If the Americans were to compete successfully with the French for early control of the Union, they

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[57] Report of the Policy Committee to the AMS Council, 5 May 1947, AMS Records, box 15, folder 112.

[58] Stone testimony to NRC-CISU on 19 Apr 1947, enclosed in Fleming to Dresden, 27 Aug 1947, AMS Records, box 32, folder 62. Cf. Krige, *American Hegemony*, on American internationalists' portrayals of postwar French motives.

would have to engineer an end-run around the process of *ad hoc* negotiation and coordination among major mathematical societies that the French appeared to favor in 1947. As an alternative, Stone lobbied fervently for a path to organization that went through UNESCO and the International Council of Scientific Unions, the latter of which became quickly attached to the former after its founding, both financially and institutionally. American ICSU president John Fleming promised Stone his full support, as did Detlev Bronk, whose positions as Foreign Secretary of the National Academy of Sciences and Chairman of the National Research Council made him a crucial figure for the United States' participation in ICSU.[59] Beyond keeping the French out of the helm, ICSU sponsorship would position Stone to hold the new Union's principal organizational meeting nearer to his home turf, at the next General Assembly of UNESCO in Mexico City that November.[60] As a first step, however, ICSU's endorsement would have to be sought at its next Executive Committee meeting, which Stone could not attend because he was to be in Rio de Janeiro, Brazil, at the time. Worse, the meeting was to take place in Paris on July 1-2, 1947, just a week after the French Mathematical Society meeting. All roads to an IMU would run through France, after all.

The French effort, for its part, was an opportunistic attempt to capitalize on the inaugural pair of Rockefeller-CNRS colloquia in mathematics, in Nancy and Paris that June, discussed in the previous chapter. Under the French CNRS's arrangement with the Rockefeller Foundation's Division of Natural Sciences, the Foundation sponsored the travel of leading non-French experts for extended scientific gatherings in harmonic analysis (Nancy) and algebraic topology (Paris) as part of a larger program meant to renew international science in France after the war on a

[59] Reported in Stone testimony to NRC-CISU on 19 Apr 1947, enclosed in Fleming to Dresden, 27 Aug 1947, AMS Records, box 32, folder 62.
[60] I discuss Stone's particular connection to Latin American mathematics below.

competitive, decentralized American model. Planned for 24 June at UNESCO House in Paris, the principal delegates would be mostly francophone mathematicians (even if traveling from abroad) from the sub-disciplines represented at the conferences, supplemented by others who might make a special trip to Paris or happen to be in the area.[61]

Harvard topologist Hassler Whitney was among the distinguished foreign mathematicians due in Paris for the Rockefeller-CNRS colloquium on algebraic topology.[62] As a result, Whitney found himself anointed the Americans' "unofficial representative" at the Paris meeting. In a letter to Stone, he worried that the AMS Council's position on universal membership would meet with "divergent views" from the French hosts.[63] Stone replied at length to explain the background to the formal Policy Committee report on the prospective Union that Whitney had received. To prepare Whitney for the range of reactions he might encounter, Stone summarized the more ambivalent results of the informal international reconnaissance conducted by the Congress's organizers concerning the Union, and clarified that whether or not the Union were to be revived before 1950, the Congress would maintain its independent organization.[64] As the French meeting drew closer, Stone distributed a letter to Americans traveling abroad making the case for ICSU sponsorship and a "truly international Union representing all national and

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[61] Albert Chatelet to the President of the AMS, 2 Jun 1947, AMS Records, box 32, folder 61. See also "International Meeting of Mathematicians Convened by the Société Mathématique de France," 4 Aug 1947, Jessen Papers, box 60, "IMU: Genetablering" folder.
[62] Laurent Schwartz, *A Mathematician Grappling with His Century*, trans. Leila Schneps (Basel: Birkhäuser, 2001), 292.
[63] Whitney to Stone, 7 May 1947, AMS Records, box 32, folder 61.
[64] Stone to Whitney, 9 May 1947, AMS Records, box 32, folder 61.

geographical groups."[65] Stone, Kline, and Fleming continued to work, at the same time, to secure a favorable reception for the ICSU proposal.[66]

As it happened, the French meeting's connection with the Rockefeller-CNRS meetings ensured that Americans were especially well-represented, with the largest non-French delegation at UNESCO House that June. Rockefeller money allowed both senior and junior scholars from the United States to participate in the colloquia, and hence the Paris IMU meeting. Both Norbert Wiener and Rafaël Salem stayed on from the Nancy colloquium, joining Whitney and three others listed as from the United States in the meeting's minutes.[67] Other foreign visitors to Nancy participated in the Paris meeting as well: Ostrowski and Plancherel from Switzerland, Bohr and Jessen from Denmark, and Beurling and Carleman from Sweden. Additional participants hailed from Argentina, Belgium, Netherlands, Poland, Portugal, and Romania.

UNESCO Deputy Director General Walter Laves welcomed the group by describing "Unesco's two principal aims: to promote international understanding and the common welfare of mankind." Noting "the long tradition of co-operation among mathematicians," Laves "congratulated them on having never lost sight of this second principle." The first principle of

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[65] Stone "Dear Colleague" letter, 11 Jun 1947, AMS Records, box 32, folder 61. The letter was distributed (according to Kline to Stone, 11 June 1947) to Whitney, MIT's Norbert Wiener and Rafaël Salem, and the Institute for Advanced Study's Hermann Weyl.
[66] Fleming to Kline, 24 Jun 1947, AMS Records, box 32, folder 61. Fleming Remarks and Memoranda enclosed in Fleming to Dresden, 27 Aug 1947, AMS Records, box 32, folder 62. In the latter, Fleming borrowed Stone's "principle of representation for all national and geographical groups."
[67] "International Meeting of Mathematicians Convened by the Société Mathématique de France," 4 Aug 1947, Jessen Papers, box 60, "IMU: Genetablering" folder. Subsequent descriptions of the meeting are from this UNESCO report, which paraphrases spoken contributions to the meeting. The roster also lists the young algebraist Roger Lyndon, UNESCO Natural Sciences Program Specialist Frank Malina, and a Mr. Compton as hailing from the U.S.A.

international understanding, all could acknowledge, had a somewhat rockier history for Europe's mathematicians.

To open the meeting's business, Chatelet read letters in support of a new union on behalf of the Romanian Academy of Sciences, a Bulgarian representative, the Finnish Academy of Sciences, a professor from Vienna, and the Italian National Academy. A last letter from Marshall Stone designated Whitney as the Americans' unofficial observer while underscoring "that there had not been time to send an official delegate to Paris" on the Americans' behalf. Chatelet also announced that notices were sent to Moscow, Belgrade, and London, and "mentioned that a Mathematical Congress would probably be held in 1950 in the United States." He then listed the goals for a new Union in terms consistent with Stone's vision, albeit with different emphases: arranging symposia and collaborations, aiding "the reorganization of research in war-devastated countries," help to develop mathematics "in those countries where this was necessary," strengthening links to other scientific unions, promoting travel, and publishing mathematical abstracts.

Whitney toed Stone's party line, presenting the American Mathematical Society's relevant resolutions and expressing his personal support for an organization with "no bar to membership for any national groups." Others were more combative. Chatelet read a letter from W.V.D. Hodge "on behalf of British mathematicians" in opposition to "the immediate re-establishment of a mathematical union." Norbert Wiener "was opposed to such a union, on the ground that mathematicians preferred personal and informal contacts to official relations between societies," adding that "the Strasbourg Congress of 1920 had done more harm than good" and it would be necessary to wait until "a few years hence" when "people would be able once more to think calmly" and permit "general participation […] without any discrimination."

Harald Bohr was skeptical of the timing and warned that without the U.S.S.R. and Great Britain a union "could not, from the start, be truly international." Two Swiss participants then advised waiting until the 1950 Congress. A third predicted "that a Congress held in America would consist of 80% of Americans and 20% of Europeans" while "If it were held in Europe, the proportions would be 25% and 75% respectively," and then suggested election and policy rules to mitigate that geographic bias.[68] He also urged that an advisory committee "be as young as possible" since "the average age of [the old Union's] Executive Council was too high and in consequence the Council lacked initiative." Other participants then batted around potential projects for a union before Bohr spoke again to underscore "the necessity for a truly international union, to ensure that no restricted group should speak and take decisions in the name of <u>all</u> mathematicians." The Argentine participant, Manuel Balanzat, argued that a union could help those "at a distance from the main scientific centres" to publish books.

Participants then heard a letter "indicating Czechoslovakia's support, provided representatives of Germany were not included." Jacques Chapelon of France's École Polytechnique averred that British and Soviet participation was essential, and that Germans should not be excluded "except in certain individual cases" as "it would be extremely unpleasant to sit in an assembly where anyone was likely to state that 'space is Aryan and number of [sic] Jewish.'" Discussion continued over what intermediate steps could be taken, both at the UNESCO assembly in Mexico and elsewhere, before establishing an official union. The mathematicians then voted "unanimously, with one abstention" to express "that the formation of an international mathematical union is desirable," though only after Whitney insisted they be

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[68] The attendance prediction for the American-hosted 1950 Congress was close to the mark if one lumps Canadians with Americans and other non-Americans with Europeans (as Kline did in his summary tabulation).

clear that they were voting "in their private capacity" rather than as representatives of national societies. After five hours of discussion, the mathematicians had hashed out a range of issues and at least knew where they all stood as individuals.

The UNESCO report of the Paris meeting was full of dissent and ambivalence, but that was not the view available to Kline, Stone, and the other American organizers absent from UNESCO House. Chatelet wrote triumphantly to Kline in early July to trumpet the unanimity of favorable European opinion toward a revived Union and to suggest a further meeting (implicitly under French leadership) in October to advance that objective further.[69] Absent the meeting's official record or a preliminary account from an American participant, Kline had only Chatelet's rosy report of the proceedings—one that raised serious concerns for the American hegemony he and Stone thought so essential. "Reading between the lines," Kline averred to Stone when transmitting the letter later that month, "I would expect that the French hope that the European delegates will present a solid bloc at our meeting in November" in Mexico City.[70] Kline was unaware that two days earlier Fleming had written to Stone with unwelcome news that put the Mexico City meeting in doubt.[71] Stone's remonstrations on the importance of ICSU sponsorship, it seemed, had backfired.

The ICSU General Secretary, University of Cambridge astrophysicist F.J.M. "Chubby" Stratton, stridently objected to Stone's account of the former Union and to his interpretation of French interest in the new one. Having participated as Secretary of the "Mechanics, Mathematical Physics, [and] Astronomy" section of the 1912 International Congress of Mathematicians at his home university and having served as General Secretary of the

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[69] Chatelet to Kline, 7 Jul 1947, AMS Records, box 32, folder 61.
[70] Kline to Stone, 18 Jul 1947, AMS Records, box 32, folder 61.
[71] Fleming to Kline, 25 Jul 1947, AMS Records, box 32, folder 61.

International Astronomical Union from 1925-1935, Stratton had a deep well of direct experience with both international mathematics and the controversy-laden interwar scientific unions. His direct involvement in the International Research Council's turn away from German exclusion and its reconstitution as the International Council of Scientific Unions, for which he became General Secretary in 1937, gave Stratton a particular skepticism for a postwar revisionism that cast the interwar period as an aberrant break with an earlier era of international cooperation.[72] Thus, in response to Stone's effort to blame German exclusion on French intransigence, Stratton was adamant that the IRC's short-lived policy was instead to blame—conveniently eliding Picard and his allies' central roles in bringing about the IRC policy.

Stratton gave little credit to Stone's attempt to distance the Americans from the period's political conflicts. Rather, he suggested that the same "bitter memories" that consigned the Union to failure in 1932 *after* the end of the IRC's ban on adherence by former Central Powers had lingered in Stone's blighted view of recent French efforts. Allowing that only "countries such as the United States with no axe to grind" can check domination by any one party, Stratton doubted the "fear of French political motives" and placed a greater concern on the difficulty of enrolling the Soviets.[73] Fleming remained optimistic that a Mexico meeting could be salvaged, but agreed with Stratton's assessment that the mathematicians had best sort out their own

[72] "Proceedings of the Sections," *1912 ICM Proceedings,* 46-63, on 51. James Chadwick, "Frederick John Marrian Stratton, 1881-1960," *Biographical Memoirs of Fellows of the Royal Society* 17 (1961): 280-293. Chadwick describes Stratton as a sociable and relentlessly active astronomer, Unitarian, and Fellow of Gonville and Caius College with "a robust constitution and enviable powers of digestion" (282).
[73] Stratton to Fleming, 2 Jul 1947, enclosed in Fleming to Kline, 21 Aug 1947, AMS Records, box 32, folder 62.

conflicts internally before ICSU took any action.[74] Where the prospect of mathematicians' infighting had been a primary rationale for Stone to attempt to work instead through ICSU, from the latter's perspective it was precisely that patina of mutual suspicion that proved the mathematicians to be unready for ICSU sponsorship.

This turn of events led Stone increasingly to doubt the viability of re-establishing the Union in advance of the 1950 Congress, although he maintained that the former "might be of real help" to the latter.[75] As far as he knew that summer, both meetings in Paris had been disastrous for his own designs to lead the world's mathematicians into a new union before 1950. He wrote nearly identical letters to Harald Bohr in Denmark and W.V.D. Hodge in London, both leaders of their respective national mathematical organizations, to reiterate his inclusive international goals and the importance of UNESCO.[76] A third letter penned on the same day to Albert Chatelet suggested "the time is not quite ripe for the formation of an International Mathematical Union."[77] AMS Secretary *protem* Arnold Dresden, meanwhile, scurried to tamp down interest resulting from an optimistic press release from UNESCO that had followed the summer's French Mathematical Society meeting.[78]

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[74] Fleming to Stone, 16 Jul 1947, copies to other members of the Policy Committee, AMS Records, box 32, folder 61.

[75] Quotation from Stone to Hille, 26 Aug 1947, AMS Records, box 32, folder 61. See also Stone to Kline, 4 Aug 1947.

[76] Stone to Bohr and Stone to Hodge, 26 Aug 1947, AMS Records, box 32, folder 63. Bohr replied on 17 September and Hodge on 2 October, both in general agreement.

[77] Stone to Chatelet, 26 Aug 1947, AMS Records, box 32, folder 63.

[78] Wood to Dresden, 23 Sep 1947, AMS Records, box 32, folder 63. Stone wrote in a similar vein to the NRC Committee on UNESCO's Bart Bok on 18 Dec 1947 to minimize expectations implied in a recent Bulletin.

That October, Stone continued to plead his case to Fleming and attempt to minimize Stratton's concerns, but to little avail.[79] Absent a clear end-run around his perceived rivals, Stone moved between 1948 and 1950 to shore up support among those he thought most sympathetic.[80] If the IMU were to be revived under the leadership of Stone and the Americans, it began to look as though it would have to happen in conjunction with, not in advance of, the 1950 Congress.

Moving Money, Moving Mathematicians

What the new Union's organizers would lack in official UNESCO or ICSU patronage they recouped in monetary assistance for their delayed undertaking. This assistance redounded both directly and indirectly to the Congress as well. A May, 1946, inquiry from Joseph Needham on UNESCO's behalf regarding a new IMU became, for Marston Morse and the Emergency Committee that October, a credible prospect for financial support for the planned Congress.[81] In November, 1948, the chairman of the ICM Organizing Committee Garrett Birkhoff requested a combined UNESCO subvention of $30,000 "to aid in the transportation of non-Americans" to the ICM and to a Conference on the IMU to be arranged in conjunction with it, and reported that organizing efforts for the ICM and IMU were to remain distinct but mutually cooperative.[82]

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[79] Stone to Fleming, 13 Oct 1947; Fleming to Stone, 6 Oct and 16 Oct 1947, AMS Records, box 32, folder 63.

[80] E.g. 18 Nov 1948 Council Minutes, LMS Minutes, which report a letter from Stone "suggesting that a representative committee of British mathematicians be formed to keep in touch with his American Committee."

[81] Richardson to Needham, 18 Jun 1946, and Morse to Needham, 11 Oct 1946, UNESCO 51 A06 (73) "50": International Congress of Mathematicians — USA 1950. Needham is best known among historians of science for his work on China, and for his efforts to organize historians of science internationally. See Francesca Bray, "Eloge: Joseph Needham, 9 December 1900-24 March 1995," *Isis* 87, no. 2 (1996): 312-317; Gary Werskey, *The Visible College: A Collective Biography of British Scientists and Socialists of the 1930s* (London: Allen Lane, 1978).

[82] Birkhoff to Wang, 10 Nov 1948, UNESCO 51 A06 (73) "50."

Arnold Dresden made a special effort to lobby family friend and UNESCO Deputy Director General Walter Laves, although it is unclear whether this advocacy had much effect.[83] By the end of March, 1949, the American Mathematical Society received word that the UNESCO executive board would be considering a transportation subvention of $15,000 for the Congress and a further $10,000 for the Union conference.[84] The Americans kept news of the financial prospect to themselves, although some delegates from abroad inquired directly to UNESCO headquarters about whether the organization would be offering travel subventions.[85] That October, Birkhoff was notified of a combined $20,000 final grant.[86]

The UNESCO-subsidized travel allowances were to be distributed at the broad discretion of the Organizing Committee, with a recommendation to favor "young scientists" and those "from war-devastated countries."[87] The Congress ultimately directly subsidized the travel of over a hundred visitors, or about 30% of the total number of foreign mathematicians attending in an official capacity. Almost all individual grants ranged from $30 to $500, with most between $100 and $200 (respectively about $1000 and $2000 in 2016 dollars). By Birkhoff's reckoning (as Transportation Grants chairman), the Congress covered between a third and a half of the total travel expenses of grant recipients, who (along with those not directly funded by the Congress) pieced together the rest of their funding from home institutions and governments, American universities they visited in connection with their trips, and fellowships from international philanthropies. The total cost of direct travel grants topped $24,000 (over $230,000 in 2016

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[83] Dresden to Laves, 11 March 1949, UNESCO 51 A06 (73) "50." Dresden mistakenly addressed Laves as the Director General.
[84] Torres Bodet to Kline, 26 Mar 1949, UNESCO 51 A06 (73) "50."
[85] Temple to Martin, 1 Nov 1949, Martin to Auger, 3 Nov 1949, Auger to Martin, 10 Nov 1949, Martin to Auger, 16 Nov 1949, UNESCO 51 A06 (73) "50."
[86] Wang to Birkhoff, 18 Oct and 19 Dec 1949, UNESCO 51 A06 (73) "50."
[87] Wang to Birkhoff, 23 Feb 1950, UNESCO 51 A06 (73) "50."

dollars), nearly a quarter of the Congress's final budget (close to $1 million in 2016 dollars), with some four fifths of travel funds disbursed to those from the World War's principal Western belligerents and occupied countries.[88] This was in addition to the nearly $8,500 allocated to 25 of the 46 delegates to the IMU Conference at Columbia University held immediately prior to the ICM.[89] The UNESCO grant, claimed Birkhoff, "went far towards giving the Congress a truly international character."[90]

Costs associated with foreign delegates, which included travel, lodging, printing and distributing proceedings, and hiring an orchestra of a suitable standard to impress European guests, helped drive the four-fold increase in budget between the Congress's initial plan in 1937 and its ultimate form in 1950.[91] UNESCO ultimately accounted for nearly 10% of the direct contributions to the Congress. Another 20% came from fees assessed on full and associate members, as the Congress's dues-paying participants were called. Together, the Rockefeller Foundation and Carnegie Corporation made up another 30% of revenues, a smaller share than that anticipated for them for 1940, but twice their combined initial nominal commitment.[92]

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[88] AMS Records, box 36, folder 15. UNESCO NS/84 (Unesdoc 126553eb), 73-74. The 2016 equivalent is based on the U.S. Bureau of Labor Statistics Consumer Price Index inflation calculator, www.bls.gov.
[89] UNESCO NS/84 (Unesdoc 126553eb), 69-70. Eight delegates to the IMU received grants only from the ICM, while twenty received grants from both the IMU and ICM.
[90] UNESCO NS/84 (Unesdoc 126553eb), 73.
[91] The 1937 budget estimate is from the report of the Eisenhart committee in AMS Records, box 26, folder 71. A 1950 budget is in AMS Records, box 15, folder 140. The orchestra situation, in particular the organizers' consternation at the unavailability of renowned Boston conductor Serge Koussevitzky, is discussed in the 29 Apr 1949 Organizing Committee minutes, AMS Records, box 37, folder 56.
[92] Figures for direct contributions here and immediately following are taken from the enclosure in Kline to Weaver, 22 Feb 1951, RF Projects: U.S., box 125, folder 1546. See Barany, "Remunerative Combinatorics," for a summary table of contributions by category and industry.

Academic institutions and scientific societies, most in the Boston area or having other direct connections to the principal organizers, made a further 18% of direct contributions.

A last share of contributions, nearly equal to that from academic bodies, came from commercial sponsors. These contributors ranged from Aetna Life Insurance to U.S. Steel and United Fruit, with the largest contributions from the insurance, electrical and telecommunications, and oil industries. Prewar commercial funding for higher mathematics had been limited at best. After the war, the increased commercial support for the Congress mirrored the "increasing mathematical requirements of government and industry" that one observer saw on display in the scientific program itself.[93] These increasingly apparent requirements emerged, in no small part, as a result of the widespread American mobilization for the recent war effort. As noted above, leading American mathematicians like Morse and Stone had worked strenuously during the mobilization to promote the very roles for their discipline that were now at the forefront after the war.

The new postwar commercial ties also reflected the American mathematicians' new international posture in at least two ways. On the one hand, American ties to other mathematical organizations helped them recognize and cultivate commercial partnerships. Most significantly, as early as the summer of 1946 McGill University's William Lloyd Garrison Williams raised the prospect of trying to replicate for the Americans the success he had found courting sponsorship from the Canadian insurance industry for recent Canadian meetings.[94] The American-born-and-trained Williams eventually served as vice chairman of the Congress's Financial Committee,

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[93] C. Raymond Adams, *Mathematical Reviews*, 3 Nov 1950, in AMS Records, box 36, folder 24.
[94] R. G. D. Richardson "Dear Colleagues" letter, 17 Jun 1946, with enclosed excerpts of Williams to Richardson, 30 Apr and 13 Jun 1946; Mullins to Richardson, 24 Jun 1946, Morse Papers, box 7, "ICM – Emergency Committee" folder.

making him the only mathematician at a non-U.S. institution on any of the organizational committees. On the other hand, American mathematicians traded on their image of non-political international cooperation to present themselves as ideal vessels for corporate beneficence. As evidence of this latter motive, the Standard Oil Development Company—a technical subsidiary with its origins in Rockefeller's oil monopoly—alone dwarfed all other single categories of contributors beyond the insurance and computing and telecommunications industries.[95]

Colonial Mathematics

The 1950 Congress's organizers directed the bulk of their efforts across the Atlantic to mathematicians in established European institutions. From their official correspondence to their roster of travel grant recipients, Western Europe looms large. In terms of setting a mold for postwar connections between mathematicians across continents, however, the 1950 Congress's most important legacy may well have been the new ties it reflected between mathematicians of North and South America.[96] For Stone, in particular, such North-South connections had a triple importance for his efforts at re-establishing an International Mathematical Union. First, they offered an important pretext for his UNESCO-centered organizational approach. Second, they furnished a valuable resource to help him carry out that approach. And third, in view of his struggles to enroll European mathematicians his Latin American connections furnished his undertaking's most notable and unequivocal dividend.

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[95] On the oil industry, see 28 Apr 1949 Organizing Committee minutes, AMS Records, box 37, folder 56. On the Standard Oil Development Company, see Frank A. Howard, "The Standard Oil Development Company and Its Contribution to Chemical Technology," *Chemical and Engineering News* 22, no. 2 (1944): 98-102.

[96] See Michael J. Barany, "Fellow Travelers and Traveling Fellows: The intercontinental shaping of modern mathematics in mid-twentieth century Latin America," *Historical Studies in the Natural Sciences,* forthcoming, from which this section has been adapted.

Of the nine countries outside of Northern, Western, and Southern Europe represented at Stone's organizational conference for the new IMU held in conjunction with the ICM, four were from Latin America.[97] The Congress itself welcomed official delegations from universities and scientific societies of Argentina, Brazil, Chile, Colombia, Cuba, Mexico, Panama, Peru, Uruguay, and Venezuela.[98] In a Congress notably lacking participation from what would become the Second World, Latin American representatives of what would become the Third World offered among the chief signals of the diversity of nations the Congress's organizers had sought. Even with their transatlantic histories of travel and exchange, American mathematicians faced hurdles in reaching their European counterparts. To reach mathematicians in Latin America, the Congress's organizers relied on a more recent infrastructure established by a range of government and philanthropic bodies working to establish modern mathematics across the Western hemisphere.

Upon his pro forma election to the presidency of the 1950 Congress, Oswald Veblen referred to the interwar twilight of the "colonial period" of American mathematics.[99] The "coming of age of mathematics in the United States," as Veblen put it, was evident in the Americans' increasing intellectual autonomy, their increasing contributions of "important

[97] UNESCO NS/84 (Unesdoc 126553eb), 69. Represented countries Argentina, Brazil, and Uruguay were on Stone's 1943 itinerary, discussed below, and most of the Latin American delegates had either studied in the United States or had other dealings with the Rockefeller or Guggenheim Foundations or UNESCO. Cuba was the fourth represented country. Mexico was notably not represented at Stone's conference. A total of 22 countries were formally represented, in addition to an official observer from Israel. The other non-European countries (as defined in the grant report to UNESCO) were India, Japan, Turkey, the United States, and Yugoslavia.
[98] *1950 ICM Proceedings*, 7-20.
[99] Oswald Veblen, "Opening Address of Professor Oswald Veblen," in J. R. Kline, "Secretary's Report," *1950 ICM Proceedings*, 121-145, on 124-125, quote on 124. Where the meaning is unambiguous, I will follow actors' use of the term "American" to refer to those based in the United States.

discoveries," their cultivation of new approaches, and their exchanges of overseas students and researchers "more and more on terms of equality."[100] American mathematicians of Veblen's generation frequently credited their formation as an elite scholarly community to their apprenticeship in German institutions in the late nineteenth and early twentieth centuries, when Americans regularly completed their advanced mathematical training in German universities. For them, the Teutonic tutelage of American mathematics was an unalloyed good—a benevolent colonialism which nurtured American mathematics to maturity.

By 1950, the colonials had begun to become the colonists, at least in the particular form of scientific colonialism Veblen envisioned. Following the war, new United States government programs built on existing philanthropy-centered infrastructures of international educational exchange in order to drive a dramatic increase in the number of foreign students and researchers in the country's universities, including a significant share of visitors from the developing world.[101] With respect to Latin America, such scientific and educational exchanges became part of the legacy of more than a century of economic, military, and political projection in the American hemisphere.[102] While "colonial" historiography can be of mixed utility with regard to Latin American science, the term remains apt as an actors' category associated with frameworks

[100] Veblen, "Opening Address," 124.
[101] Liping Bu, *Making the World Like Us: Education, Cultural Expansion, and the American Century* (Westport: Praeger, 2003), esp. ch. 5; Paul A. Kramer, "Is the World Our Campus? International Students and U.S. Global Power in the Long Twentieth Century," *Diplomatic History* 33, no. 5 (2009): 775-806; Margaret O'Mara, "The Uses of the Foreign Student," *Social Science History* 36, no. 4 (2012): 583-615; Zuoyue Wang, "Transnational Science during the Cold War: The Case of Chinese/American Scientists," *Isis* 101, no. 2 (2010): 367-377.
[102] See George C. Herring, *From Colony to Superpower: U.S. Foreign Relations Since 1776* (Oxford: Oxford University Press, 2008), esp. ch. 4, 9, 13-14.

such as Veblen's that were oriented around migration and institution-building in higher education.[103]

Postwar American mathematicians' links to Latin American institutions emerged most directly from Franklin Roosevelt's interwar "good neighbor" policy of patronage and fraternity toward the region's countries. With Europe's scientific communities deeply divided, American scientists had a special opportunity to establish themselves as new pivots between Latin American scientists and the wider world. From the mid-1930s, Harvard mathematician George Birkhoff joined other leading American men of science in touring the region, establishing contacts with local academics, and reporting on the conditions for science they encountered.[104] Birkhoff sat as high in the American mathematical hierarchy as they come, training many future leaders of the American Mathematical Society and setting a precedent for their regular travel and

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[103] See Eden Medina, Ivan da Costa Marques, and Christina Holmes, "Introduction: Beyond Imported Magic," in Eden Medina, Ivan da Costa Marques, and Christina Holmes, eds., *Beyond Imported Magic: Essays on Science, Technology, and Society in Latin America* (Cambridge: MIT Press, 2014), 1-23, on 4-5. Recent surveys of the historiography of science in Latin America include Michael Lemon and Eden Medina, "Technology in an Expanded Field: A Review of History of Technology Scholarship in Latin America in Selected English-Language Journals," in Medina, Marques, and Holmes, eds., *Beyond Imported Magic*, 111-136; Stuart McCook, ed., "Focus: Global Currents in National Histories of Science: The 'Global Turn' and the History of Science in Latin America," *Isis* 104, no. 4 (2014): 773-817. On the varieties of imperialism, colonialism, and other economic, political, and cultural formations in the Americas, see Gilbert M. Joseph, Catherine C. LeGrand, and Ricardo D. Salvatore, eds., *Close Encounters of Empire: Writing the Cultural History of U.S.-Latin American Relations* (Durham: Duke University Press, 1998).

[104] Parshall, "Marshall Stone," 464-467. On Birkhoff's travels in Latin America, see also Eduardo L. Ortiz, "La Política Interamericana de Roosevelt: George D. Birkhoff y la Inclusión de América Latina en las Redes Matemáticas Internacionales," *Saber y Tiempo: Revista de Historia de la Ciencia* 4, no. 15 (2003): 53-111 and 4, no. 16 (2003): 21-70. Cf. Olival Freire Jr. and Indianara Silva, "Diplomacy and Science in the Context of World War II: Arthur Compton's 1941 Trip to Brazil," authors' translation of their Portuguese article in *Revista Brasileira de História* 34, no. 67 (2014): 181-201, accessed online at http://arxiv.org/abs/1407.6770. On the good neighbor policy in Brazil, see also Antonio Pedro Tota, *The Seduction of Brazil: The Americanization of Brazil during World War II,* trans. Lorena B. Ellis (Austin: University of Texas Press, 2009).

other ties to Latin America. He was slated to preside over the 1940 International Congress of Mathematicians before its cancelation, and Veblen only assumed the role for the 1950 Congress after Birkhoff's death in 1944.

Government bureaus such as the Office of Inter-American Affairs joined private entities like the Rockefeller and John Simon Guggenheim foundations to underwrite American scientists' travels, and the institutional lines between these bodies were often blurred. Personnel moved freely in both formal and informal capacities, as functionaries and advisors, between the philanthropic, governmental, and academic organs of mathematical colonialism. While an officer of both the Guggenheim Foundation and the OIAA, for instance, Henry Allen Moe helped orchestrate the Latin American travels of both George Birkhoff (in 1942) and Birkhoff's former student Marshall Stone (in 1943).[105] Plans and reports from American missions to Latin America circulated freely across these groups. Thus, Stone's influential post-voyage report for Moe (addressed to his OIAA Committee) made its way through the American Mathematical Society and Mathematical Association of America's War Policy Committee, which Stone chaired, to the officers of the Rockefeller Foundation, which underwrote the War Policy Committee's activities. Annotations in the Rockefeller Foundation's copy of Stone's report show that it was attentively read by Harry Milton Miller, an assistant and later associate director of Rockefeller's Natural Sciences Division under Warren Weaver. Miller, in turn, was the prime mover behind his foundation's own interventions in Latin American mathematics.[106] Stone, for his part, became a

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[105] Parshall, "Marshall Stone," 464-465.
[106] Stone to Moe (copy), 13 Apr 1944, RF Projects: U.S., box 127, folder 1561. On Miller's Latin American interventions, see esp. Maria Gabriela S. M. C. Marinho, *Norte-americanos no Brasil: uma História da Fundação Rockefeller na Universidade de São Paulo, 1934-1952* (Bragança Paulista: Universidade São Francisco, 2001), ch. 4.

regular visitor to Latin America—including during the critical summer of 1947 while he attempted to launch a new International Mathematical Union in earnest.

This narrow circle of American experts modeled their inter-American outreach on their understanding of its transatlantic predecessor, even as the institutions involved could be quite different. Young Latin American mathematicians would complete their studies in leading U.S. institutions, while leading U.S. scholars would travel to key regional centers in Latin America. These two populations cycling between North and South were to form a virtuous circle: northern elites helped to identify prospective northbound fellowship recipients and to prioritize southern institutional needs, while southern fellows built ties to northern institutions that helped them recruit future expert visitors and implement reforms at home. As they had done before and during the war, American mathematicians also used their work with United States government agencies and leading philanthropies to cultivate relationships with influential bureaucrats and policymakers who might fund or otherwise facilitate their research at home and their travels abroad.

United States foundations, and later the U.S. government, accounted for the largest concerted efforts to build Western mathematical institutions in Latin America from the 1930s onward, but they were not alone. On a more-or-less individual basis, short-term and long-term migrants from Spain, Italy, France, Germany, Poland, and the United States brought mathematical expertise, correspondence networks, institutional wherewithal, and collections of books and papers to a small set of cities and universities in Latin America in those early years. Interwar immigrants from Spain and Italy, including a number of refugees, were particularly numerous and influential in establishing mathematical programs and libraries in South American metropoles. Spaniard Julio Rey Pastor, typically cast as the father of Argentine pure mathematics, joined others with established European reputations and the small class of home-grown elites (often of European or upper-class parentage) to reshape the engineering, technical, and in some cases arts and philosophy arms of South American higher education to allow a place for mathematics fashioned after the latter's role in European scholarship and pedagogy.[107]

In the early postwar years, a number of young French mathematicians made extended trips to South America (especially Brazil), beginning with André Weil's residency at the University of São Paulo from 1945 to 1947—following his Rockefeller-facilitated wartime refuge in the United States—before he was hired by Stone at the University of Chicago. Weil was joined in São Paulo in 1945 by geometer Oscar Zariski, whose presence dissuaded the Rockefeller Foundation from sending at least one São Paulo mathematician slated for a

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107 See Luis Santaló, with M.O. González, Godofredo García, and Rafael Laguardia, *Latin American Contribution to Scientific Progress: Mathematics* (Montevideo: UNESCO Science Cooperation Office for Latin America, 1951). This report was among the first mathematics projects of UNESCO's Science Cooperation Office for Latin America, discussed below. On Santaló, see Carlos Borches, "Luis Santaló: Geometra y Maestro" and "La Etapa Argentina de Santaló: Un Matemático de Dos Mundos," *La Ménsula*, 15 (2012): 1-6. On Pastor, see Carlos Borches, "Rey Pastor y el Nacimiento de la Escuela Matemática Argentina," *La Ménsula* 17 (2013): 1-6. See also, Thomas F. Glick, "Science and Society in Twentieth-Century Latin America," in Leslie Bethell, ed., *The Cambridge History of Latin America,* volume 6: *1930 to the Present*, part 1: *Economy and Society* (Cambridge: Cambridge University Press, 1995), 461-536, on 509-513. There is a considerable recent literature from Brazil on Brazilian mathematics in this period, e.g. Circe Mary da Silva, "Politécnicos ou Matemáticos?" *História, Ciências, Saúde – Manguinhos* 13, no. 4 (2006): 891-908; Rogério Monteiro de Siqueira, "Enciclopedismo, Distinção Profissional e Modernidade nas Ciências Matemáticas Brasileiras (1808-1930)," *Revista Brasileira de História da Ciência* 7 (2014): 81-91; André Luís Mattedi Dias, *Engenheiros, Mulheres, Matemáticos: Interesses e Disputas na Profissionalização da Matemática na Bahia (1896-1968)* (PhD Dissertation, Universidade de São Paulo, 2002); Plínio Zornoff Táboas, *Luigi Fantappiè: Influências na Matemática Brasileira. Um Estudo de História como Contribuição para a Educação Matemática* (EdD Dissertation, Universidade Estadual Paulista, Rio Claro, 2005); Lucieli M. Trivizoli, *Intercâmbios Acadêmicos Matemáticos entre EUA e Brasil: Uma Globalização do Saber* (EdD Dissertation, Universidade Estadual Paulista, Rio Claro, 2011). On the broader development of Brazilian science, a significant survey is Simon Schwartzman, *A Space for Science: The Development of the Scientific Community in Brazil* (State College: Pennsylvania State University Press, 1991).

fellowship northward "at the very moment when the Mathematics Department here has been temporarily strengthened by the addittion [sic] of two ~~foreign~~ [sic] competent mathematicians."[108] The next year, Bourbaki collaborator Jean Dieudonné began his own two-year appointment in São Paulo. By the end of the 1940s, the combined effects of wartime and early postwar American efforts with this broader circulation of (especially French) mathematicians and mathematics fostered an image and expectation for modern mathematics in South America that, by the end of the 1940s, leaned considerably more toward abstract theories and internationalist orientations than might be found even in much of the developed world. So South American mathematicians were primed to participate in the 1950 Congress not just institutionally but also intellectually.

Though the Latin American developments described here were principally driven by actors rooted in the Western hemisphere, and though the particular mathematics that took hold could be found in many parts of the developed world, the international discipline of mathematics in Latin America retained a crucial European orientation. For U.S. mathematicians who had built a national discipline through direct exchanges with European counterparts, Europe's relevance generally went without saying. For Latin American actors, this view of Europe was one part practical and two parts promissory. U.S. and Latin American mathematicians alike presumed the relevance and importance of European actors and institutions for Latin American audiences and participants. Legitimate subjects of research and means of instruction and dissemination took European models, and this made it easier to exchange people and texts with European institutions and (perhaps more importantly) to expect to do so in the future. As in Europe,

[108] Zariski to Miller, 11 Jun 1945, RF Fellowships: Brazil, box 20, "Omar Catunda" folder.

mathematics in Latin America was expected to subsist highly unevenly, with a handful of elite centers with particular research specialties, a small cadre of elite men at the reins of institutional power judged according to their reputation in the disciplinary landscape of European mathematics, and a wide and variegated subsidiary infrastructure of mathematical training whose integration with and deference to the prerogatives of the mathematical elite could be assumed (whatever the case may have been in practice).

By the end of the Second World War there were two principal hubs of South American mathematics: a Spanish-speaking one centered around Buenos Aires and Montevideo and a Portuguese-speaking one within Brazil orbiting São Paulo and Rio de Janeiro—with a range of centers of research and teaching scattered through the rest of the region connected to each, mostly according to language. A third arm of Latin American mathematics ran through Mexico and the Caribbean. The institutions of this latter group were (as a whole) more closely integrated with their U.S. counterparts, and were regular waypoints for South American travelers in passage to and from the United States. Perhaps due to proximity, Mexican mathematicians were particularly well represented in U.S.-based regional efforts (such as those of the Rockefeller Foundation) but often left out of other projects conceived of as "Latin American." Conversely, Americans like Princeton's Solomon Lefschetz who maintained close ties to Mexican mathematicians were largely absent from inter-American undertakings directed farther south.

For mathematics, as for the sciences, a variety of efforts spearheaded by U.S. foundations, Latin American universities and governments, and a wide range of individual actors provided the institutional, political, and practical basis for postwar programs on a substantially grander scale. While direct U.S. government enterprises provided some of the greatest conduits for the circulation of people and resources, and the foundations that dominated interwar exchanges continued to play a significant role for mathematicians, many of the most visible postwar programs for mathematics flowed instead through UNESCO.[109] Headquartered in Paris, UNESCO in its early years reflected a broad coalition of hegemonic interests centered around France, Britain, and the United States.[110] UNESCO's formal entry into the organization and development of Latin American science began with a resolution at the organization's second General Conference, in Mexico City in December of 1947. The organization's Science Cooperation Office for Latin America, based from 1949 in Montevideo, Uruguay, coordinated scientific planning and funding across a wide range of disciplines and institutions.[111] The office sponsored conferences, workshops, courses, and exchange programs, and coordinated visits by European and American mathematicians to Latin America in order to provide advanced training, stimulate research, and suggest institutional reforms.

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[109] On UNESCO and internationalism in this period in relation to other governmental and non-governmental organizations, see esp. Akira Iriye, *Global Community: The Role of International Organizations in the Making of the Contemporary World* (Berkeley: University of California Press, 2002), ch. 2.

[110] William R. Pendergast, "UNESCO and French Cultural Relations 1945-1970," *International Organization* 30, no. 3 (1976): 453-483. Pendergast notes, in particular, French Foreign Ministry targets for cultural influence through technical assistance in Latin America from 1957 (on 469-470). Cf. David Ekbladh, *The Great American Mission: Modernization and the Construction of an American World Order* (Princeton: Princeton University Press, 2010), 85, on the Tennessee Valley Authority and American New Dealism as a model for Julian Huxley's early direction of UNESCO development programs.

[111] Patrick Petitjean, "Crossing Borders: Contributing to the development of science in Latin America," in P. Petitjean, V. Zharov, G. Glaser, J. Richardson, B. de Padirac, and G. Archibald, eds., *Sixty Years of Science at UNESCO 1945-2005* (Paris: UNESCO, 2006), 71-72. The office was established in consultation with the Rockefeller Foundation, International Labour Organization, and Smithsonian Institution, among a number of non-governmental organizations. For a broader analysis of UNESCO's vision of scientific internationalism, see Perrin Selcer, *Patterns of Science: Developing Knowledge for a World Community at Unesco* (PhD Dissertation, University of Pennsylvania, 2011, UMI No. 3463035). Cf. Marcos Chor Maio, "O Projecto UNESCO e a Agenda das Ciências Sociais no Brasil dos Anos 40 e 50," *Revista Brasileira de Ciências Sociais* 14, no. 41 (1999): 141-158, on the particular role of race, anthropology, and the social sciences, all construed in internationalist terms, in UNESCO's early interventions in Brazil.

UNESCO's presence in Latin America, especially after Stone's disappointing summer of 1947, gave the latter further impetus to corral the IMU's organizing process through the Western Hemisphere. Individual mathematicians from Latin America were occasional participants in International Congresses of Mathematicians prior to 1950, but the region's many new ties to American mathematical elites in the decade and a half before 1950 spurred an entirely new scale of participation. Stone made these Latin American mathematicians, whose advanced training in the United States mirrored Americans' own historical ties to Germany, into new constituents of the proposed International Mathematical Union. He hoped, with good reason, that their American connections would lead them to support his own vision for international mathematics. But however they voted in this or that assembly or council meeting, their very presence was further proof that the institutional balance of elite mathematics had moved West. Latin American mathematicians, by numbers alone, thus bolstered Stone's claims to hegemony over a discipline whose remaining leaders in Europe were not eager to cede their longstanding prerogatives.

Moreover, they offered Stone an answer to those European holdouts who insisted that a new union must await an assurance of universal adherence. If mathematicians in Europe persisted in squabbling over past slights or found themselves barred by geopolitics from free and open collaboration, those in the Western hemisphere were ready to move forward into a cooperative international future. Here was one answer to Morse's initial concerns over an "open" Congress: leave European rivalries aside, and let an open Congress be judged by how many nations of the world would be welcome at the Congress, if only they could come. As preparations for the Congress advanced in the years and months leading up to August, 1950, the organizers settled into an approach to foreign participations that heavily emphasized inclusion *in principle* as a shifting geopolitical terrain increasingly ruled out such inclusion *in practice* for many desired participants. Their approach worked well enough for assuring their right to host the meeting, and ultimately to lead the International Mathematical Union's initial organization process. Even so, they quickly found that the logistical problem of foreign attendance could not be ignored, and carried its own substantial implications for the prospects and practicalities of American leadership in postwar mathematics.

Chapter 4: Internationalism By Committee

Foreign Intelligence

It was one thing to trade lofty ideals and principles about bringing the world's mathematicians together in the abstract, but quite another thing to solicit and then secure their individual attendance from abroad. Once it became clear that the American Mathematical Society would be able to host an International Congress of Mathematicians in 1950, its committees pivoted in earnest to these latter challenges. AMS officers and committee members continued to wield and adapt their ideas of internationalism throughout their engagement with the concrete exigencies of intercontinental communication and travel. This chapter pairs their ideological adaptations to their practical means of inviting foreign mathematicians and navigating impediments to those mathematicians' attendance. This relatively bureaucratic side of the Congress's organization exposed many of the most significant ambiguities in the organizers' international ideals, and those ideals did not ultimately prove to be the kind of compelling resource the Americans hoped they would be. As the 1950 Congress's hosts worked to convert international ambitions into a broadly intercontinental gathering, they confronted a mathematical world that was not nearly so easily connected as they imagined.

The problem of reaching foreign mathematicians was central for the Congress's prewar planners, whose 1937 report offered the organizational template for the planned 1940 and eventual 1950 Congress.[1] Both before and after the war, organizers assumed that American and Canadian mathematicians would be able to attend without extensive efforts toward recruitment

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[1] The so-called "Eisenhart Report" as amended by the AMS Council is in Morse Papers, box 7, "ICM – Analysis" folder. The Committee's original version is in AMS records, box 26, folder 71. The report's provisions were re-adopted at the 16 Apr 1948 meeting of the Organizing Committee of the ICM; AMS Records, box 37, folder 56.

and financial support. Tabulated together in the Secretary's official report, 1,410 members and 540 associate members, or nearly 17 out of every 20 registered participants, had American or Canadian addresses.[2] That figure, moreover, excludes those domestic students, junior scholars, or others who attended without registering and thus were not included in the Congress's official rosters or tabulations.[3]

The Congress's secretariat recruited American and Canadian participation primarily through invitations sent to institutions of higher education and major academic societies, although they circulated announcements as well through American Mathematical Society publications and member communications. The Congress hosted official delegations from colleges or universities in nearly every U.S. state, and had registered members from each of the 48 states save South Dakota.[4] It received delegations as well from sixteen societies ranging from the Acoustical Society of America, to the American Psychological Association, to the National Academy of Sciences, though these delegates were often also part of college or university delegations.[5] Since the organizers could safely assume that the most significant American mathematicians were well-known domestically, they entrusted a single committee with nominating Americans from all fields for the prestigious "hour speaker" roles.[6]

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[2] *1950 ICM Proceedings*, 135.

[3] Their lack of official documentation makes a full accounting impossible, but evidence suggests that unregistered interlopers had few difficulties gaining entrance to the Congress. E.g. Richard Kadison (personal communication, 23 Feb 2013). It is unlikely that there were many foreign participants who could similarly choose to pass up the logistical assistance (especially regarding housing) that registering afforded.

[4] The University of Honolulu, in the U.S. territory of Hawaii, designated Michigan State College's Hugh Stelson (who taught briefly in Honolulu) as its delegate, and there was also a registered member from the U.S. territory of Puerto Rico.

[5] *1950 ICM Proceedings*, 20-35, 135.

[6] The organizing committee collected the recommendations of the various hour speaker committees and issued the final invitations. The hour speaker committees were not listed

The same breadth of familiarity could not be assumed for foreign mathematicians. As discussed in chapter 1, elite mathematicians at the turn of the twentieth century who wished to keep up with foreign colleagues' locations and subjects of research struggled to do so, even when not separated from them by an ocean. In 1938 or 1948 (albeit for somewhat different reasons), a leading American mathematician could not be sure of knowing who all his European counterparts were, much less where they were currently located and what subjects occupied their most recent research. Some regular channels of postal communication supported personal correspondence at a distance, but most news of the profession (particularly regarding work not yet published or widely available) spread through extended trips in one direction or the other. The war disrupted these and other opportunities for reconnaissance by limiting travel and the circulation of publications. Mathematicians relied heavily on review journals to piece together broader pictures of the latest research in their fields and profession, but even in the comparatively expeditious *Zentralblatt* (or later *Mathematical Reviews*) a delay of two years between publication and review was not uncommon, and it could take longer for interested mathematicians to obtain copies of the articles to which the reviews referred. That was hardly a recipe for staying abreast of research in a discipline where new developments could sometimes come at a rapid pace. If a mathematician beyond Europe was known to the Americans, it was often the result of an extended stay at an American institution.

The Congress's organizers thus divided the problem selecting foreign mathematicians for invitations as "hour speakers" among separate committees for broad fields including analysis, algebra, topology, and applied mathematics. With addresses uncertain and many postwar postal

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separately in the Proceedings, and so my account of their membership and operation is derived from the Eisenhart Report (noted above) and the records Marston Morse retained as chair of one such committee (discussed below).

systems slow or dubiously reliable, the Congress secretariat relied on the U.S. State Department's diplomatic mail system and other extended indirect means of inviting mathematicians from abroad. For the comparatively time-sensitive work of selecting distinguished foreign speakers, such difficulties put directly consulting European colleagues out of the question. Indeed, there is no evidence such a possibility was ever entertained explicitly. Instead, each hour speaker committee was to piece together the morsels of professional intelligence that, amalgamated over its field's American leaders, might be hoped to yield a representative evaluation of the leaders of international mathematics. The organizers hoped that, within a broad channel of research at least, well-connected Americans would have enough incidental information at least to determine the very best foreign scholars in their area of study, but the plans for their search process presumed as well that assembling such a list would not be easy.

The committees worked mostly through letters and surveys over the half year between October, 1948, and April 29, 1949, when a list of 27 invited hour speakers was approved by the organizing committee, including four Soviet mathematicians thought unlikely to attend.[7] Even after having mustered the expertise of their leading domestic colleagues, the organizers remained concerned about the possibility of conspicuous omissions. When considering the final list of whom to invite, they noted the absence of several famous names and a "geographic distribution" that differed markedly from that of previous Congresses.[8] As a concession to these concerns, the organizing committee expanded the number of honorary posts to be given to famous names and representatives of famous places. These ultimately included the Congress's three Honorary

[7] Organizing Committee minutes, 29 Apr 1949, AMS Records, box 37, folder 56.
[8] Ibid.

Presidents: Guido Castelnuovo (of Italy), Jacques Hadamard (of France), and Charles de la Vallée Poussin (of Belgium).[9]

Those corrections were relatively straightforward. The half year of committee work that resulted in the initial list of speakers for invitation was much less so. The first part of this chapter takes a close look at how one foreign hour speaker committee, for the field of analysis, wound its way toward a consensus list of speakers. The committee faced, above all, the challenge of integrating and reconciling their many partial sources of information and assessment for mathematical analysts in and beyond Europe. The war disrupted their most reliable sources, and disrupted as well the careers of those whose work the committee set out to evaluate. Selecting the most meritorious speakers thus required interpolating about both mathematicians' pattern of research achievements and the piecemeal documentary record of those achievements. The committee's ultimate resolution process shows the pervasive effects of mathematicians' operational reliance on supporting infrastructures from universities and research institutes. Here, I show how the committee's chair Marston Morse worked in collaboration with Institute for Advanced Study secretary Gwen Blake to manufacture consensus in a muddled field.

Having selected the foreign hour speakers, the organizers' work was not done. A speaking invitation was only effective if it could be converted into a flesh-and-bone speaker. While Morse's Emergency Committee initially worried most about German and Japanese mathematicians, the Congress's organizers quickly shifted attention to those who might be barred from traveling under stringent new Cold War travel restrictions. After some early disappointments, the organizers did not ultimately make extensive special efforts to secure Soviet attendance. For non-Soviet mathematicians with Communist pasts (or presents), however,

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[9] *1950 ICM Proceedings*, front matter.

there was just enough room to negotiate on individual cases that the organizers felt it worthwhile to draw on their government connections to attempt to secure travel permissions. Sometimes these attempts succeeded; other times they quietly fell flat; and in at least one situation the attempt backfired spectacularly in a way that some organizers feared would threaten the entire Congress. This chapter lays out the diplomatic machinations that underwrote mathematicians' attendance in the most controversial cases by first examining the relatively unknown story of one mathematician who was unable to attend due in part to his Communist politics before turning (using a variety of sources historians have not previously considered) to the Congress's most famous diplomatic episode: the visa negotiations surrounding Laurent Schwartz.

Ranks and Files

On October 4, 1948, Marston Morse received his official appointment to the chairmanship of the Conference in Analysis, the principal organizational unit within the Congress for assembling speakers in his field of mathematical analysis.[10] This role also made him responsible for convening a formally separate (but in practice overlapping) committee to select the foreign hour speakers in that field. Like other organizational operations of the Congress, Morse's committees drew from a small pool of established figures to whom he was personally or professionally tied. It took him a month to settle on two overlapping lists of Harvard and Princeton notables and American Mathematical Society intimates for the Conference in Analysis and foreign hour speaker committees, and he included members of both committees in the discussion on hour

[10] Kline to Morse, 4 Oct 1948, Morse Papers, box 7, "Analysis Conference" folder.

speakers.[11] One committee member, 1936 Fields Medalist Lars Ahlfors, thought his foreign hour speaker appointment "must be based on a misunderstanding," as he was already on the American hour speaker, Conference in Analysis, and Fields medal committees.[12] Though Morse did not have the Congress's Organizing Committee make an official appointment, he included Ahlfors fully in the foreign hour speaker committee's correspondence as well, and Ahlfors participated as though a full-fledged committee member.

Within weeks, the group assembled a panel of 36 American analysts of various ages and sufficient reputation. Indeed, Berkeley's Griffith Evans was "pleased to see so many young ones" among the list of American voters.[13] The group also helped Morse compile a list of 30 foreign analysts to be circulated to the American panelists for votes and comments.[14] A round of balloting yielded, by the year's end, a ranked collection of candidates for further scrutiny by the core committee and selected others.[15]

Even at this stage some difficulties were apparent. Morse took it upon himself to exclude from the initial ranking some of those he considered unlikely to attend for reasons of politics or personal circumstances, but left Soviet mathematicians (considered equally unlikely) on the roster. There was some question of what a particular speaker's proper field was for the purposes of invitation to the Congress. Many potential candidates worked in multiple areas of mathematics, and the committee had to debate the relative priority of these different areas,

[11] Kline to Ahlfors, Bochner, Evans, and Stone, 9 Nov 1948, Morse Papers, box 7, "Analysis Conference" folder. Kline to Bochner, Evans, and Hildebrandt, 11 Nov 1948, Morse Papers, box 8, "ICM – Select Speakers" folder.
[12] Ahlfors to Morse, 1 Dec 1948, Morse Papers, box 8, "ICM – Select Speakers" folder.
[13] Evans to Morse, 20 Nov 1948, Morse Papers, box 8, "ICM – Select Speakers" folder.
[14] Morse to panelists, 7 Dec 1948, Morse Papers, box 8, "ICM – Select Speakers" folder.
[15] Morse to Ahlfors, Bochner, Evans, Hildebrandt, Hille, von Neumann, Rademacher, Stone, 21 Dec 1948, Morse Papers, box 8, "ICM – Select Speakers" folder.

balancing the likelihood of an invitation from another section against the risk of a high profile mathematician being snubbed for lack of a clear single predominant field of research.

On behalf of the Organizing Committee, Kline instructed Morse's committee members that "Care should be exercised so that no candidate for an hour address is overlooked by a particular committee because the committee believes he will be included in another field in which he has made outstanding contributions" and that their list thus "should be based on a broad and not a narrow interpretation of the field in question."[16] However in practice it did not always boil down to a question of breadth. The long interval since the last Congress required the committee to weigh how to treat mathematicians whose fields of study had shifted in the interim, or whose latest work remained unknown. Because they could not poll possible speakers directly, committee members relied on inferences from recent personal encounters and from such publications as were available to them.

Morse, for instance, drew on his contact with French mathematician Henri Cartan while the latter was a visitor at the Institute for Advanced Study to conclude that Cartan seemed solely a topologist now, although he might in the past have been considered an analyst under the committee's purview. Cartan was, indeed, ultimately invited at the behest of a committee other than Morse's, though his address "On analytic functions of complex variables" may well have been within Morse's jurisdiction after all.[17] For others, the committee weighed whether their

[16] Kline to Bochner, Evans, and Hildebrandt, 11 Nov 1948, Morse papers box 8 "ICM – Select Speakers" folder. When invitations finally went out in the summer of 1949, Henri Cartan noted several prominent mathematicians who had not received one and recorded Marcel Riesz was "very vexed" by his omission. Cartan to Weil, 21 Jul 1949, Michèle Audin, *Correspondance entre Henri Cartan et André Weil (1928-1991)* (Paris: Société Mathématique de France, 2011), 265-266.

[17] *1950 ICM Proceedings*, p. 88, 136; Henri Cartan, "Problèmes Globaux dans la Théorie des Fonctions Analytiques de Plusieurs Variables Complexes," *1950 ICM Proceedings*, 152-164.

work was too similar to certain co-nominees, and whether such similarities should be considered. This latter question implied a larger one about the committee's goals which was also up for discussion: whether they were to single out individual meritorious speakers, assemble a slate of speakers representative of the field as a whole, or somehow balance those objectives.[18]

Other difficulties were altogether more banal, and more embarrassing. The committee worried not just about disciplinary balance but about the balance of nations among their distinguished speakers. Their assembled nominations, however, included no Germans, and other national groups were conspicuously short of representatives. Indeed, one committee member saw the list of nominees and commented to Morse that "Presumably German mathematicians were intentionally omitted from the list." He continued, "I wonder whether this is wise, seeing that we are insisting that we are having an International Congress," before admitting he was "not sure who would be good to invite."[19] As far as Morse's records suggest, such unfamiliarity with recent German mathematics (rather than any deliberate omission) accounted for the void of nominees. In other cases, Morse had to sort out nominators' intentions when they listed mathematicians who shared a surname with another plausible candidate. He worked to supply

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Cartan identified his lecture as on analysis "in the large," an approach (and English coinage) Morse himself pioneered between the wars, which drew on analytic, algebraic, and topological methods. See Marston Morse, "The Foundations of a Theory in the Calculus of Variations in the Large," *Transactions of the American Mathematical Society* 30 (1928): 213-274; Marston Morse, *The Calculus of Variations in the Large* (New York: American Mathematical Society, 1934); Marston Morse, *Lectures by Marston Morse on Analysis in the Large, 1936-1937*, notes by William L. Duren Jr. (Ann Arbor: Edward Bros., 1937); Renaud Chorlay, "'Local-Global': the first twenty years," *Archive for History of Exact Sciences* 65 (2011): 1-66.

[18] Morse to Ahlfors, Bochner, Evans, Hildebrandt, Hille, von Neumann, Rademacher, Stone, 21 Dec 1948, Morse Papers, box 8, "ICM – Select Speakers" folder. On representing the field as a whole, cf. Hille to Morse, 11 Jan 1949, Morse Papers, box 7, "Analysis Conference" folder.

[19] Hildebrandt to Morse and Evans, 28 Nov 1948, Morse Papers, box 8, "ICM – Select Speakers" folder.

other names that nominators omitted, forgot, or misremembered. One Italian nominee was two years deceased.[20]

When discussions resumed in earnest in the new year, Morse's men set to weighing individuals' merits and debating the appropriate desiderata for their final recommendations. For their individual assessments, they relied on each other's personal familiarity with the candidates or their work, alongside cursory perusals of published literature and summaries and citations in review journals. The committee differed on whether and how to balance nationality and age in the cohort of speakers, as well as how much credit to give older or more recent work, especially as work from the last decade was likely to have been interrupted or not well known.[21] Only a few of those who were considered hailed from outside the traditional European centers of mathematics, and those who did—like Japanese mathematician Shizuo Kakutani—tended to have previously worked in the United States for extended periods of time and to have maintained personal contact with committee members.[22]

To rise on the list, it was important to have one or more advocates, often in the form of personal acquaintances or researchers in closely allied areas of analysis. Ahlfors, of Finnish extraction, gave an unusually high ranking to the Swede Arne Beurling. Laurent Schwartz, meanwhile, stood primarily on the forceful words of Stone (who had already taken an interest in his theory of distributions) and Morse's colleague at the Institute for Advanced Study John von Neumann (whose interwar work on the theory of function spaces was broadly understood as

[20] Nov 1948 correspondence, Morse Papers, box 8, "ICM – Select Speakers" folder.
[21] E.g. comment sheets following Morse circular of 21 Jan 1949, Morse Papers, box 8, "ICM – Select Speakers" folder.
[22] Kakutani was ultimately invited, but had also immigrated to the United States before the Committee's deliberations had finished. In the Congress *Proceedings* he is listed as an official delegate of the Mathematical Society of Japan, with Yale University as his address.

important to Schwartz's own innovations).[23] Such advocates were particularly important where, as for both Beurling and Schwartz, not everyone on the committee had previously heard of the nominee in question.[24]

Amidst such a range of considerations for committee members with substantial differences of opinion as well as access to substantially varying sets of partial information about the different candidates, Morse's task of reaching consensus on a ranked list of speakers faced many obstacles. Conducting the entire selection process by postal correspondence, moreover, Morse lacked recourse to the kinds of in-person negotiations that might otherwise have bridged divergent assessments and priorities. Instead, he relied on three important technologies of committee intelligence—the committee letter, the table of rankings, and the questionnaire—to set the terms of his committee's work and to reconcile divergent assessments. These technologies, in turn, depended on an individual present throughout the organization of the Congress but hitherto largely invisible: Gwen Blake, secretary to the Institute for Advanced Study's mathematicians since 1933.[25]

Eight years Morse's senior, in 1932 Blake had turned to her acquaintance Abraham Flexner, the leading figure behind the Institute's founding, for help in landing what the latter called a "congenial and remunerative" post in the New York City area, where she owned a house

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[23] For emphasizing Schwartz's theoretical connections to von Neumann, I am grateful to Peter Lax (personal communication, 27 Mar 2014).

[24] Hildebrandt to Morse, 25 Jan 1949, Morse Papers, box 8, "ICM – Select Speakers" folder.

[25] Flexner to Blake, 7 Jun 1933, Records of the Comptroller, Box 1, folder 17, from the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA (hereafter IAS archives). Blake retired around the time of the 1950 Congress, which added urgency to Underwood's hiring as Blake's successor; see "Underwood, Caroline May 27, 2010," interview by Linda Arntzenius, Oral History Project files and recordings, Box 10, IAS Archives, pp. 3-4. Underwood was introduced to Morse by J. R. Kline, for whom Underwood had been a secretary at the University of Pennsylvania during the war before pursuing a Master's degree in library science under the GI Bill.

with her sisters.[26] The position she ultimately took with the newfound mathematics faculty initially paid $2,100 annually and required, in addition to typing and other secretarial skills, a "reading knowledge of French and German" (both of which she honed on the job) and a tolerance for Princeton's "hot and humid" summers.[27] German was presented as especially important in view of the large share of Institute mathematicians who were "German or were educated in Germany" and so maintained regular correspondence in the German language where possible.[28] As the Institute rapidly became a leading hub for international mathematics, Miss Blake (as Institute staff and faculty called her) managed professional correspondence, typed mathematical manuscripts, assisted foreign mathematicians with everything from visas to housing, and insulated them from the outside world as a gatekeeper for inquiries to the Institute.[29] As Morse took on assignments before, during, and after the war on behalf of the American Mathematical Society and the Congress's organizing committee, Blake's secretarial

[26] Flexner to Blake, July 20, 1932, IAS Comptroller, box 1, folder 17. See also Blake to Flexner, 11 May 1932; Flexner to Blake, 7 Dec 1932.
[27] Bailey to Blake, 9 Jun 1933, IAS Comptroller, box 1, folder 17.
[28] Ibid. Richard Courant saved the following instructive extracts from the correspondence of van der Waerden and Neugebauer in Courant Papers, box 15, folder 9. Van der Waerden to Neugebauer, 28 Feb 1941: "Mir ist es egal, ob Du mir deutsch, englisch oder italienisch schreibst. Wenn Du aber auf den Verkehr mit deutschen Mathematikern Wert legst (und das müsstest Du doch eigentlich, als Leiter von zwei internationalen Zeitschriften), so must Du wohl die kleine extra Mühe nehmen, Dich Deiner Muttersprache zu bedienen. Auf Gefühle, die Du nicht teilst, kannst Du doch trotzdem Rücksicht nehmen, nicht?" ["It is all the same to me if you write in German, English, or Italian. But if you place any value on exchange with German mathematicians (and you really must, as the leader of two international journals), you must surely take the little extra effort to make use of your mother tongue. You can still take sentiments into consideration even if you do not share them, no?"]. Neugebauer to van der Waerden, 26 Mar 1941: "I must remark that the language I use in my letters does not depend on my mother but on my secretary." I thank Ryan Dahn for his assistance translating van der Waerden's excerpt.
[29] On the gatekeeper role, see Caroline Underwood, 27 May 2010, IAS Oral History, box 14, on 4-5. On visas for foreign mathematicians, see e.g. Bailey to Blake, October 21, 1935, Records of the Comptroller, Box 1, folder 17, IAS Archives.

docket grew in stride. The bulk of Morse's correspondence cited in this and the previous chapter passed through her typewriter.

Business and labor historians have documented the emergence in the first part of the twentieth century of corporate secretarial corps in conjunction with a broader reorganization of American and European labor under the banner of scientific management.[30] Academic institutions adopted many of the white collar labor systems of their corporate cousins, both through direct contact and exchange of personnel and through relationships with philanthropies, corporations, and government entities that presumed or required particular administrative regimes.[31] Blake represented a generation of well-educated women skilled in a range of clerical and administrative roles without whom institutions like Morse's would not have functioned with the scales, budgets, and paper-based connections to other institutions that they sported in this period. Professional organizations like the American Mathematical Society relied extensively on this hidden and versatile workforce of academic staff to enable the volumes of committee work through which they ran.[32] Historians of science have examined scientists' paper-based technologies of reasoning and creativity to great effect, but Feynman diagrams and exam scripts

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[30] Sharon Strom, *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930* (Urbana: University of Illinois Press, 1992); Angel Kwolek-Folland, *Engendering Business: Men and Women in the Corporate Office, 1870-1930* (Baltimore: Johns Hopkins University Press, 1994). There is a rich sociological literature on the sociality and gendered division of labor in such corporate settings. Two landmark studies are Rosabeth Moss Kanter, *Men and Women of the Corporation* (New York: Basic Books, 1977), esp. 69-103; Rosemary Crompton and Gareth Jones, *White-Collar Proletariat: Deskilling and Gender in Clerical Work* (Philadelphia: Temple University Press, 1984), ch. 1, 2, 4, 6.

[31] See Rebecca S. Lowen, *Creating the Cold War University: The Transformation of Stanford* (Berkeley: University of California Press, 1997), ch. 1-4.

[32] They also relied on highly educated women for technical typesetting (for their publications), as well as a range of managerial and organizational roles. As noted in chapter 3, Mina Rees was an especially central and effective organizer of mathematical research through military and related contracts.

are not much use in themselves for planning a conference or running an institute.[33] In these latter respects, scientists depended on a different array of paper-based organizational technologies and practices—typing, correcting, copying, addressing, mailing, compiling, sorting, indexing, filing, scheduling, booking, tabulating, and more—which in turn depended on skilled support personnel like Blake.[34]

The committee letters that Blake prepared and circulated on Morse's behalf shared many features with ordinary interpersonal professional correspondence. They expressed Morse's views and agendas for the selection process, enlisted cooperation, and shared pertinent information. As documents that were legible as shared among a listed cohort of recipients, they established the primacy of Morse's representations of the committee's work as the common substrate for the group's negotiations. This made it possible for Morse to mediate communication amongst all committee members from an influential hub position where he could selectively adapt and curate others' perspectives in order to frame debates and cultivate consensus. Committee members wrote to Morse with the understanding that all committee members had common access to the content of his committee-wide communication, and so framed their interventions against that presumed backdrop.

With tables of rankings, Blake assembled ordered lists of candidates from committee members into formats that helped them to reconcile divergent qualitative assessments into what

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[33] E.g., David Kaiser, *Drawing theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005); Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003); Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century* (Stanford: Stanford University Press, 2003).

[34] See Juliet Webster, *Shaping Women's Work: Gender, Employment and Information Technology* (London: Longman, 1996); Thomas Haigh, "Remembering the Office of the Future: The Origins of Word Processing and Office Automation," *IEEE Annals of the History of Computing* 28, no. 4 (2006): 6-31.

appeared to be a numerical consensus. On Morse's instructions, Blake listed nominees in the first column of her ranking tables followed by the number of rankings the nominee received at or above a particular place enumerated in subsequent columns. In one table circulated to the committee, for instance, Laurent Schwartz received no first or second place rankings, but was the third preference for two committee members, the fourth preference for another, the seventh preference for a fourth committee member, and the tenth preference for a fifth. His row of the table was thus blank in the first two columns after his name, then had a "2" to mark the two third-place rankings, followed by a "3" to mark the three committee members who ranked him at least fourth, a "4" in the seventh column after his tame to indicate that four members had ranked him at least seventh, and so on. This allowed committee members to read quickly how many of their colleagues thought a particular nominee of at least a particular rank with respect to the other nominees, and to compare those evaluations between candidates.

These tables helped Morse, in particular, to cobble together a tentative roster of possible speakers, parceled into groups whose composite rankings appeared similar on the table. Blake then compiled Morse's roster into a standardized questionnaire along with a series of questions about particular mathematicians and about general criteria, like the relative importance of age. Committee members annotated their questionnaires in pen, expressing further preferences within Morse's narrow rubric of desiderata, and returned the documents to Blake by post. With their responses, Morse could then assemble a more precisely ranked slate of speakers with the confidence that most committee members assented to its contents, within the parameters he had set. The resulting list of speakers presented to the organizing committee could fairly be said to represent the highly curated collective wisdom of a highly curated group of elite American analysts—a state of the field, viewed obliquely from the top.[35]

Diplomatic Preparations

Even mathematicians with every intention of traveling to the Congress could face considerable challenges to their eventual attendance. For Soviet mathematicians, the organizers' mounting skepticism with respect to these obstacles made for an easy rule of action: invite the most notable and hope for their participation, but do not count on it. Morse's committee, like the Congress's broader Organizing Committee, treated Soviet invitations as mostly symbolic and used them to acknowledge valued colleagues with no expectation of their eventual participation. The principle extended even to committee membership. While most committees drew only from American Mathematical Society members, the committee to select the Fields medalists drew on notables with international reputations from a range of countries. Their number included Soviet analyst Andrey Kolmogoroff, also one of Morse's symbolic foreign hour speaker designees, and he was the one member of the Fields committee not to participate in discussions.[36]

For non-Soviet would-be participants, matters were more complicated. The organizers expected entry visas to be a concern for all foreign delegates, but particularly for those in Germany and Japan subject to complicated postwar diplomatic regimes. They also expected potential trouble for those whose past political activity might rouse suspicions under a rising tide

[35] The penultimate, broadly ranked survey list, the last retained in Morse's papers, included Gelfand, Kolmogoroff, H. Hopf*, Littlewood, F. Riesz, Beurling*, Hodge*, Schwartz*, S. Bernstein, and Kakutani* (already at Yale by 1950). I have marked those who ultimately gave invited hour addresses with an asterisk. Littlewood was, with Schwartz, one of those initially excluded from discussion because Morse considered him unlikely to be able to attend.
[36] Harald Bohr, "Address of Professor Harald Bohr," *1950 ICM Proceedings*, 127-134, on 127.

of official American anti-communism. Those with ties to Communist politics, in particular, fell in 1949-50 in an awkward limbo between the surge in official American anticommunism and the advent of routine protocols for scientific and cultural exchanges that eschewed political tests for short-term visitors.[37] In fact, the Congress's organizers actively advocated for programs and procedures that would routinize scientific exchanges and visitors, and followed their progress with great interest.[38]

Confident that Dewey would defeat Truman in the 1948 U.S. Presidential election, J. R. Kline and Oswald Veblen waited to begin negotiations with the State Department until the dust had settled under the new presidential administration.[39] In the meantime, they busied themselves with the groundwork for those negotiations. Veblen planned to meet with New Jersey Republican Senator H. Alexander Smith, a graduate and stalwart of Princeton University who in the previous year had drafted a bill to create a civilian National Science Foundation that passed through Congress but fell to Truman's veto pen.[40] Kline and Veblen also aimed to confer with Detlev Bronk of the National Academy of Sciences, who had lent his support to their abortive 1947 effort to establish an International Mathematical Union through the International Council of Scientific Unions, and who they supposed "can certainly give us good advice and help in this

[37] See] Ellen W. Schrecker, *No Ivory Tower: McCarthyism and the Universities* (Oxford: Oxford University Press, 1986), 84-125; Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: University of North Carolina Press, 1999). A first-hand account from a junior mathematician of the period on the political left is Chandler Davis, "The Purge," in Duren, ed., *A Century of mathematics in America, Part I* (Providence: American Mathematical Society, 1988), 413-428.
[38] E.g. correspondence in AMS Records, box 35, folder 83.
[39] Kline to Morse, 25 Oct 1948, Morse Papers, box 7, "ICM – Analysis Conference" folder.
[40] See Daniel J. Kevles, *The Physicists: The History of a Scientific Community in America* (Cambridge: Harvard University Press, 1995 [1977]), 357. Truman ultimately joined with a Democratic Congress to establish an NSF in 1950.

matter."[41] At the same time, they worked to clarify the American Mathematical Society's and the Organizing Committee's positions on relevant points of policy so that they could present a unified front in their diplomatic conversations.

Truman retained the presidency in a surprise victory on November 2, 1948, and promoted Dean Acheson to Secretary of State at the start of his new term the following January. Acheson had already established himself by this point in early Cold War science policy through his work surrounding atomic energy and weapons, and would soon lead the European Recovery Program (known unofficially as the Marshall Plan) of American investment in European reconstruction.[42] Kline and Veblen, as Secretary and President of the Congress, wrote that February to Acheson's office in order to press their case for a "truly international" Congress in the non-exclusive sense of admitting any "professionally qualified" mathematician "irrespective of nationality" (including those "whose governments may be regarded as inimical") and to solicit State Department advice to "help us to take the necessary steps to insure the international character of the forthcoming Congress."[43] This formulation of "truly international" minimized the inclusive (and by then entirely unrealistic) goal of universal participation in favor of a principle of non-exclusion with which the organizers might press for limited official support for potentially controversial visa cases. Acheson referred their letter to Warren Kelchner of the Division of International Conferences of the Department of State, the same "most sympathetic" official with whom Morse and Kline had conferred in 1946 on the Emergency Committee's behalf as part of

[41] Kline to Morse, 25 Oct 1948, Morse Papers, box 7, "ICM – Analysis Conference" folder.
[42] See John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge: MIT Press, 2006); James Chace, *Acheson: the Secretary of State who created the American world* (New York: Simon and Schuster, 1998).
[43] Veblen and Kline to Acheson, 19 Feb 1949, Morse Papers, box 7, "Organizing Committee" folder.

that committee's efforts to canvas relevant supporting institutions and infrastructures for the Congress before its official resumption.

Kline and Veblen were soon joined by Samuel Eilenberg, who assumed the chair of the Cooperation Committee whose tasks included facilitating visas for foreign scholars. The three aimed to secure, as quickly and thoroughly as possible, the personal connections and knowledge of formal policies and precedents that would enable them to lobby effectively on foreign participants' behalves. They recorded, for instance, that the U.S. government could exercise discretionary authority to admit scholars with passports from "Communist-dominated governments" for temporary visits for "bona fide educational, cultural, and scientific purposes."[44] This part of their fact-finding remained hypothetical, however, for reasons that surely disappointed them but could not have surprised them. As far as the American organizers recorded, no mathematician from the Soviet Bloc faced American visa problems since none, ultimately, could obtain the necessary travel permissions from his own government.

Rather, the bulk of the organizers' discussions with the State Department and about immigration, especially in the second half of 1949 after invitations had been sent to plenary lecturers, concerned mathematicians from non-Communist countries whose past political activity presented concerns for immigration authorities. Congress representatives met formally with State Department officials in May, leading to a memorandum that the State Department then revised "in such a manner as to make it more liberal than expected."[45] Kline reported to the Organizing

[44] Kline to Veblen, 8 Mar 1949, with attached policy excerpt, AMS Records, box 35, folder 83.
[45] ICM Organizing Committee minutes 29 Oct 1949, AMS Records, box 37, folder 56.

Committee in October that a general policy was in the works with respect to international congresses.[46]

By that stage, elite American mathematicians and their sponsors in the Rockefeller Foundation and elsewhere had substantial experience with these kinds of negotiations. During the war, their efforts to place refugee mathematicians from Europe required regular communication with diplomatic authorities that taught American advocates the terms and formalities of the politics of migration. Affirmations from prominent mathematicians helped candidates establish credibility, and they knew to assure officials of their personal independence from whatever political concerns tarred their places of origin. As the war drew to a close, the Americans turned their expertise to the challenges of administering fellowship programs that brought young mathematicians to the United States for advanced training. Many young mathematicians who hoped to attend the Congress, especially those from outside of Europe, relied on those fellowship programs both for their initial induction into the international community of professional mathematicians and for their concrete means to travel to the United States in order to participate. For these mathematicians, visa discussions from their periods of fellowships carried naturally to their arrangements for the 1950 Congress. When all had gone smoothly in the first instance, their specific arrangements for the Congress faced few hurdles. Where there were complications initially, those complications could have a magnified effect for any future efforts to visit the United States.

[46] Ibid.

Political Tests

In his Secretary's Report at the start of the 1950 Congress's *Proceedings,* Kline protested at length the organizers' efforts to facilitate the entry of foreign nationals and "to maintain the non-political nature of the Congress."[47] Historians have tended to take Kline's claim at face value, casting it as evidence of the organizers' unwavering commitment to international ideals.[48] Kline reported that the "most sympathetic and helpful" State Department officials had made every effort "to secure a visa for every mathematician who notified them about any visa difficulties before cancelling his passage." With their help, Kline claimed that "only one mathematician from any independent nation was prevented from attending the Congress because he failed a political test and this man did not notify the officers of the Congress about his difficulties" while "Only two mathematicians from occupied countries failed to secure visas." Across the Iron Curtain, mathematicians' "own governments" prevented their attendance, and their absence "was not due to any action of the United States Government."[49]

Look behind the scenes, however, and the Americans' resolve shows evidence of having wavered in the face of what Kline acknowledged were "many serious difficulties" in securing foreign mathematicians' attendance.[50] Beneath Kline's official accounting of who could or could not attend for whatever reason, there is no way to know with certainty how many mathematicians would have made the trip under different political circumstances but were dissuaded at one point or another. Kline did not record the name of the neutral mathematician who failed a political test, but if he had Uruguayan Communist mathematician José Luis Massera in mind then the facts on

[47] J. R. Kline, "Secretary's Report," *1950 ICM Proceedings,* 121-145, on 122.
[48] E.g. Olli Lehto, *Mathematics Without Borders: A History of the International Mathematical Union* (New York: Springer, 1998), 89.
[49] Kline, "Secretary's Report," 122.
[50] Ibid.

the ground give a different picture from Kline's implication that he simply neglected to inform the Congress's officers of his troubles. If it was not Massera, then Massera's difficulties stand in evidence of just how substantial a political barrier Kline was prepared *not* to blame on U.S. authorities. Either way, Massera's political tests help foreground the tangible consequences of American anti-communism in a critical period for intercontinental mathematics.

In many respects, Massera offered a shining success story for the new North-South partnerships that tied together mathematicians in the Americas in the 1940s.[51] After drawing the attention of well-connected Americans in Uruguay, he won a fellowship to continue his studies at elite universities in the United States. While there, Massera honed his mathematical skills, gained familiarity with American institutions and publications, forged contacts with other junior mathematicians, and won the regard of several established leaders of his field. On his return to Uruguay, he maintained his contact with U.S. mathematicians, published in American journals, and enthusiastically participated (where possible) in American-led international projects. But his experience also laid bare the political fault-lines of such North-South exchanges, and furnishes the necessary background for understanding why, despite his firm intentions, he did not attend the 1950 International Congress of Mathematicians.

Born in Italy to Uruguayan parents, Massera was trained as an engineer in Uruguay by a small corps of European mathematicians who visited periodically from Buenos Aires, including Italian Jewish emigrant Beppo Levi, who supervised some of Massera's first mathematical research. Marshall Stone met Massera on his 1943 visit to South America and later reported that Massera was young ("under forty") and showed a promising combination of interests, training,

[51] See Michael J. Barany, "Fellow Travelers and Traveling Fellows: The intercontinental shaping of modern mathematics in mid-twentieth century Latin America," *Historical Studies in the Natural Sciences,* forthcoming, from which parts of this section have been adapted.

and judgment that would help him “make a real contribution to mathematical and scientific development” in Uruguay.[52] As noted in chapter 3, Stone’s report made its way to the Rockefeller Foundation’s Harry Miller by way of Stone’s War Policy Committee. The report reached Miller at around the same time that he received a letter from Walter S. Hill, a contact in the Faculty of Engineering in Montevideo, recommending “a young man of exceptional talent and background” who might benefit from further training in the United States.[53] After about six months of vetting and discussion about the best possible placement, Miller approved a year’s fellowship for Massera to study at Stanford and Massera began making his travel arrangements.

Massera’s Italian birth presented one easily-solved complication: until the U.S. concluded a peace treaty with Italy, Massera would be unable to pass through the Panama Canal and would instead travel to Stanford by way of the U.S. East Coast.[54] A different and altogether more challenging difficulty, one Miller had not quite appreciated on the basis of his initial intelligencing, owed to what Massera’s colleague Rafael Laguardia described to Miller as Massera’s “certain inclination for politics.”[55] Massera himself reported that he was Secretary General “of an important institution which is aiding the allies”—the Acción Antinazi de Ayuda a

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[52] Stone to Moe, 13 Apr 1944, RF Projects: U.S., box 127, folder 1561, 6.
[53] Excerpt of W.S. Hill to H.M. Miller, 15 May 1944, RF Fellowships: Uruguay, box 56, “José Luis Massera” folder. A copy of the entire letter, in Spanish, is included in Massera’s own records related to his Rockefeller fellowship in the Massera Papers, folder 5A. Hill also noted that Massera’s colleague Rafael Laguardia was then visiting Harvard on his own Rockefeller fellowship, having impressed Birkhoff on the latter’s 1942 South American travels. Laguardia appeared next to Massera as well in Stone’s report, and Miller’s annotations about Laguardia but not Massera suggest that he read Hill’s and Stone’s reports within a few months of each other.
[54] Massera to Wells, 26 Feb 1947, Massera Papers, folder 5A. Massera initially projected to Miller that he would travel through New York, but FBI records indicate he ultimately traveled instead via Miami. Memos of 12 May and 2 Jun 1947, FBI Massera File, Section 01.
[55] Laguardia to Miller, 2 Jul 1944, RF Fellowships: Uruguay, box 56, “José Luis Massera” folder.

los Pueblos Libres organization.[56] It was only later that Miller learned that Massera was a fast-rising member of the Uruguayan Communist Party.[57] The fact itself did not appear to bother Miller, who awarded fellowships to multiple avowed Communists in this period, but neither was it lost on him that this would complicate Massera's entry into the United States. Through his contacts in the region, Miller sought consular assurances over Massera's prospects. By January, Rockefeller officials were confident enough to joke in letters amongst themselves that Massera "will probably try to convert the people at Stanford."[58] Massera later shared this humor in a letter to his prospective Stanford supervisor expressing his disappointment at the "anti-democratic" roadblock to his voyage: "I am a Communist and […] [m]aybe [the State Department] think that I was going to organize the Social Revolution at Stanford University."[59]

For U.S. authorities, Massera was no laughing matter. In March, 1945, Massera's visa was denied, and Miller began to work through the Foundation's contacts with U.S. immigration authorities, the office of the Attorney General, and the State Department, while expressing a greater pessimism to Rockefeller colleagues. In October, 1946, Miller learned that Massera was

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56 Massera to Miller, 15 Jun 1944, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. The organization's name (which does not appear in the Rockefeller dossier) is from a biographical sketch in Massera Papers, folder 11B. An accompanying translation gives the name Antifascist National Action of Solidarity Towards the Free Peoples.

57 Massera joined the Partida Communista de Uruguay in 1942, was elected to its Central Committee in 1950, and joined its Executive Committee in 1955. For more on Massera's political and scientific lives (and their intersections), see Vania Markarian, ed., *Un Pensamiento Libre: Cartas de José Luis Massera* (Montevideo: Archivo General, Universidad de la República, 2005); Roberto Markarian and Ernesto Mordecki, eds., *José Luis Massera, Ciencia y Compromiso Social* (Montevideo: Pedeciba, 2010); Vania Markarian, "José Luis Massera, Matemático Uruguayo: Un Intelectual Comunista en Tiempos de Guerra Fría," *Políticas de la Memoria* 15 (summer 2014-2015): 215-224.

58 Hackett to Miller, 23 Jan 1945, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. An assurance from the U.S. Consulate in Montevideo is Sparks to Rockefeller Foundation, 4 Jan 1945, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder.

59 Massera to Szegö, 7 Jan 1946, Massera Papers, Folder 5A. FBI analyst Donald L. Allen excerpted from this letter in his report of 11 May 1948, FBI Massera File, Section 02.

running for the Uruguayan Congress, though unlikely to win a seat. Angst at the embassy led Miller to muse about the possibility of awarding Massera a fellowship to go to Russia, instead.[60] By January, 1947, however, he learned that a visa could be granted (though the FBI records this was over the protests of the American Ambassador to Uruguay), and by March Massera was off to California.[61]

The surprises and adaptations did not stop there, however. Miller's confidant in the Rockefeller Foundation's International Health Division in Buenos Aires, in addition to helping Miller with local negotiations over Massera's visa, had at an early stage suggested Massera might be well-suited to a stay at Stanford or Princeton, or perhaps even to splitting his fellowship between the two institutions. Miller encouraged Massera to consult with several contacts, particularly Alberto González Domínguez of Buenos Aires, to explore his possibilities for placement further. González Domínguez pointed Massera to Stanford University's Gábor Szegö and two of Szegö's colleagues, a suggestion whose pertinence Massera confirmed by tallying the institutional affiliations of everyone whose papers were indexed under "functions of a complex variable" or "differential equations" in the 1941 and 1943 editions of *Mathematical Reviews*. Massera also estimated that Stanford's proximity to Los Angeles and the California Institute of Technology would still further increase the value of a stay in Stanford, although Miller annotated that particular claim with a "sic!" in his translated copy of Massera's proposal.[62]

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[60] Miller diary, Montevideo, 23-26 Oct 1946, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. The initial visa denial was communicated to Miller in a confidential letter Pierson to "Dusty" [Miller], 7 Mar 1945, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder.

[61] Caldwell to Miller, 8 Jan 1947, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. Memo, 4 Apr 1947, FBI Massera File, Section 01.

[62] Massera to Miller, 30 Nov 1944, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. The two institutions are separated by more than 350 miles.

Massera and Miller both wrote directly to Szegö, who wondered in his reply to Miller whether Massera's interests were as well-aligned with his own as was supposed, but who nevertheless assured both correspondents that Massera would be welcome at Stanford.[63] Miller, eager to set the fellowship in motion, was sufficiently satisfied to approve the destination. Upon meeting, however, Szegö and Massera were mutually disappointed. Szegö worried to Miller that, although Massera was an able mathematician, his command of the relevant literature was significantly impaired by his lack of access to recent journals in Montevideo.[64] Massera, for his part, found Szegö an outstanding mathematician but also one much too busy to provide the sort of mentorship Massera desired.[65] By September, Massera had sought and received approval to transfer his fellowship to the East Coast, where he was based at Richard Courant's institute at New York University but commuted by train for part of each week to study at Princeton University under Solomon Lefschetz. He greatly impressed both men, but found the latter's seminar more conducive to his studies than the former's intensely focused research group.[66]

While he was in the United States, government agents followed his movements, intercepted his mail, covertly copied his diary, cultivated informants in his Stanford department and Palo Alto residence, investigated the Russian-born Lefschetz, and interviewed Szegö, Miller, and others, though his FBI file reflects uncertainty over some of his whereabouts and activities.[67]

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[63] Szegö to Massera (Massera's copy), 17 Oct 1944; Massera to Miller, 30 Oct 1944; Miller to Massera, 14 Dec 1944, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. Massera Papers, Folder 5A contains further Massera-Szegö correspondence.
[64] Miller diary, Stanford, 13-14 Jun 1947, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder.
[65] Massera exit report, Sep 1948, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder.
[66] Miller diary, 15 Sep 1947; GRP diary, 6 Oct 1947; Courant to Maggi, 10 Mar 1948; Massera exit report, Sep 1948, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder.
[67] E.g. Szegö to Massera, 18 Feb 1948, Massera Papers, Folder 5A; Memo, 8 Apr 1947, and Report of Charles G. Campbell, 21 Aug 1947, FBI Massera File, Section 01; Reports of Donald

"Massera allegedly has the intelligence and training to understand atomic bomb formulas if available," wrote one FBI analyst near the start of Massera's trip, and the Bureau noted every possible contact with American nuclear physics.[68] While atomic secrets motivated some of their early concerns over Massera, the FBI was ultimately most preoccupied by his more conventional political interests and his potential as a propagandist. Indeed, Massera's correspondence indicates that he closely followed American politics, and he noted news items and other observations that marked the flaws of American capitalism.[69] His office-mate at New York University could not recall Massera mentioning politics at work, and mathematicians who were not interviewed by the FBI about Massera appear to have remained unaware that he was a source of such political consternation.[70] Outside of the office, however, the FBI tracked every hint of potential contact with American Communists, recording for instance that he was received as "That important man from Uruguay" at the San Francisco headquarters of the U.S. Communist Party.[71]

L. Allen, 11 May 1948, and Alfred B. Novak, 5 Jun 1948, FBI Massera File, Section 02. A memo of 18 Apr 1947 claims Massera was at MIT at the start of his fellowship. The initial report on Lefschetz is by John H. Lupton, 5 Mar 1948, FBI Massera File, Section 02.

[68] Memo, 4 Mar 1947, and Report of Charles G. Campbell, 21 Aug 1947, FBI Massera File, Section 01. On the problematic and consequential notion that atomic bombs could be expropriated on the basis of secret formulas, see David Kaiser, "The Atomic Secret in Red Hands? American Suspicions of Theoretical Physicists During the Early Cold War," *Representations* 90 (2005): 28-60.

[69] See, e.g., Massera Papers, folders 26A and 26C.

[70] Nirenberg (personal communication, 14 Mar 2014). Peter Lax (personal communication, 27 Mar 2014), another member of Courant's group, had a similar recollection. Their impressions match those of Paul Halmos when interacting with Massera in Montevideo: "From personal observation I wouldnt [sic] know that Massera is a communist. […] He is a remarkably pleasant almost placid type and he certainly doesn't do anything like going around and spreading propaganda among students at the institute. Just the opposite: as far as I can tell from external observation he is the only one at the institute who really works." Halmos to Stone, 15 Oct 1951, Stone Papers, A95-32, box 38, folder 15.

[71] Memo, 15 Sep 1947, FBI Massera File, Section 01.

In a move not uncommon for fellows whose geographic remove made further U.S. travel in the near future unlikely, Massera arranged to delay his return to Uruguay until after an American Society of Engineering Education meeting in June, 1948.[72] When Miller next saw him that October, he had just completed a short stay in prison. Miller recorded in his diary that Massera "will continue to engage in political activities (Communistic)," adding "He said that the overthrow of the U.S. Government is not the business of Uruguayans, that it is up to the Communists in the U.S.A.!"[73] The next the Foundation heard from him was the following March, by way of a clipping from a communist periodical where Massera recounted the "Yankee Gestapo" actions against Edward Condon and railed against the "hypocritical blanket" of American "Freedom of Investigation."[74]

Massera's work while a fellow led to several publications in leading American journals, including the *Annals of Mathematics* edited by Lefschetz.[75] His command of Russian (a skill he pursued out of political conviction) put him in demand as a reviewer for *Mathematical Reviews*. Fifteen of the sixteen reviews he completed of works published before 1950 were of Russian-language articles, and Lefschetz (among others) considered him "the best master outside of the USSR" on his topics of research.[76] In the mid-1950s, Massera helped mediate the International

[72] Miller diary, 1 Jun 1948, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder.
[73] Miller diary, 13-15 Oct 1948, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. Miller did not record the reason for the imprisonment, though it was likely political.
[74] News clipping transmitted by Rivera to Miller, 11 Mar 1949, RF Fellowships: Uruguay, box 56, "José Luis Massera" folder. On the Condon affair, discussed briefly in chapter 3, see Jessica Wang, "Science, Security, and the Cold War: The Case of E. U. Condon," *Isis* 83, no. 2 (1992): 238-269.
[75] J. L. Massera, "The Number of Subharmonic Solutions of Non-linear Differential Equations of the Second Order," *Annals of Mathematics (2)* 50 (1949): 118-129. Massera, "On Liapounoff's Conditions of Stability," *Annals of Mathematics (2)* 50 (1949): 705-721.
[76] Lefschetz's comments were in the context of forwarding Massera two articles originally assigned to Lefschetz to review for *Mathematical Reviews*; Lefschetz to Massera, 14 Feb 1949,

Mathematical Union's outreach to the Soviets, who were not initially party to the Union.[77] These broad international ties would make him a cause célèbre some three decades later, when an international group of left-leaning mathematicians (led in part by Laurent Schwartz) agitated for his release from an extended political imprisonment in Uruguay.[78]

Linked to leading American mathematicians by more than half a decade of travel and correspondence, Massera and his colleague Rafael Laguardia at the Universidad de la República in Montevideo eagerly anticipated their return to the American Northeast in 1950. By the early months that year, both mathematicians planned to attend both the International Congress at Harvard and Stone's meeting on the International Mathematical Union at Columbia immediately prior. Their American counterparts shared their enthusiasm for such a reunion. As Courant explained to the Dean of their Faculty of Engineering that March, "the previous visits of these two men to this country have greatly strengthened here the desire to maintain and increase scientific ties between United States' and Uruguayan mathematicians" and "it would be extremely worthwhile if these two colleagues could return for a short time on the occasion of the Congress."[79] Courant recognized that funding would be one obstacle to their attendance, and vowed to the Dean to help however he could in that regard.

Laguardia, meanwhile, wrote to Stone to express that he and Massera were "convinced that our attendance at the Congress would be very useful, not only for us but above all for the

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Massera Papers, folder 5A. Massera is credited with more than 200 reviews in the MathSciNet database of *Mathematical Reviews* entries.

[77] E.g. Massera to Topchiev, 31 Jan 1955, Massera Papers, folder 5A.

[78] On Massera's imprisonment (from 1975 to 1984), see Markarian, *Un Pensamiento Libre;* Markarian and Mordecki, *José Luis Massera*; Reuben Hersh and Vera John-Steiner, *Loving and Hating Mathematics: Challenging the Myths of Mathematical Life* (Princeton: Princeton University Press, 2010), ch. 3.

[79] Courant, 8 Mar 1950, Massera Papers, folder 5A.

development of mathematics in our country," but added "there is an infinity of difficulties." He had arranged for his university to name them delegates, but it appeared there were no funds for that sort of scientific congress and these would have to be solicited from Parliament, which "might not do anything or might reduce the delegation to some professor of secondary teaching who is on a recreational trip." Most importantly, he learned from the director of UNESCO's Science Cooperation Office for Latin America (itself based in Montevideo) that UNESCO had granted travel funds for both the ICM and Stone's IMU meeting, and hoped Stone could advise him on how much they could expect to receive "so that our country is represented at the Congress."[80]

The paper trail then drops away for a few months, resuming on 22 July with Massera bringing Stone up to date with reference to a now-missing letter from Stone to Laguardia of 10 July.[81] Massera thanked Stone profusely for his willingness to help "to solve the probable difficulties that could take place in obtaining my visa to attend the Congress." "Unfortunately," he continued, "it is possible that those will not be the only difficulties to present themselves." Specifically, Kline promised the Uruguayan delegation $400 in travel assistance out of UNESCO's grant for the IMU conference and Garrett Birkhoff, chair of the ICM Organizing Committee, committed a further $500 from the Congress's travel funds. But the Uruguayans had just heard again from Birkhoff that in view of Kline's contribution "and the financial exigencies of the Congress" he was obliged to reduce the Congress's contribution to $200. Under the circumstances, this would cover only Laguardia's costs, not both mathematicians' as intended.

[80] Laguardia to Stone, 3 Mar 1950, Laguardia Papers, box 18, folder 9.
[81] Massera to Stone, 22 Jul 1950, Massera Papers, folder 5A.

Massera hoped to obtain funds domestically to make up the difference, but he suspected it would not be easy.

Matters came to a head at the start of August. By then, neither Laguardia's nor Massera's visas for the Congress had been approved—unsurprisingly in Massera's case but puzzlingly for Laguardia. Absent further funding and undoubtedly with memories of the drawn-out process of securing travel permission for Massera for his last visit to the United States, their university determined that Laguardia should be the one to attend the Congress.[82] Massera wrote on 4 August to inform Kline, without stating a reason, that he would be unable to attend.[83] Having needed both further funding and a visa and having received neither so close to the Congress, Massera had no shortage of targets to blame, but at that point it was too late for recriminations. Without his going through all the formalities of a denial and appeal for his latest application, it was clear that Massera's history of difficulty with U.S. authorities in Montevideo and Washington nonetheless figured decisively in his inability to travel northward in 1950.

Even then, with Massera's own visa case a moot point, U.S. reactions to his politics created trouble for the Congress. The same day Massera wrote his letter to Kline, Laguardia sent urgent cables to both Kline and Stone: "consulate refuses my visa[,] not having and not giving any reason[;] please do something."[84] He elaborated in a letter to Stone that "you can be absolutely sure that your precautions for Massera's case are not applicable to me, because my life transpires exclusively between my home and the University." Specifically, "I have no

[82] Laguardia to Stone, 4 Aug 1950, Laguardia Papers, box 18, folder 9.
[83] Massera to Kline, 4 Aug 1950, Massera Papers, folder 5A. Kline replied only after the Congress, assuring Massera that his abstract would be published in the Proceedings nonetheless. Kline to Massera, 15 Oct 1950, Massera Papers, folder 5A.
[84] Cables in Laguardia Papers, box 17, folder 9 (Kline) and box 18, folder 9 (Stone), dated according to Stone's response in box 18, folder 9.

political activity of any type, I do not belong to any political party either ostensibly or secretly, nor any cultural organization with political derivations."[85] In letters to both Kline and Stone on 8 August, Laguardia stressed that the refusal was "completely unexpected" and that on 7 August he had spoken again with the American Consul in Montevideo to insist that "he had no basis to refuse my visa on political grounds because I have no political activity whatsoever, I do not belong to any political party or cultural organization with political implications." The Consul informed Laguardia that "he has suspicions about my 'ideology'" and "was to study the possibility of reconsidering his decision and to give me his answer next week."[86]

Upon receiving Laguardia's cable, Stone placed a long-distance telephone call to Kline, who then "communicated with our attorney in Washington who in turn took up your case with the Visa Section of the State Department, as well as certain other groups which are interested in the International Exchange of Persons."[87] Kline informed Laguardia that he "also enlisted the services of the Guggenheim Foundation" and cabled the United States Consul in Montevideo to press his case. "I hope most sincerely that this matter may be resolved in your favor shortly," Kline concluded, and "You may rest assured that everything possible will be done from this end."[88]

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[85] Laguardia to Stone, 4 Aug 1950, Laguardia Papers, box 18, folder 9.

[86] Quotations from Laguardia to Kline, 8 Aug 1950, Laguardia Papers, box 17, folder 9. Cf. Laguardia to Stone, 8 Aug 1950, Laguardia Papers, box 18, folder 9. Throughout this episode, Laguardia's typewritten letters to Stone were in Spanish, from which he appears to have translated a version for Kline into English, shortening the letter somewhat in the process while customizing it to refer to his past correspondence with Kline instead of Stone.

[87] Kline to Laguardia, 8 Aug 1950, Laguardia Papers, box 17, folder 9. I have inferred that Stone telephoned Kline (rather than the other way around) because Laguardia's cable to Stone mentioned Kline but his cable to Kline did not mention Stone.

[88] Ibid.

Stone was not so confident. In a return cable on 4 August he assured Laguardia that he "will do everything possible" and asked whether the "consulate consulated [sic] Washington or acted independently."[89] Stone then wrote out a longhand letter for airmail to Laguardia later that day, cautioning that "There is great danger that there will not be enough time to achieve our aim, because matters of this kind <u>always</u> move slowly."[90] He elaborated from his cable that if the Montevideo Consul did not consult Washington then the lack of information about the case in Washington could cost them time. Stone shared that "Two very difficult visa cases had to be handled in connection with the Congress," those of Hadamard and Schwartz (discussed below), and "It took a great deal of time and effort to persuade the authorities to grant the visas." After summarizing those cases, Stone averred that "In your case, the obvious factor is your close personal and official association with Massera." Stone concluded by forcefully asserting a sentiment that cut strongly against Kline's constant refrain of optimism: "You know how ashamed and angry this makes me. Since a year ago I have believed that the Congress should <u>not</u> be held in the U.S. on account of the attitude of the government towards liberal and radical scientists from other countries. Unfortunately I have had no influence on the Congress and have been forced to watch one disgraceful incident after another." To his trusted correspondent in Montevideo, Stone laid bare an insider's view of a project of internationalism disastrously undercut by his own government.

On 14 August Stone shared the news that "the State Department in Washington has taken a real interest in the problem of your visa and has probably already asked the Consul for information." He added that "Gonzalez Dominguez had a little trouble over his visa at the

[89] Cable, Stone to Laguardia, 4 Aug 1950, Laguardia Papers, box 18, folder 9.
[90] Stone to Laguardia, 4 Aug 1950, Laguardia Papers, box 18, folder 9.

Consulate in Buenos Aires" because "Doubts as to his 'ideology' were created by an old connection he had had with some Soviet-Argentine cultural association." In that case, "doubts were dispelled by a statement from the Chief of Police of Buenos Aires, undoubtedly an authority on all political thought in the Capital (and presumably an enemy of it!)."[91] The next day, before Stone's letter had time to reach Laguardia, the latter cabled with good news of his own: "I obtained my visa[.] Many thanks to you and Kline."[92] He wrote the subsequent day to inform Kline as well.[93]

The Schwartz Affair I: A Definitive Showdown

Massera's political troubles, for which Laguardia's attendance narrowly avoided becoming collateral damage, played out behind the scenes in letters and cables between a small core set of mathematicians and a similarly confined network of diplomats and officials, mostly in the final weeks before the Congress. Its most complete paper trail resides in a pair of folders saved, respectively, by Massera and Laguardia and now stored in their archives in Montevideo. The Congress's best known diplomatic episode, by contrast, unspooled for nearly an entire year. It made it into the Organizing Committee's official minutes, was the subject of debate and speculation in both the United States and France, and threatened, for quite a few, the viability of the Congress itself. The affair began when the American organizers learned in the second half of

[91] Stone to Laguardia, 14 Aug 1950, Laguardia Papers, box 18, folder 9.
[92] Cable, Laguardia to Stone, 15 Aug 1950, Laguardia Papers, box 18, folder 9.
[93] Laguardia to Kline, 16 Aug 1950, Laguardia Papers, box 17, folder 9. Laguardia evidently met with Harry Miller while in New York for the IMU meeting. On 29 Aug 1950, Miller recorded an update from Laguardia: "L. said that when the U.S. Consul in Montevideo seemed hesitant about granting him a visa to come to the U.S., he insisted that a thorough investigation be made of the rumors that he is a Communist. The Consul, fide L., finally admitted that he has an entirely clear slate." RF Projects: U.S., box 125 folder 1546.

1949 that Laurent Schwartz had been denied a visa, and it later came to include visa negotiations and appeals for Schwartz's great uncle Jacques Hadamard, one of the Congress's honorary presidents.[94]

Today, Schwartz's visa troubles have the status of legend among mathematicians and historians of mathematics. Schwartz's memoirs offer a relatively detailed record of this legend, based on Schwartz's firsthand recollection of the information available to him during and shortly following the crisis itself.[95] As Schwartz told it, he applied for a visa upon his invitation to spend three weeks in Princeton, but was refused "well after the projected date of the voyage."[96] At issue, it seemed, was Schwartz's candidacy with the Trotskyist Party in Grenoble legislative elections in 1945 and 1946, though Schwartz claimed to have fallen away from the party shortly thereafter.[97] In autumn, 1949, he learned that his visa refusal was effectively permanent: "It was in full MacCarthyism [sic], and I was catalogued for life as a former Trotskyist."[98] This did not

[94] On Hadamard, see Vladimir Maz'ya and Tatyana Shaposhnikova, *Jacques Hadamard: a universal mathematician* (Providence: American Mathematical Society, 1998).

[95] Laurent Schwartz, *A Mathematician Grappling with His Century*, trans. Leila Schneps (Basel: Birkhäuser, 2001) [*Un mathématicien aux prises avec le siècle* (Paris: Odile Jacob, 1997)], 311-316 [319-325].

[96] Schwartz, *Mathematician Grappling*, 311 [319].

[97] Schwartz, *Mathematician Grappling*, 256 [268]. Schwartz's recalled break with Trotskyism should be taken with a grain of salt, and there is a significant polemical historiography of the revisionism of the French left. E.g. Raymond Aron, *L'Opium des intellectuels* (Paris: Calmann-Lévy, 1955); Alain Besançon, *Une Génération* (Paris: Julliard, 1987); François Furet, *The Passing of an Illusion: The Idea of Communism in the Twentieth Century,* trans. Deborah Furet (Chicago: University of Chicago Press, 1999); Tony Judt, *Past Imperfect: French Intellectuals, 1944-1956* (Berkeley: University of California Press, 1992); Mark Lilla, *The Reckless Mind: Intellectuals in Politics* (New York: New York Review of Books, 2001); Pierre Rigoulot, *Le Paupières Lourdes: Les Français face au goulag: aveuglements et indignations* (Paris: Editions universitaires, 1991); Jeannine Verdès-Leroux, *Au service du parti: Le Parti communiste, les intellectuels et la culture (1944-1956)* (Paris: Fayard, 1983).

[98] Schwartz, *Mathematician Grappling*, 311 [319]. While 1949 was certainly a time of official anticommunism, McCarthyism as a term and phenomenon associated with Joseph McCarthy dates only to 1950.

augur well, as that summer he received a letter informing him of his Fields Medal, which was to be presented at the 1950 Congress in the United States.[99] The American organizers hired lawyers and "laid siege" to the State Department.[100] The French threatened to boycott, along with "a good number" of sympathetic Americans.[101] Schwartz finally received his visa "a few months before the date of the congress, but it required nothing less than the intervention of president Truman himself."[102] Hadamard's visa took longer and required another intervention from Truman, allowing the visa to be issued one day before the French delegation's deadline to confirm or cancel their boat tickets.[103]

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[99] Ibid.

[100] Ibid., 312 [321].

[101] Ibid., 312-313 [321].

[102] Ibid., 313 [321]. Those involved at the time were aware that, as a legal matter, final word rested with the Attorney General (at the time, J. Howard McGrath), as visas were ultimately issued through the Immigration and Naturalization Service under the Department of Justice—though officials from the Department of Justice did routinely consult the Department of State in situations like this. Records of Acheson's exchanges with Truman for March through August 1950 make no mention of the situation. Cf. the discussion of visas for foreign Communists for a conference sponsored by Harlow Shapley's National American Council of the Arts, Sciences and Professions; Memorandum of Conversation with the President, 14 Mar 1949, Acheson Memoranda. I thank Randy Sowell of the Harry S. Truman Library for confirming that there are no references to Schwartz or Hadamard in the correspondence or INS files in the J. Howard McGrath Papers, nor any references in the General File of the Truman Papers. I have not yet examined the General Records of the Department of Justice for references to the case. A Freedom of Information Request for FBI records relating to Hadamard returned no responsive material, and Schwartz's FBI file does not indicate a Truman's involvement. Schwartz's visa difficulties continued for much of his life. He blamed these problems on the long memory of his Trotskyism, but his FBI file makes clear that his activism against the Vietnam War was ultimately the main impediment to having his status normalized as early as the 1960s, and after the end of the Vietnam War the FBI did not express (nor record from other departments) further interest in his case. Schwartz also never learned that during his period of Vietnam activism a U.S. President—Lyndon Johnson—did in fact personally overrule a recommendation from Secretary of State (and former Attorney General) Nicholas Katzenbach that he be admitted, the opposite of what Schwartz asserted Truman had done in 1950. Rostow Action Memorandum, 24 Apr 1968, LBJ Memoranda, folder 002871-004-0605, accessed at http://congressional.proquest.com/histvault?q=002871-004-0605.

[103] Schwartz, *Mathematician Grappling*, 313 [322].

Like any legend, Schwartz's recollection is a mix of fact, exaggeration, and invention. None of the actors had a comprehensive view of the situation at the time, so there was never a definitive original narrative to distort. Some aspects of the story, especially concerning Hadamard, continue to elude historical scrutiny. Some pivotal documentation, such as what Robert Oppenheimer referred to as "all the awful dossier about Laurent Schwartz" compiled by Marshall Stone, may be scattered or lost.[104] However, a close examination of available archival sources from the period—including some, like Schwartz's FBI file, that have only become available as a result of the research for this dissertation—permits a significant revision and correction to the story. By fixing certain dates and filling in missing actors and events, one obtains a substantially more complex account of how Schwartz's visa troubles emerged, how they were eventually resolved, and what consequences they had for the Congress.

At least one of the American organizers saw it coming. When deliberating over hour speakers in Analysis, Morse left Laurent Schwartz off of his committee's first round of balloting because he "would have difficulty in getting a visa."[105] Morse must have reached that conclusion toward the close of 1948, and he shared it with his committee on 21 December. Schwartz was soon added into consideration, but Morse's assessment would rapidly prove one of the most spectacular understatements of the Congress.

Harald Bohr set the affair in motion with his side-trip to Montreal during his visit to Courant and Veblen in the spring of 1948. That summer, on the strength of Bohr's recommendation, Schwartz was invited to lecture at the Canadian Mathematical Congress from mid-August to mid-September, 1949. Word reached the mathematicians at Veblen's and Morse's

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[104] Oppenheimer to Stone, 25 Jul 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[105] Morse to Ahlfors, Bochner, Evans, Hildebrandt, Hille, von Neumann, Rademacher, Stone, 21 Dec 1948, Morse Papers, box 8, "ICM – Select Speakers" folder.

Institute for Advanced Study, already introduced to Schwartz's theory on Bohr's visit, that Schwartz himself would be crossing the Atlantic soon. It was not uncommon for American institutions to pool resources to share the costs and rewards of hosting visiting mathematicians from Europe, allowing those mathematicians to make the most of an arduous Atlantic crossing. Perhaps the IAS's mathematicians had such an arrangement in mind when they voted to invite Schwartz to join them following the Canadian meeting, but whatever their intentions they supplied the next link in Schwartz's fateful chain of events. On 8 December 1948, John von Neumann wrote to invite Schwartz to visit the IAS from October-December, 1949, and to express personally "that it was a quite exceptional pleasure for me to learn of your very interesting results on Distributions."[106]

Schwartz accepted von Neumann's invitation. Recalling that Richard Courant, following Bohr's visit, had previously invited him to visit Courant's institute at New York University, Schwartz wrote to Courant on 27 December, 1948, to express his extreme desire to visit the latter's institute "and to study the applications of distributions to practical problems."[107] In order to facilitate his arrangements, Schwartz wrote in February to ask that an official invitation be sent via the French Cultural Attaché in New York. After consulting with fellow Bourbaki collaborator Claude Chevalley, then at Columbia University, Schwartz also asked about the availability of housing at the Institute.[108] Robert Oppenheimer conveyed the desired letter to the

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[106] Von Neumann to Schwartz, 8 Dec 1948, IAS Member Files, box 125, "Schwartz, Laurent" folder. See also von Neumann to Schwartz, 1 Mar 1949, confirming Schwartz's stipend and official invitation.
[107] Schwartz to Courant, 27 Dec 1948, Courant Papers, box 79, folder 18.
[108] Schwartz to IAS ('Cher Monsieur'), 25 Feb 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.

Cultural Attaché shortly thereafter, on von Neumann's request.[109] As for housing, Schwartz was informed "the Institute authorities will do their best" but there were no guarantees.[110] At the end of March, Courant informed Schwartz that any time after his stay in Princeton "would be very agreeable to us in New York University for your visit."[111] In reply to Courant, Schwartz revealed, first, that he hoped to shuttle between Princeton and New York during his IAS residency rather than visit consecutively; and, second, that Marshall Stone had also invited him to visit Chicago in July *en route* to Vancouver.[112] Schwartz now expected to be in the United States in a matter of months, and he needed a visa.

As far as Schwartz's correspondence with the IAS indicated, everything was on track through 15 May, when Schwartz lodged his official response to the formal letter he had requested of Oppenheimer.[113] Then, on or before 20 May, Morse heard word of visa trouble from Stone. "Since Laurent Schwartz is invited to come to the Institute this fall," Morse explained to Oppenheimer, "I suppose we have a responsibility in giving this case a thorough tryout." Under the circumstances, Morse thought the best approach was to confront the matter head-on: "He is one of the best young mathematicians in the world, reportedly a left-winger. His case is critical for the International Congress of Mathematicians in 1950. The Institute has here a concrete forerunner of our [the Congress's] problems. […] A definitive showdown under the best

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[109] Von Neumann to Oppenheimer, 4 Mar 1949, Oppenheimer to Schwartz, 5 Mar 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[110] Von Neumann to Schwartz, 7 Mar 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder. See also Schwartz to IAS (Mrs. Leary), 21 Mar 1949.
[111] Courant to Schwartz, 24 Mar 1949, Courant Papers, box 79, folder 18.
[112] Schwartz to Courant, 10 Apr 1949, Courant Papers, box 79, folder 18.
[113] Schwartz to IAS ('Cher Monsieur'), 15 May 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.

conditions is desirable."[114] Morse would have his definitive showdown, but the result would worsen rather than ameliorate the Congress's problems.

The problem for Morse, as we saw in the initial negotiations over creating a new International Mathematical Union, was that Marshall Stone was an unreliable guarantor of the "best conditions" for international diplomacy. Stone summarized the situation to Oppenheimer in July, at which point Stone's own University of Chicago had "done about all we can alone" and he was resigned to hosting Schwartz in January, 1950, after his stay at the IAS.[115] Recognizing a politically difficult case, Stone inferred, consular officials in Strasbourg "were inclined to solve the problem by delaying action." They ultimately referred the case to Washington with no indication of urgency, at which point Stone intervened via the newly-elected U.S. Senator from Illinois, Paul Douglas, to seek a quick decision. As with his bid two years prior to organize the IMU through ICSU, Stone's attempt to force the issue backfired in a way that erased what little hope had existed for Stone's preferred course of action.

Douglas turned to Jack Peurifoy, a high-ranking State Department official, who issued a quick negative recommendation. Stone speculated that Peurifoy was "somewhat nervous" after a difficult hearing in front of the Senate Judiciary Committee, and may not have had a full report from Strasbourg on Schwartz's presumably anodyne "present political views." In any event, the matter then returned to the Strasbourg Consulate, which "has actually refused to issue the visa or has simply not acted on the application." Stone hoped "that a very skillful handling of the case might still produce a favorable decision." But he admitted "The probabilities are very low, I am

[114] Morse to Oppenheimer, 20 May 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[115] Stone to Oppenheimer, 22 July 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.

sure – one chance in a thousand seems a generous estimate to me." After speculating about possible next steps, including pursuing an appeal through the Consulate in Vancouver and appealing to the Attorney General, Stone concluded his summary to Oppenheimer with the exhortation that "it will be necessary to assess the wisdom of making an issue of a case which has so few apparent prospects of success. I think that failure would be preferable (as more honorable) to inaction, unless our persistence in this case would jeopardize success in less difficult ones arising in the future. […] It is also well to keep in mind that each case where a scientist of repute is denied a visa strengthens the hands of those who would clamp all kinds of restrictions on liberty of thought, and at the same time costs us friends in powerful intellectual circles abroad."

Oppenheimer was matter-of-fact in reply: "We know Jack Peurifoy, and I will try to find out on what grounds, with what conviction, and with what finality he made his determination." He added that "I have discussed the general problem with the Secretary of State, but whether this is or is not a specific case to try to battle through I cannot tell until matters are clearer."[116] On 19 August, 1949, Stone gave Oppenheimer an unwelcome update, that the Strasbourg Consulate officially denied the visa application in a notice dated 3 August.[117] Morse pressed ahead, convincing the French Cultural Attaché in New York to petition the French Minister of Foreign Affairs for diplomatic credentials for Schwartz, with which Schwartz could obtain a diplomatic visa from the United States.[118] Officials at the French Embassy avowed that "we profoundly

[116] Oppenheimer to Stone, 25 Jul 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[117] Stone to Oppenheimer, 19 Aug 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[118] EWL [likely Eleanor Leary] to Oppenheimer, 7 Sep 1949; Basdevant to Morse, 19 Sep 1949, with Morse annotation 21 Sep 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder. I thank Erica Mosner and Casey Westerman for their "EWL" identification.

deplore that the personal political views of a scholar prevent establishing contact on a uniquely scientific rubric with his foreign colleagues."[119] Von Neumann considered Morse's attempts that September to work through French diplomatic channels to influence American ones to be "new and quite promising," but they proved too little and too late for Schwartz's prospects at the Institute for Advanced Study, at least before his trip for the 1950 Congress.[120] Schwartz's Canadian adventure ended without any of his hoped-for prolongations in the United States.

The Schwartz Affair II: Hopes and Promises

Having recommended Schwartz for an hour speaker invitation, Morse was attuned from the start to the potential implications for the Congress of Schwartz's unresolved visa case. The Organizing Committee confirmed the foreign hour speaker list at the end of April, so Schwartz would have received his invitation in the late spring or summer—a piece of news he conflated with his Fields Medal notification in his memoir.[121] Morse kept his fellow organizers apprised of the situation over the summer of 1949, but even that August some of the organizers considered Schwartz's case merely indicative of the Congress's potential diplomatic hurdles, rather than a diplomatic hurdle for them in itself. As one member put it, "I feel very strongly that although the Congress has nothing to do with the Schwarz [sic] case, the latter is a pretty plain indication that before we go any further with plans for a Congress we should have a written indication from the State Dept. that noone [sic] will be refused entry to the U.S. (by <u>our</u> State Dept.) for attendance

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[119] Basdevant to Morse, 19 Sep 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[120] Von Neumann to Schwartz, 10 Oct 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[121] Cartan records that several French mathematicians received their official invitations in July. Cartan to Weil, 21 Jul 1949, Audin, *Henri Cartan et André Weil,* 265-266.

at the Congress because of political beliefs."[122] By early October, however, the Congress's official committees had taken on Schwartz's case, "prosecuting the matter of visas, etc. […] very energetically with the proper authorities."[123] Kline reported at the Organizing Committee's end-of-October meeting that he had secured legal counsel in Washington to assist in negotiations with the State and Justice Departments. The committee discussed Schwartz's case at length, urging unity in their support, and were confident he would receive a term-limited *visa de service* (as opposed to an open-ended passport or diplomatic papers) in short order.[124]

Meanwhile, the Fields Medal committee debated whom to choose, knowing full well that their decision could give Schwartz's visa case yet another layer of implications for the Congress. The committee's deliberations were meant to be secret—and indeed, any official committee documentation in the International Mathematical Union's possession remains sealed to this day.[125] But the committee's chair Harald Bohr could not restrain himself from sharing morsels of news with his protégé Børge Jessen, allowing a rough reconstruction of their timeline and some features of their process.[126] In Schwartz's recollection, his Fields Medal preceded his visa troubles, but in fact the Fields Medal committee knew of his visa troubles well before completing their deliberations.[127] Given Bohr's opinion of Schwartz, the visa situation may indeed have provided an extra impetus to guide the committee to pick the embattled French mathematician.

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[122] Wilder to Kline, 25 Aug 1949, AMS Records, box 35, folder 48. Wilder inserted the parenthetical by pen to the typed letter.
[123] Von Neumann to Schwartz, 10 Oct 1949, IAS Member Files, box 125, "Schwartz, Laurent" folder.
[124] ICM Organizing Committee minutes, 29 Oct 1949, AMS Records, box 37, folder 56.
[125] It is possible, indeed likely, that no such documentation exists. The recently unsealed file for the 1936 Fields Medals was empty, and as in 1936 the 1950 deliberations took place before the IMU's reestablishment. Ingrid Daubechies (personal communication, 18 Feb 2016).
[126] I thank Jesper Lützen for compiling and translating the relevant fragments.
[127] Bohr to Jessen, 28 Sep 1949. Jessen Papers, box 10, "Bohr, Harald (1941-1951)" folder.

For Bohr's remarks to Jessen suggest the former's active intervention on Schwartz's behalf, though (as Bohr described the process) "it will require blood and tears."[128] Bohr noted only one candidate other than the eventual winners to Jessen, but the candidate was a significant one. In October, the committee was deeply divided over Schwartz's countryman and fellow Bourbaki collaborator André Weil, with Lars Ahlfors considering his candidacy "disastrous" while Damodar Kosambi declared it "ridiculous" if Weil did not win. Bohr resolved to split the vote, observing to Jessen that "exaggerations can sometimes help by canceling each other."[129] The committee ultimately confirmed Schwartz and Selberg as medalists near the start of December, 1949.[130]

Schwartz's selection for the Fields Medal compounded the potential embarrassment from his non-attendance, though it is unclear at what stage the American organizers working most closely on the visa issue became aware of the Fields Medal committee's decision. As chapter 5 will show, the selection also had profound consequences for Schwartz's career and for the global adoption of his theory of distributions. Bohr's leaked information on the committee's deliberations reveals, moreover, a further way Bohr's advocacy for Schwartz shaped not just Schwartz's career but the careers of countless younger mathematicians, as well as the very image and culture of mathematics. At the 1950 Congress, Bohr explained that his committee members "were, unanimously, of the opinion that the medals, as on the occasion of the first awards in Oslo, should be given to two really young mathematicians, without exactly specifying, however, the notion of being 'young.'"[131] It was not until 1966 that the Fields Medal committee, then

[128] Bohr to Jessen, 6 Nov 1949, Jessen Papers, box 10, "Bohr, Harald (1941-1951)" folder.
[129] Bohr to Jessen, 24 Oct 1949, Jessen Papers, box 10, "Bohr, Harald (1941-1951)" folder.
[130] Bohr to Jessen, 3 Dec 1949, Jessen Papers, box 10, "Bohr, Harald (1941-1951)" folder.
[131] Bohr, "Address," 127.

chaired by Georges de Rham, specified an age limit of forty for the award. That limit has since become a critical threshold for mathematicians who aspire to elite status in the discipline, as well as a frequently-cited marker of an association between youth and mathematical genius and productivity.[132] Yet it was largely an accident that no medalist exceeded age forty before de Rham's committee set its limit in 1966. By virtually any measure, as Kosambi's view affirms, André Weil was a more accomplished young French mathematician than Schwartz in 1949.[133] Had Ahlfors and Bohr not pushed Weil aside he may well have received a Fields Medal at age 44 in 1950, and it is doubtful that the limit would have been set as low as forty with Weil as a precedent.

Before October, 1949, Schwartz's visa troubles were a diplomatic concern between the American organizers and their government. As the Congress drew less than a year away, the visa issue became enmeshed in the ongoing diplomatic tensions between the American organizers and

[132] See George J. Borjas and Kirk B. Doran, "Prizes and Productivity: How Winning the Fields Medal Affects Scientific Output," *Journal of Human Resources* 50, no. 3 (2015): 728-758; János Kollár, "Is There a Curse of the Fields Medal?" *Notices of the American Mathematical Society* 62, no. 1 (2015): 21-25; Michael J. Barany, "The Myth and the Medal," *Notices of the American Mathematical Society* 62, no. 1 (2015): 15-20, on 17-18.

[133] Kosambi was not alone in his assessment. While trying to negotiate his visa to travel to Brazil to join the faculty in São Paulo, Weil asked for assistance from the Institute for Advanced Study. On behalf of the IAS faculty, Frank Aydelotte wrote "He is one of the ablest mathematicians in this or any other country" (Aydelotte, 6 Dec 1944) and elsewhere "Weil is one of the ablest of all the young French mathematicians," though he also implicitly justified Ahlfors's position by adding "he is somewhat difficult personally, and in my opinion will never fit comfortably into the organization of an American university. He is the kind of man who is critical of his surroundings wherever he is, and the best way to make him a loyal supporter of the United States would be to send him to Brazil." Aydelotte to Travers, 10 Jun 1944. See also Weil to Aydelotte, 3 Dec 1944. IAS Faculty Files, box 36, folder 2. Weil's 1957 letters of recommendation for joining the IAS faculty are full of statements like Jean-Pierre Serre's, that "among living mathematicians (in all branches of mathematics), he has no superior, if any equal." Serre to Montgomery, 27 Feb 1957. MIT's Kenkichi Iwasawa described one of Weil's pre-1949 results as "one of the most important contributions to mathematics in this century." Iwasawa to Selberg, 25 Feb 1957. See also Selberg's summary report on Weil's candidacy. IAS Faculty Files, box 36, folder 3.

their counterparts in the French Mathematical Society, as well. André Weil reported to Henri Cartan in July that Marshall Stone was encouraging "personal letters from French mathematicians […] expressing their reactions to the Schwartz affair," adding in pencil that "One can also envisage an official letter from the Société Mathématique to the American Math. Society." Weil opined that other hosts should be considered, "by reason of the fact that it becomes more and more evident that a Congress held in America would not have the international character that one desires to give it."[134] At the same time, Weil began agitating for the Congress's relocation to a location such as Denmark that he believed would present fewer diplomatic constraints to as broad participation as possible.[135] At their October meeting, the Americans recorded receiving a formal inquiry from the French over whether "qualified scholars, without political discrimination," would be admitted if they could assure that such scholars "would abstain from political activity" during their visits—a thinly-veiled reference to the Schwartz case.[136] As with the French IMU efforts of 1947, several American organizers viewed this (quite correctly if Weil is any guide) less as an earnest inquiry than as a challenge to their diplomatic wherewithal.

During the last quarter of 1949, the American organizers alternated between guarded optimism and indignant consternation at their progress (or lack thereof) with the State Department. On the one hand, they were convinced of the principle that politics should not impede participation. On the other hand, they insisted that their good efforts not be sullied by a

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[134] Weil to Cartan, 15 Jul 1949, Audin, *Henri Cartan et André Weil,* 263-264.

[135] E.g. Weil to Cartan, 15 Jul 1949, 25 Jul 1949, 31 July 1949, Audin, *Henri Cartan et André Weil,* 263-264, 267-272

[136] ICM Organizing Committee minutes, 29 Oct 1949, AMS Records, box 37, folder 56. Weil congratulated Cartan on his "excellent" letter to Kline in a letter of 13 Aug 1949, Audin, *Henri Cartan et André Weil,* 280-281.

few isolated bad cases. “As strongly as I feel that political beliefs should not interfere with the attendance at such an affair,” Raymond Wilder wrote to Kline, “I would have hated to have seen all the work that you and others have put into it gone for naught.”[137] By December, Kline had begun to make allowances for possible failures. If everything that could be done was done, he wrote at mid-month, and “for reasons beyond our control or power, we cannot secure the admission of a few individuals,” then this should not upset the “hope and promise for future international cooperation among mathematicians,” and his Congress, in particular.[138] Within American mathematics, whose luminaries continued to include those like Kline’s thesis advisor who railed against “Communists, Fellow-travelers, Reds, Pinks, New Dealers, Square Dealers, or any other type of dealers,” much hinged on the successful realization of a Congress untainted by political tests.[139]

There was reason to worry, in particular, as rumors began to circulate by the end of 1949 that the French would boycott the Congress if the visa situation were not satisfactorily resolved. While Kline was keen to point out individual French mathematicians, including Arnaud Denjoy and Szolem Mandelbrojt, whom he thought unlikely to join a boycott, the prospect of a boycott at any scale weighed on him.[140] So close to the Congress, Kline worried the visa situation might derail European participation more broadly. He relied on sympathetic European mathematical statesmen to rally to the Congress. After the Congress Kline recalled, on learning of Harald

[137] Wilder to Kline, 2 Dec 1949, AMS Records, box 35, folder 48.
[138] Kline to Wilder, 16 Dec 1949, AMS Records, box 35, folder 48; see also Kline to Wilder, 16 Dec 1949, AMS Records, box 35, folder 48; Wilder to Kline, 19 Dec 1949, AMS Records, box 36, folder 4.
[139] Kline to Wilder, 16 Dec 1949, AMS Records, box 35, folder 48; Wilder to Kline, 19 Dec 1949, AMS Records, box 36, folder 4.
[140] Kline to Wilder, 16 Dec 1949, AMS Records, box 35, folder 48. See also Schwartz, *Mathematician Grappling,* 312-313; Weil to Cartan, 3 Jul 1950; Cartan to Weil, 11 Jul 1950, Audin, *Henri Cartan et André Weil*, 298-302, and Audin’s note 304.2 on 602.

Bohr's death, that "He undoubtedly was one of the major factors in keeping all of the Europeans in line for the Congress when the difficult negotiations about visas were in progress."[141]

Provocations and misunderstandings continued to mount. Schwartz retained close contact with Bourbaki collaborators on both sides of the Atlantic, and they stayed abreast of his visa situation as part of their extensive written and in-person communication.[142] This "self-appointed group of young French geniuses," as Morse called them in 1949 (no doubt reflecting his consternation at the visa situation), made a custom of lampooning institutions like the American Mathematical Society that they saw as rigid, obstinate, and humorless.[143] Late in 1948 they (we do not know who, exactly, but Weil was likely involved) teased Kline and the AMS with an official application for membership in the Society in the name of Nicolas Bourbaki, himself, which Kline quietly suppressed.[144] A year later, they found a most apt form of revenge by applying again in Bourbaki's name under the reciprocity agreement that Kline negotiated in 1946 with the French Mathematical Society.[145] The application reached the AMS early in January, 1950, prompting in Kline a fit of pique. There is no clear indication of any involvement by Schwartz in the prank and only circumstantial evidence that its Bourbaki perpetrators intended it

[141] Kline to Lewy, 23 Jan 1951, Lewy Papers, carton 2, folder 16.
[142] For discussions of Schwartz's visa case in Bourbaki correspondence, see, e.g. Audin, *Henri Cartan et André Weil*, 259-310.
[143] Morse to Oppenheimer, 16 Nov 1949, IAS Member Files, box 85, "Leray, Jean" folder. On Bourbaki's sense of humor, see esp. Liliane Beaulieu, "Bourbaki's Art of Memory," *Osiris* 14 (1999): 219-251.
[144] AMS Records, box 34, folder 57. See also Everett Pitcher, *A History of the Second Fifty Years: American Mathematical Society, 1939-1988* (Providence: American Mathematical Society, 1988), 159-162.
[145] Reciprocity Agreement between the American Mathematical Society and the French Mathematical Society, AMS Records, box 37, folder 33; Application for Membership, 15 Dec 1949, AMS Records, box 36, folder 11.

explicitly as a rebuke over Schwartz's visa.[146] Amidst an interminable run of French-induced frustration, however, Kline quickly assumed the worst, supposing that the Bourbaki application was yet another attempt to bait the Americans over the visa situation. "It does not seem to me," he huffed, "that the American Mathematical Society should lower the dignity of its membership merely to bribe a French group not to boycott our Congress."[147]

As American lawyers and officials pressed Schwartz's case at the start of 1950, the Federal Bureau of Investigation received a confidential tip that "efforts are being made to assist the above-named individual [Laurent Schwartz] to enter the United States to attend a Congress of Mathematicians."[148] The source alerted the FBI to Schwartz's Trotskyite candidacy for the French Chamber of Deputies and "also indicated that a number of unidentified individuals were in favor of cancelling this Congress in the event the 'Iron Curtain representatives are not permitted to enter the United States to attend this Congress.'"[149] This was the first the FBI heard of Schwartz, but they followed the tip by locating two corroborating confidential foreign sources, likely in France. One identified Schwartz as "a Trotsky Party member since 1936." The other "reported on October 28, 1947, that one Laurent Schwartz of French nationality, Professor of Mathematics, had joined the 4th International in 1936," adding that "He was also described as an ex-member of the Ecole Normale Superieur [sic]" and "had been wanted by Vichy Police and the Gestapo for Trotskyist activities in 1942-43."[150]

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[146] The strongest circumstantial evidence I have found of a general link between annoyance at the Congress secretariat and the Bourbaki application is Weil to Cartan, 20 Feb 1950, Audin, *Henri Cartan et André Weil,* 296-297.
[147] Kline to Hille, 6 Mar 1950, AMS Records, box 36, folder 11.
[148] Hoover to Neal, 26 Jan 1950, FBI Schwartz File.
[149] Ibid. The reference to cancelation is an indication of the reach of Stone's, Weil's, and others' exhortations, noted above.
[150] Ibid.

If the denunciations that reached the FBI were meant to be alarming, they did not succeed. Rather, the Harvard hosts' political work appears to have made the difference. During the first part of April, mathematicians with an interest in Schwartz's case circulated copies of a 28 March, 1950, letter from the United States Department of Justice, Immigration and Naturalization Service, to Massachusetts Representative Christian Herter, a future Secretary of State who earlier helped craft an initial template for the Marshall Plan.[151] The letter indicated that "On March 27, 1950, an order was entered in the above case directing that if Dr. Schwartz applies for admission to the United States he be admitted under the 9th Proviso to Section 3 of the Immigration Act of 1917 for a temporary visit not to exceed 30 days for the sole purpose of attending the International Congress of Mathematicians," and that this information had been shared with the Department of State "for the information of the appropriate American Consul in France" and to the INS's "New York office inasmuch as it appears likely that Dr. Schwartz will apply for admission to the United States at the port of New York."[152] The FBI made a note of the decision, attributing it to the Attorney General, and requested that the Embassy in Paris notify the Department of State of Schwartz's arrival details.[153] Morse shared with his IAS colleagues in late April that "The latest information is to the effect that the Amer. Consul at Paris has given Schwartz an ordinary visa."[154]

[151] "Biographies of the Secretaries of State: Christian Archibald Herter (1895–1966)," United States Department of State, Office of the Historian, https://history.state.gov/departmenthistory/people/herter-christian-archibald (accessed 2016).
[152] Miller to Herter, 28 Mar 1950, copy (with a cover note from Schwarts to Oppenheimer, 13 Apr 1950) in IAS Member Files, box 125, "Schwartz, Laurent" folder.
[153] Nicholson to Hoover, 4 Apr 1950, FBI Schwartz File.
[154] Morse to von Neumann, Weyl, Veblen, and Oppenheimer, 26 Apr 1950, IAS Member Files, box 125, "Schwartz, Laurent" folder.

The background and timing for Jacques Hadamard's own visa struggles is unclear, though as what Jean Delsarte described to a Rockefeller Foundation official as "a fairly ardent communist" his troubles could also have been predicted.[155] In any event, by the start of July he had not received a visa, and some mathematicians in both France and the United States continued to agitate as they had for Schwartz while eagerly trading news concerning both of their embattled French eminences.[156] In the prevailing atmosphere of mistrust, Weil began circulating a rumor that "Prof. Estermann, from London" arrived in New York and "was asked, not once, but *twice*" if the International Congress "was a political organisation" and "*whether he knew Laurent Schwartz,* 'who was going to make a speech there,'" implying that New York immigration authorities might yet thwart Schwartz's visit.[157] Near the end of July, Hadamard received a negative decision from the Department of Justice, prompting Kline (who was in Washington at the time) to walk to the State Department to seek a reversal. Late on 26 June, Kline sent a telegram to Paris announcing that Hadamard's visa had been granted, and in early August Hadamard had his own visa in hand.[158]

Valiant Efforts

All the highly visible trouble over the French delegation proved a salve for the absence of any mathematicians from the Eastern Bloc in Cambridge, Massachusetts, that summer. In the

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[155] GRP diary June 23, 1950, in RF Projects: U.S., box 125, folder 1546 (also copied in RF Projects: France), underlining in original.
[156] E.g. Audin, *Henri Cartan et André Weil*, 298-302, 306-310, 313. Cartan and Weil, in addition to sharing information between themselves, track some of the information flow among their French and American contacts.
[157] Weil to Kline, 24 Jul 1950, Audin, *Henri Cartan et André Weil*, 603. See also Weil to Cartan, 24 Jul 1950; Cartan to Weil, 31 Jul 1950, Ibid., 305, 308-309.
[158] Weil to Cartan, 27 Jul 1950; Cartan to Weil, 31 Jul 1950; Cartan to Weil, 4 Aug 1950, Ibid., 306-309, 313.

presidential address with which he opened the Congress, Veblen acknowledged "that many of our most valued colleagues have been kept away by political obstacles and that it has taken valiant efforts by the Organizing Committee to make it possible for others to come."[159] The Soviets, in particular, were represented by a cablegram from Soviet Academy of Sciences President Sergey Vavilov, read in the opening session of the Congress, expressing the Soviet mathematicians' good wishes and offering the excuse that they were "very much occupied with their regular work" and so "unable to attend."[160]

The organizers' "valiant efforts" stood as proof of the exceptional American commitment to true internationalism, in spite of those (passively) "kept away" by others. Kline made this a theme in his reports to the Congress's sponsors. For the Rockefeller Foundation, he wrote "that the gathering could be truly international in the sense that mathematicians could be invited irrespective of national or geographic origins"—celebrating a non-exclusive internationalism that jettisoned the inclusive ideal the organizers had manifestly failed to achieve.[161] Kline did not dwell on the fact that so many mathematicians could not come, and perhaps had no hope of coming. That they could all be invited had in the end to suffice, for even though his valiant efforts made a difference to some visa cases, in the final analysis invitations were all he could really control. Here, in full force, was the dual character identified in this dissertation's Introduction of claims to be "truly international": *emphatically*, Kline's loudly trumpeted

[159] Oswald Veblen, "Opening Address of Professor Oswald Veblen," *1950 ICM Proceedings*, 124-125, on 124.
[160] Kline, "Secretary's Report," 122. On the genre and rhetoric that would have shaped this brief cable, see Alexei Kojevnikov, "President of Stalin's Academy: The Mask and Responsibility of Sergei Vavilov," *Isis* 87, no. 1 (1996): 18-50.
[161] Kline report enclosed in Kline to Weaver, 22 Feb 1951, RF Projects: U.S., box 125, folder 1546. Kline used nearly identical language already in a 19 Apr 1950 letter to Weaver (in the same folder), at which time the Congress's forthcoming gaps in participation were largely apparent.

internationalism let him stake significant moral, political, and financial claims; *ambiguously*, the implications and execution of his internationalism could shift in the face of the many insuperable barriers that stymied his ambitions.

Both Kline and Veblen lauded the Congress's scale. Kline called it "the largest gathering of persons ever assembled in the history of the world for the discussion of mathematical research" while Veblen more modestly called it representative of "a very large part of the mathematical world" including "most of the currents of mathematical thought that are discernible in the world today."[162] Veblen, for his part, waxed ambivalent about meetings on so grand a scale. "Mathematics is terribly individual," he declared, and "the ideal communication is to a very few other individuals." But mathematicians were joined in their idiosyncratic individualism: "The more one is a mathematician the more one tends to be unfit or unwilling to play a part in normal social groups." Mathematicians were united by "a universal human interest" that "cuts across all sorts of political, racial, and social differences," and "will be an influence for conciliation and peace."[163] If mathematicians were unable to reach across a divided world in practice, at least their commitment to non-exclusion could join the world's mathematicians in principle and in spirit. This was the same principle that let Kline, just three months earlier, decline to complete a questionnaire from the University of Chicago's Committee on Race Relations on the grounds that his was "a purely scientific society" open to all members regardless of race, creed, or national origin.[164] Just as the IMU's founders were organizing for

[162] Kline report enclosed in Kline to Weaver, 22 Feb 1951, RF Projects: U.S., box 125, folder 1546. Veblen, "Opening Address," 124.
[163] Veblen, "Opening Address," 125.
[164] Kline to Wirth, 27 May 1950, AMS Records, box 37, folder 3. As little as one year later, AMS officers labored to suppress some members' outrage over the lack of accommodations for Black mathematicians at its conferences in the South. See e.g. box 37, folder 18.

political unity with tangible infrastructural consequences, the ICM's organizers found in mathematics a paragon of universal peace through non-political indifference to the normal order of society.

That universalism was, of course, a patent fiction. The 1950 International Congress of Mathematics decisively shaped the discipline's international stature, both in the personal and intellectual connections it created and reshaped and in the institutional arrangements (foremost the International Mathematical Union) forged around it. It represented mathematicians' first postwar effort to grapple with the entirety of their discipline: its theories, people, institutions, nations, politics, and practicalities. Its universalism, such as it was, was necessarily that of non-exclusion. For those same practicalities, politics, nations, institutions, people, and even theories made universal inclusion impossible.

International mathematics depended integrally on a narrow and exclusionary professional system built on often-invisible academic infrastructures, which let a small cohort of well-connected mathematicians claim to speak for a single worldwide discipline. Their technologies of communication and reconnaissance—in forms like letters, telegrams, reports, or questionnaires—let them define and assert their prerogatives at great distances. These same institutional forms and media depended on the often-invisible labor of mid-century academia and the often-taken-for-granted infrastructures of postwar governments and economies. If the discipline's elite premised their internationalism on the possibility of effortlessly bridging borders and societies, such a premise was only even thinkable because of institutional settings that made communication and consensus seem effective and automatic.

Though mathematicians' demographics broadened markedly after the war, they did so in a decidedly elitist, hegemonic context. Mathematicians, in 1950, were overwhelmingly male, privileged, and racially and geographically homogeneous—all the more so in the discipline's institutional apices. This state of affairs was, in one sense, an accident of timing and circumstance. In another sense, however, it was the most concrete realization to date of the Second World War's realignment of the resources, personnel, and geopolitics of mathematics.

So it is important to stress that the international order that emerged in 1950 was not the one its projectors intended. Getting internationalism right, or "true," meant aggressively pursuing national politics and prerogatives that fell short as often as they succeeded. It meant emphatically defending grand ideals while maneuvering opportunistically through their manifold ambiguities. As the ideological terrain shifted beneath them, from postwar worries extrapolated from interwar politics to new irruptions of American anticommunism and the emerging competition of the postwar superpowers, internationally-minded mathematicians found that the truth and implications of their internationalisms were imperfect guides to the messy politics of a putatively non-political discipline in a divided world. From its inception, postwar mathematical internationalism was a compromise born of many forces—an unruly guide for a discipline whose leaders' ideals routinely outran their practices.

Chapter 5: Integration by Parts

Considerable Excitement

The first order of business, after Oswald Veblen welcomed the mathematicians of the Western and non-aligned world to Harvard for the 1950 International Congress of Mathematicians, was the presentation of two Fields Medals. The Fields Medal committee's chair, Harald Bohr, offered a brief background to the award, which was being given for only the second time and at fourteen years' remove from its debut at the 1936 International Congress in Oslo. Neither recipient, in fact, had heard of the award before learning he had won it.[1] By lauding the recipients in front of more than two thousand mathematicians and their guests at the opening ceremony, Bohr set out the first of several self-realizing declarations that day, that the Fields Medal marked a pinnacle of the profession, reserved for those who "not only are most promising as to their future work but […] have already written their names in the history of mathematics of our century."[2]

Bohr presented the first gold medal to Norwegian mathematician Atle Selberg, a recent arrival at the Institute for Advanced Study in Princeton, New Jersey, whom Bohr lauded for his work "of a sensational character" in the theory of prime numbers.[3] It would have been clear to those assembled, however, that the balance of Bohr's enthusiasm lay with the second medalist, Laurent Schwartz of France, and his theory of distributions. American Mathematical Society librarian and Swarthmore professor Arnold Dresden, for one, singled Schwartz out as the

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[1] Laurent Schwartz, *A Mathematician Grappling with His Century*, trans. Leila Schneps (Basel: Birkhäuser, 2001) [*Un mathématicien aux prises avec le siècle* (Paris: Odile Jacob, 1997)], 311 [319]; Atle Selberg, 11 Jun 1989 and 15 Jun 1989, IAS Oral History, box 2.

[2] Harald Bohr, "Address of Professor Harald Bohr," *1950 ICM Proceedings*, 127-134, on 127.

[3] Ibid., 129.

highlight of the entire Congress in his brief report to Warren Weaver at the event's conclusion.[4] So clear were Bohr's priorities that AMS and ICM Secretary J.R. Kline reversed Bohr's order of presentation in his summary report, listing Schwartz first.[5] In a presentation double the length he had devoted to Selberg, Bohr attested to his "considerable amount of pleasant excitement, on seeing the wonderful harmony of the whole structure of the calculus to which the theory [of distributions] leads and on understanding how essential an advance its application may mean to many parts of higher analysis."[6] Another self-realizing claim, as we shall see, for distributions' implications for calculus and higher analysis were far from certain in August of 1950. For Bohr, however, Schwartz and his theory appeared to promise pleasant excitement and harmony not just for the calculus but for the postwar discipline of mathematics.

"No wonder," Bohr exclaimed, "that the work of Schwartz has met with very great interest in mathematical circles throughout the world, and that a number of younger mathematicians have taken up investigations in the wide field he has opened for new researches."[7] A third self-realizing claim: If Schwartz was somewhat well known before August 30, 1950, it was only after the Congress's delegates returned home that interest in his theory could really be said to extend throughout the world. A fourth such claim: If some young researchers had by 1950 begun to work with Schwartz's theory, it was on the heels of Bohr's evangelism that mathematicians made the theory a mainstay of their pedagogy and research. A fifth claim: those growing communities of researchers did not encounter Schwartz's theory

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[4] Excerpt from Warren Weaver Officer Diary, September 7, 1950, RF Projects: U.S., box 125, folder 1546.
[5] J. R. Kline, "Secretary's Report," *1950 ICM Proceedings,* 121-145, on 126.
[6] Bohr, "Address," 133.
[7] Ibid.

ready-formed in a wide field, but opened up the theory and its conceptual possibilities through their research.

For a young mathematician or his newborn theory to reach across the world so rapidly and to such acclaim would have been unthinkable before or during the Second World War. Both the speed and the geographic range with which the theory of distributions crossed the globe were evidence of swift and striking changes in the discipline of mathematics. These changes arose out of new personal and institutional relationships forged before and during the war, as well as new material circumstances for the production and exchange of mathematical texts. After the war, mathematicians and their sponsors engaged in a wide range of debates and negotiations that reshaped the discipline's institutions, practices, problems and pedagogy.[8] Mathematicians not only pursued theoretical research in more and further-flung places, but did so in ways that depended on new and refashioned connections between the discipline's global centers and peripheries.[9]

This infrastructural change in mathematics accompanied what Leo Corry, Amy Dahan Dalmedico, and others have described as a conflict over the image of mathematics as a body of knowledge—whether it was unified and structured, adaptable and applied, organic or artificial.[10]

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[8] 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.

[9] Karen H. Parshall, "Marshall Stone and the Internationalization of the American Mathematical Research Community," *Bulletin of the American Mathematical Society* 46, no. 3 (2009): 459-482; Olli Lehto, *Mathematics Without Borders: A History of the International Mathematical Union* (New York: Springer, 1998).

[10] Leo Corry, *Modern Algebra and the Rise of Mathematical Structures*, 2nd edn. (Basel: Birkhäuser, 2004 [1996]); Amy Dahan Dalmedico, "An image conflict in mathematics after 1945," in U. Bottazzini and A. Dahan Dalmedico, eds., *Changing images in mathematics: from the French Revolution to the new millennium* (London: Routledge, 2001), 223-253; 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. 2 (1997): 297-

Where Corry and Dahan emphasize how changing intellectual and institutional contexts transformed what mathematicians thought they were doing, the early history of the theory of distributions demands a converse emphasis on how such contexts altered how mathematicians produced, understood, and shared individual mathematical theories themselves.[11] For the theory that spread so far and wide was not always and everywhere the same. Mathematicians brought distributions to new audiences through successive translations, adaptations, annexations, and reinterpretations. These vectors, in turn, depended on the highly variegated social, material, financial, institutional, linguistic, political, pedagogical, and ideological circumstances of postwar mathematics. If Schwartz's theory appeared, even in 1950, as just one thing, it owed to mathematicians' willingness and ability to see unity in the great diversity of texts and contexts, of knowledges and practices, that came to be associated under its banner.

In accounts of the history of the theory of distributions surveyed near the start of chapter 2, its formalisms, concepts, and interpretations traveled mostly without complication.[12] Where differences arose, they owed principally to choices derived from mathematicians' training,

342; Christopher J. Phillips, "In Accordance with a 'More Majestic Order': The New Math and the Nature of Mathematics at Midcentury," *Isis* 105, no. 3 (2014): 540-563; Alma Steingart, *Conditional Inequalities: American Pure and Applied Mathematics, 1940-1975* (PhD Dissertation, Massachusetts Institute of Technology, 2013).

[11] Cf. Alma Steingart, "A group theory of group theory," *Social Studies of Science* 42, no. 2 (2012): 185-213.

[12] See esp. Jesper Lützen, *The Prehistory of the Theory of Distributions* (New York: Springer, 1982); John Synowiec, "Distributions: The Evolution of a Mathematical Theory," *Historia Mathematica* 10 (1983): 149-183; Schwartz, *Mathematician Grappling*, ch. 6; Jean-Michel Kantor, "Mathematics East and West, Theory and Practice: The Example of Distributions," *The Mathematical Intelligencer* 26, no. 1 (2004): 39-46; Klaus-Heinrich Peters, *Der Zusammenhang von Mathematik und Physik am Beispiel der Geschichte der Distributionen: Eine historische Untersuchung über die Grundlagen der Physik im Grenzbereich zu Mathematik, Philosophie und Kunst* (Dissertation zur Erlangung des Doktorgrades des Fachbereichs Mathematik der Universität Hamburg, 2004); Anne-Sandrine Paumier, *Laurent Schwartz (1915-2002) et la vie collective des mathématiques* (PhD Dissertation, Université Pierre et Marie Curie, 2014).

intellectual milieux, or philosophical commitments. The theory's users achieved consensus or articulated disputes through interactions that did not, in these accounts, seem to depend much on the theory's languages, media, and formal features. But, as David Kaiser demonstrated with the history of Feynman diagrams in theoretical physics, medium-dependent representational practices both shape and reflect how new theories are communicated, debated, and understood.[13] Theoretical disciplines were far from immune to the political, economic, and ideological wages of World War II and the early Cold War.[14] The practical consequences of these latter circumstances, rather than the locally idiosyncratic difficulties of transmitting a particular kind of diagram or formalism, would decisively shape how distributions were shared and understood across continents.

Adapting Kaiser's approach, this chapter charts the heterogeneous international elaboration of the theory of distributions alongside the ideas and practices that gave it the appearance of unity. Bohr's address at the International Congress represented one of the most visible forms the theory took in its early years, but it was quickly joined in different contexts by expository works in a range of languages, formats, styles, and framings that drew together

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[13] David Kaiser, *Drawing theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005). Cf. Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003); Warwick here analyzes similar relationships between media, representations, and theories in a more fixed geographic and institutional context. See also Marwa S. Elshakry, "Knowledge in Motion: The Cultural Politics of Modern Science Translations in Arabic," *Isis* 99, no. 4 (2008): 701-730. For a broader historiographical framing, see James Secord, 'Knowledge in Transit,' *Isis* 95, no. 4 (2004): 654-672.

[14] E.g. Peter Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision," *Critical Inquiry* 21, no. 1 (1994): 228-266; 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 Kevles, "Cold war and hot physics: Science, security, and the American state, 1945-56," *Historical Studies in the Physical and Biological Sciences* 20, no. 2 (1990): 239-264.

audiences ranging from mathematical physicists in Canada and Argentina to function theorists in India, West Germany, and the Soviet Union. In several places, scholars' expository and pedagogical efforts helped to incorporate aspects of the theory of distributions into their own and their students' research. Mathematicians used new and newly accessible forms of international travel and communication to consolidate such disparate lines of research into a single flourishing theory, albeit one with alternative schools and versions. From textbooks and articles to classrooms and conferences, Schwartz and his acolytes and critics brought a new theory into the world with a speed and scale perhaps unprecedented for so new a mathematical theory, in part because of the changed circumstances of postwar mathematics but in part changing those circumstances along the way.[15]

A Synthesis and a Simplification

The 1950 International Congress of Mathematicians was not, of course, the first occasion where Bohr heralded Schwartz's theory on American shores. As chapter 2 documented, Bohr had been an enthusiastic international expositor of distributions since his first rapturous encounters with both Schwartz and his theory in the summer of 1947, and his evangelism included a 1948 mission by trans-Atlantic steamer to and beyond New York City. So it was with a convert's zeal and a tradesman's polish that Bohr reported to the Congress on Schwartz's "new and most

[15] The extent to which distributions were unprecedented depends, of course, on how one accounts for the rise of a new theory. Many such ways of accounting do not make for easy or comprehensive comparisons. However, one goal of this chapter is to make a case for the historically-specific means by which distributions traveled, in a way that situates the nature of the theory's spread in its particular historical moment, suggesting at least the unlikelihood of comparable past precedents. As for contemporary comparisons, my examination here of the role of the 1950 International Congress of Mathematicians (in which distributions figured with particular prominence among new theories) indicates the theory's distinctiveness among frameworks of similar vintage.

fruitful notions."[16] For many in his audience, this was the first glimpse of the theory of distributions. The theory took hold in this period in part because, for many, a single glimpse was enough.

Though others would place greater weight on the difficult function-theoretic foundations with which Schwartz justified his distributional calculus, few would have denied that his most striking claims could be stated and provisionally justified with remarkable speed and ease. Between 1945 and 1950, Schwartz had honed a presentation of his theory for broad audiences (by mathematicians' standards) that hinged precisely on such a claim to simplicity and perspicacity. Now, on the theory's biggest stage yet, Bohr elevated this supposed perspicacity from a resource for understanding into something more like a compulsion to propound. The theory was "so easy to explain," professed Bohr, that he "cannot resist the temptation, notwithstanding the general solemn nature of this opening meeting, to go into some detail."[17]

Trumpeting the novelty of Schwartz's theory, Bohr explained that it approached "the very foundation of the old calculus" with an original approach "intimately connected with the typical modern development of our science with its highly general and often very abstract character."[18] The problem with the old calculus was that it dealt with functions not well "adapted to the process of differentiation." Following the template Schwartz elucidated principally between 1945 and 1948, Bohr described the function, important to "the physicist and the

[16] Bohr, "Address," 130.
[17] Ibid., 131.
[18] Ibid., 130. On such stipulated relationships between generality and abstraction, see Renaud Chorlay, "Questions of Generality as Probes into Nineteenth-Century Mathematical Analysis," preprint of chapter (forthcoming as of this writing) in K. Chemla, R. Chorlay, and D. Rabouin, eds., *Handbook on Generality in Mathematics and the Sciences* (Oxford: Oxford University Press, 2016), from the author's website http://www.sphere.univ-paris-diderot.fr/spip.php?article185 (accessed 2016).

technicians," whose value was 1 when its variable was less than or equal to 0 and which jumped to the value 1 when the variable exceeded 0. That function's derivative was the ubiquitous "Dirac function" which, Bohr stressed, was no function at all. It had the value 0 at all but one point and an infinite value at that point. Moreover, the area enclosed by the zero-by-infinity rectangle at that point was stipulated to equal 1. By contrast, "legitimate" mathematical functions had only finite values at each point and could not enclose areas over regions with zero width.

"But this is of course not a legitimate way of speaking," continued Bohr, taking on Schwartz's Bourbaki-inflected insistence on signaling abuses of language, with the judgments of mathematical formulations they implied. Mathematicians, Bohr explained, could "naturally" account for this non-function with the theory of mass distributions or their associated linear functionals. But these could only go so far. Such rationalizations did not give a clear justification for the derivative, in turn, of the Dirac function, nor for the derivative of its derivative and any subsequent derivatives. Rather than dwell on these shortcomings, Bohr delved directly into Schwartz's proposed alternative. Here, a function would be replaced by something "essentially different" defined not by the values it took at different points "but by what we may call its effect when operating on an arbitrary auxiliary function."[19]

Bohr, like Schwartz, defined the "effect" of an ordinary function $f(x)$ on an auxiliary function $\phi(x)$ with the integral $f(\phi) = \int f(x)\phi(x)dx$. So for each auxiliary function, the ordinary function gave (with the integral) a numerical output that Bohr deemed its effect on the auxiliary function. Such an ordinary function was uniquely characterized by its effects on a certain collection of auxiliary functions. If the function $f(x)$ was differentiable and its derivative was denoted $f'(x)$, then a common calculus technique known as partial integration or integration

[19] Bohr, "Address," 131.

by parts guaranteed that $\int f'(x)\phi(x)dx = -\int f(x)\phi'(x)dx$, which Bohr restated more compactly in linear functional notation as $f'(\phi) = -f(\phi')$.[20] Such a small reformulation was all that Bohr needed. Mathematical analysts defined so-called functionals purely by the number (or effect) they associated to each auxiliary function, whether or not (as with ordinary functions) that number came from computing an integral. (Such associated numbers were only very rarely computed explicitly. The principle, rather than the practice, of associating a number to an auxiliary function was what mattered to Bohr and others.) Now, Bohr could present integration by parts no longer as just a calculation technique intrinsic to elementary calculus but rather as a transformation in the abstract calculus of functionals. Bohr explained that Schwartz's distributions were any functionals defined by their effects on this special group of auxiliary functions, and their derivatives were given by this compact formulation of integration by parts even if, strictly speaking, the distribution did not correspond to a differentiable function. Bohr thus presented distributions as a generalization of functions (as well as of mass-distributions like the so-called Dirac function): each function gave rise to a distribution, but there were many distributions that did not correspond to functions.

Bohr suggested that this generalization simplified a great variety of problems involving older notions while opening up new avenues of research regarding Schwartz's new notions, just

[20] It is not clear whether Bohr used visual aids for the formalisms in his presentation, but standard ways of narrating such formal expressions would have made them easy to grasp for most audience members, even those without a strong command of the English language, who might have jotted such expressions in notes from the presentation. I have reproduced the formalisms as closely as possible to their appearance in the Congress's proceedings, a form in which even more mathematicians than were at the initial presentation would have encountered Bohr's remarks. Even those who were present would have used this format of the address for reference after their publication. The particular formalisms of Bohr's talk were canonical and relatively common in advanced mathematical instruction, and so all the more accessible even if not displayed as a shared visual referent.

as the introduction of imaginary and complex numbers simplified many problems involving real numbers while leading to a new branch of "complex analysis" that rose and flourished in the nineteenth century.[21] As noted in chapter 2, Schwartz would adopt this analogy to great effect in his autobiography. According to Bohr, the most important feature of Schwartz's generalization was that each distribution, unlike each function, both could be differentiated to yield another distribution and was itself the derivative of some other distribution. This promised "the easy justification of different 'symbolic' operations often used in an illegitimate way by the technicians" and was "of such striking nature" that Bohr considered it "more than a utopian thought that elements of the theory of the Schwartz distributions may find their place even in the more elementary courses of the calculus in universities and technical schools."[22] Like Schwartz before him, Bohr had to assert the illegitimacy of widespread technical practices in order to validate the justification distributions were said to offer them. His story of sin and redemption gave distributions a catechistic character, one suited for curricular interventions as a lesson at once technical and moral.[23]

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[21] Bohr, "Address," 132-133. As noted in chapter 2, this was a highly idealized portrayal of the history of complex analysis that scarcely corresponded to the relatively controversial history documented by historians of mathematics.
[22] Bohr, "Address," 133. On calculus instruction, which was by then nearly universal in technical and scientific higher education, see See Luciana Zuccheri and Verena Zudini, "History of Teaching Calculus," in Alexander Karp and Gert Schubring, eds., *Handbook on the History of Mathematics Education* (New York: Springer, 2014), 493-513.
[23] Bohr was not alone in combining the technical and moral in assessments of distributions. Cf. a 25 Nov 1958 memo on a 24 Nov meeting, where Schwartz's theory of distributions figured in part of a presentation that asserted the importance "tanto tecnica como moralmente" of Argentine regional scientific leadership, UNESCO 51/82/A031/TA: "Argentina—Latin American Centre for Mathematics—TA Project, Part I—up to 31/12/63." See Michael J. Barany "Fellow Travelers and Traveling Fellows: The intercontinental shaping of modern mathematics in mid-twentieth century Latin America," *Historical Studies in the Natural Sciences,* forthcoming.

Schwartz had been careful in his writings not to make too much of his theory's novelty, emphasizing instead its organic and conceptual relations to its various antecedents. Bohr, by contrast, had few such hesitations. He quoted Schwartz's claim in the recently-published first volume of the latter's two-part textbook on the theory that distributions were "une synthèse et une simplification." But if this were so, Bohr declared, Schwartz's theory was a synthesis and simplification in the grand sense that Descartes' analytic geometry drew together prior ideas. Others may have done in their own piecemeal ways much of what Schwartz could do with distributions, but, Bohr exhorted, "the main merit is justly due to the man who has clearly seen, and been able to shape, the new ideas in their purity and generality."[24] Selberg had made significant advances regarding well-established problems by ingeniously using known methods, but Schwartz had opened up a field fit to inspire the discipline's next generation.

Bohr's address was itself a synthesis and a simplification. He anointed Schwartz and his theory in the short space of a prize presentation with a suite of carefully chosen metaphors and comparisons, many taken directly from Schwartz's writings and lectures on the theory—including lectures in Copenhagen and Vancouver that Bohr directly or indirectly helped arrange. Some of Bohr's chosen comparisons, like his link between distributions and Descartes's analytic geometry, signaled the depths of Bohr's enthusiasm but did not appear in most other mathematicians' presentations of or commentaries about the theory. Later interpreters

[24] Bohr, "Address," 133. This would also be Lützen's ultimate assessment in his detailed comparison of Schwartz's work with that of his contemporaries and relevant predecessors. Lützen, *Prehistory*, 159-160.

wholeheartedly took on other metaphors from Bohr, placing them at the center of their expositions in a variety of settings.[25]

Two such metaphors, both present to varying degrees in Schwartz's prior presentations of the theory of distributions, were particularly important in Bohr's 1950 synopsis. The first centered on the Dirac function and its relation to the so-called Heaviside function (flat but jumping from 0 to 1 at the origin) from which it was derived. Physicists, electrical engineers, and others had developed rules of symbolic manipulation surrounding these two formalisms which, while immensely useful in solving problems and developing theories, were widely seen to lack a completely clear and convincing mathematical justification.[26] Mathematicians had supplied just such a justification with the theory of mass distributions and linear operators which, while not dramatically changing physicists' or engineers' symbolic practices, offered them a substantial legitimizing or "legalizing" authority. With these abstractions, mathematicians emphasized the measure-theoretic and, respectively, the algebraic and topological properties of functions while jettisoning those features, like point-values, that made such entities as the Dirac function mathematically untenable in the usual frameworks of functions and elementary calculus.

[25] E.g., Bernard Malgrange, "Laurent Schwartz et la théorie des distributions," in M. R. Herman and B. Teissier, eds., *Colloque en l'honneur de Laurent Schwartz,* Palaiseau, 30 May 1983, *Astérisque* 131, no. 1 (1985): 25-33, republished in Colette Anné, Jean-Pierre Bourguignon, and Claude Viterbo, eds. *Supplément au numéro 98 de la Gazette des mathématiciens*, 2003*,* 67-74, on 67-68.

[26] This lack of a formal mathematical justification proved important for Heaviside when he initially developed and presented his calculus, but it had ceased to be especially problematic except in matters of disciplinary boundary-marking long before Schwartz's intervention. See Bruce J. Hunt, "Rigorous Discipline: Oliver Heaviside Versus the Mathematicians," in Peter Dear, ed., *The Literary Structure of Scientific Argument: Historical Studies* (Philadelphia: University of Pennsylvania Press, 1991), 72-95; Jesper Lützen, "Heaviside's Operational Calculus and the Attempts to Rigorise it," *Archive for History of Exact Sciences* 21, no. 2 (1979): 161-200.

By providing a yet more abstract and comprehensive framework for such theoretical devices, Schwartz promised to legalize still more symbolic feats—both those of already-acknowledged utility and those yet to be devised. Claiming distributions' relevance for established symbolic methods, Schwartz and Bohr could suggest that future applications would be comparably useful and pervasive. Schwartz's distributions, Bohr contended, improved upon mass distributions just as the latter had improved upon functions. For each advantage mass distributions offered in comparison to functions—increased rigor, generality, abstraction, conceptual flexibility and suggestiveness—distributions could be seen to offer something similar in comparison to mass distributions. Here, Bohr portrayed distributions as a natural next step in a fruitful chain of generalizations. In so doing, he turned a claim about the past development of the theory of functions into an assertion about the theory's future with the advent of distributions.

The second metaphor, which signaled for many both the theory's profundity and its simplicity, came from the technique of integration by parts. In undergraduate calculus curricula, integration by parts was an almost magical method. Taking a function's derivative was nearly always easier than finding its integral, particularly since students generally had to master a variety of differentiation techniques before tackling integration in their courses. Integration by parts was a straightforward means of replacing difficult integration problems with easier ones by strategically differentiating parts of the expression one intended to integrate. Finding and correctly differentiating those parts of an expression could be a ready source of exercises for students, each of which resolved (when solved correctly) from a potentially perplexing concatenation of terms into a satisfying formal expression.[27] In the theory of partial differential

[27] On the central pedagogical role of calculation techniques that supply diverse and satisfying student exercises, independent of their technical utility, see Peggy Aldrich Kidwell, Amy

equations, the technique could also be used to avoid differentiating parts of an expression whose derivatives could not be assumed with mathematical validity to exist. This latter use supplied the key metaphor for Schwartz's proposal for differentiation, while the former (undergraduate) use lent the proposal an image of a method at once elementary and supremely useful.

Amidst the conceptual fragmentation of postwar mathematics—evident in a ballooning professional population, publishing infrastructure, and much else—mathematicians used metaphors like these to hold theories and communities together.[28] Though many found the theory of distributions easier to learn than other theories of comparable reach and novelty, to use distributions in one's research could still require weeks or months of special study. But to appreciate distributions required little more than experience with undergraduate calculus, an experience so ubiquitous among mathematicians as to be definitional.[29] Appreciation was the crucial gateway to other kinds of engagement. Future users of distributions began, of necessity, with little more than a bare apprehension of what the theory entailed and what they might do with it. In order to sustain their studies with the theory, meanwhile, they required a wider audience of those who might find their work valuable or interesting, and who could understand its significance without delving into its technical details.

There are many different ways to know a scientific or mathematical theory. Those with the requisite technical preparation can disagree or agree with certain tenets or offer their own interpretations of the theory's claims and implications, but the variety of ways of knowing does

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Ackerberg-Hastings, and David Lindsay Roberts, *Tools of American Mathematics Teaching, 1800-2000* (Baltimore: Johns Hopkins University Press and Washington, D.C.: Smithsonian Institution, 2008), ch. 8.

[28] On professional scale and fragmentation in postwar science, cf. David Kaiser, "Booms, Busts, and the World of Ideas: Enrollment Pressures and the Challenge of Specialization," *Osiris* 27 (2012): 276-302.

[29] See Zuccheri and Zudini, "Teaching Calculus."

not stop with interpretive variations among those with a common technical understanding. Sociologists of science, in their early laboratory studies and participant ethnographies, developed an important distinction between the kinds of competence a practitioner brings to scientific knowledge and the (lesser) competence a researcher needs to interact with the practitioner and to understand what she or he is doing.[30] To understand the how knowledge of the theory of distributions was differentially held and spread, particularly as the theory was known in 1950, requires further distinctions and divisions ranging from recognition to familiarity to connoisseurship to interactional competence to pedagogical competence to research expertise.

In his brief time at the podium, Bohr demonstrated that a meaningful appreciation of Schwartz's theory did not require anything approaching a technical competence with the theory. Bohr's irresistible details were, to be sure, closely tied to the proofs, structures, problems, and practices of those who had made distributions an object of sustained study, and for whom such details guided intuitions and shaped narratives and explanations. But the theory's advanced research community, such as it was, depended on a significantly larger audience of those interested in the theory at different levels and for different purposes, some of whom would become experts but most of whom would not. This large group of mathematicians newly familiar with distributions would supply, in the coming years, the theory's enthusiastic researchers along with their supportive colleagues, seminar audiences, and prize committees. Without proofs and

[30] See especially Harry Collins, "Researching spoonbending: concepts and practice in participatory fieldwork," in Colin Bell and Helen Roberts, eds., *Social Researching: Politics, Problems, Practice* (London: Routledge and Kegan Paul, 1984), 54-69; Harry Collins, "How Do You Know You've Alternated?" *Social Studies of Science* 34, no. 1 (2004): 103-106. On this phenomenon in contemporary mathematical research, see Michael J. Barany and Donald MacKenzie, "Chalk: Materials and Concepts in Mathematics Research," in Catelijne Coopmans, Michael E. Lynch, Janet Vertesi, and Steve Woolgar, eds., *Representation in Scientific Practice Revisited* (Cambridge: MIT Press, 2014), 107-129.

with very few formal statements, Bohr made Schwartz's theory available to its largest audience yet. It was metaphors and comparisons, not formal proofs and structures nor problems and practices, that constituted the theory for this group.

The research community for the theory of distributions, importantly, did not include Bohr himself. While Bohr worked in areas of analysis related to those to which distributions were pertinent before his death in January, 1951, he has not typically been cited as a contributor to the theory itself. Because he lacked an identifiable research result about distributions, historians and mathematicians have almost entirely neglected his crucial contribution to the theory's meteoric early adoption—arguably his last major intervention on the stage of international mathematics. However that same lack was perhaps what made his ultimate contribution possible. In a sense, Bohr sat at the bottom of what Donald MacKenzie has called the "certainty trough" of trust in technical systems—with enough expertise to grasp and articulate the theory's significance but not enough to have had to grapple with its intricate difficulties and ambiguities, many of which researchers had not yet articulated before his death.[31] At a slight remove from the theory's sometimes dense technicalities, he was an ideal ambassador.

A Definitive Textbook

Schwartz's two-volume textbook on the theory of distributions was impeccably timed. His publications on the topic to date were not particularly easy to access and did not become widely cited, but the first volume of his textbook, whose wide comparative reach was spurred in part by Bohr's advertisement at the opening of the 1950 Congress, became a fixture of mathematics libraries and an obligatory citation for those adapting or applying Schwartz's theory. The second

[31] See Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, Mass.: MIT Press, 1990), 370-372.

volume, often cited in tandem with the first and less often cited on its own, appeared the next year.[32] Together, they helped define Schwartz's theory and its associated research communities and pedagogies in a variety of ways.

Textbooks have long drawn the attention of historians of science and mathematics, in part for their uses in particular disciplinary contexts (especially pedagogical ones) and in part as indications (albeit neither neutral nor transparent ones) of the state of settled and commonly accepted knowledge in different times and places.[33] As a genre of scientific exposition, textbooks have varied considerably across times, places, and disciplines. Works like Schwartz's were simultaneously programmatic arguments, teaching tools, reference manuals, and collections of central proofs and results (independent of whether one intended to refer to them). In part, Schwartz's textbook was to give a complete and rigorous justification for a theory whose rudiments had hitherto been indicated only schematically. In part, it was intended as a resource for researchers outside of a classroom context, complete with numerous references to recent mathematical literature and a combined bibliography in the second volume.[34] In these respects,

[32] Laurent Schwartz, *Théorie des Distributions*, tome 1 (Paris: Hermann, 1950). The work was published as part of two series which were often cited alongside or in place of the publisher: most commonly, the Actualités Scientifiques et Industrielles (in which the textbook was volume number 1091), and less commonly, the Publications de l'Institut de Mathématique de l'Université de Strasbourg (no. IX). In prospective references to the textbook in writings that appeared before its completion, Schwartz tended to refer to the latter (Strasbourg) series. The second volume was published by Hermann in 1951, and was no. 1122 in the Actualités series and no. X in the Strasbourg series.

[33] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1996 [1962]); Kidwell, Ackerberg-Hastings, and Roberts, *American Mathematics Teaching*, ch. 1; David Kaiser, ed., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge: The MIT Press, 2005), esp. part 3.

[34] At least one reviewer found it odd that the first volume was released without a bibliography of its own, without which its in-text references could be hard to decipher. I.E. Segal, review of L. Schwartz *Théorie des distributions. Tome I.*, *Mathematical Reviews* MR0035918 (12, 31d), accessed on MathSciNet.

the textbook also functioned as a research monograph, and those two genres often blended in works by twentieth-century mathematicians. Some mathematicians used the textbook for their own study of the theory, and others used it as either a private aid or a student guide for teaching the theory to others. Unlike many mathematical textbooks, it did not include organized sets of exercises or many remarks of an explicitly didactic character.

Hermann, the Paris publisher of Schwartz's textbook, produced the books in a relatively inexpensive and portable format similar to what it used for the influential textbooks of Nicolas Bourbaki.[35] The volumes' chapters each began with a summary of its main definitions and claims, making them convenient references for teaching and for standardizing basic results and notations in research and exposition. The books themselves were written in French, but referred to a range of well-known works in English and German and to a broader literature that included works in Russian and Spanish and whose countries of origin spanned Western and Eastern Europe and the United States. The volumes' notation, following conventions of international research and review journals, and their numerous technical terms cognate to those of other languages made them accessible to expert readers who were not fluent in French. Though they received multiple editions in French, the volumes were never published in full cover-to-cover translation.

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[35] See Leo Corry, "Writing the ultimate mathematical textbook: Nicolas Bourbaki's *Élements de mathématique*," in Eleanor Robson and Jacqueline Stedall, eds., *The Oxford Handbook of the History of Mathematics* (Oxford: Oxford University Press, 2009), 565-588. Bourbaki's *Elements* were published initially in Hermann's Actualités Scientifiques et Industrielles series. On Bourbaki and Hermann, see Liliane Beaulieu, *Bourbaki: Une histoire du groupe de mathématiciens français et de ses travaux (1934-1944)* (PhD Dissertation, Université de Montréal, 1989), I: 138-140. The publisher had strong ties to elite French mathematicians dating to the press's origins in the early Third Republic. See the publisher's online survey of its history at http://www.editions-hermann.fr/content/9-hermann (accessed 2015).

Schwartz's first volume began, like his 1945 article first introducing distributions, with the Heaviside and Dirac functions.[36] After suggesting this motivation, Schwartz cited a series of mostly well-known authors who had proposed means of treating generalized or weak solutions of partial differential equations—that is, solutions that were not strictly functions or that otherwise confounded usual interpretations of differential equations. Schwartz presented distributions as a synthetic culmination of those prior approaches. The theory of distributions, here, played a "very elementary…foundational role" that unified a great variety of "ancestor or close parent" theories. Distributions' virtue was as much pedagogical and philosophical as technical or theoretical.[37] A series of references at the end of Schwartz's introduction suggested that others (principally Deny, Riss, and Garnir) had already "systematically used" the theory.[38]

The 1945 article continued to provide the template for the textbook after the latter's introduction. Where the article followed its introductory section with an exposition of mass distributions in a single variable, the textbook began its body chapters with mass distributions in several variables. These led to formal definitions of distributions (both in several variables), followed by presentations of differentiation and integration for the newly-defined formalism. Both pieces, with their very different degrees of detail, then covered distributions in the context of the theory of topological vector spaces. The article concluded with a description of distributions in Fourier analysis (a topic reserved for the second volume of the textbook), while the first volume of the textbook concluded with considerations of algebraic features of distributions (such as their multiplication) and their application to partial differential equations.

[36] Laurent Schwartz, "Généralisation de la notion de fonction, de dérivation, de transformation de Fourier et applications mathématiques et physiques," *Annales de l'université de Grenoble* 21 (1945): 57-74, on 57. Schwartz, *Distributions*, 5-6.
[37] Schwartz, *Distributions*, 6-9.
[38] Ibid., 11-12.

Reviewers of the textbooks tended to praise Schwartz's exposition but were less bullish than Bohr about the theory's implications for the field. Irving Segal, a colleague of Marshall Stone's at the University of Chicago, reviewed both volumes for the American Mathematical Society's flagship *Mathematical Reviews*.[39] The first was "generally clear, carefully organized, and detailed," he averred, while the second was "unusually clearly written and well organized." Summarizing the two together in his review of the second, Segal wrote that Schwartz's "more systematic and algebraic" approach did not seem to advance existing analytic methods by much. Rather, "it would seem to amount to a much greater improvement in form than in essential content." The best-known skeptical review of the two volumes appeared in 1952 in the *Bulletin of the American Mathematical Society*. It was written by Princeton University's Salomon Bochner, one of the principal figures whose work Schwartz purported to clarify and extend. For much of the review, Bochner alternated between describing claims from Schwartz's book and citing corresponding claims from his own work from the 1920s and 1930s. He dismissed other stipulations from Schwartz's textbook as "rather obvious," and concluded by suggesting that the work's "analytical and even conceptual" innovations were difficult to discern.[40]

In the German counterpart to *Mathematical Reviews*, the newly restored *Zentralblatt für Mathematik*, Gustav Doetsch of Freiburg offered extended summaries in German of both volumes of Schwartz's text, just as he had done for Schwartz's landmark 1947 paper on

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[39] *Mathematical Reviews* accessed via MathSciNet, MR0035918 (vol. 1) and MR0041345 (vol. 2). On Segal, see John C. Baez, Edwin F. Beschler, Leonard Gross, Bertram Kostant, Edward Nelson, Michèle Vergne, and Arthur S. Wightman, "Irving Ezra Segal (1918-1998)," *Notices of the American Mathematical Society* 46, no. 6 (1999): 659-668.

[40] Salomon Bochner, Review of Laurent Schwartz, *Théorie des distributions*, vols. 1 and 2. *Bulletin of the American Mathematical Society* 58, no. 1 (1952): 78-85.

distributions in the *Annales de l'Université de Grenoble* some years prior.[41] Like others who had followed Schwartz's postwar work as it emerged, Doetsch readily adopted many of Schwartz's general claims on his theory's behalf. Schwartz's works, according to Doetsch, "systematically placed analysis on a new foundation," and included "important applications" to the theory of differential equations, most powerfully demonstrated in Schwartz's treatment of the calculus of variations.[42] Doetsch's reviews of both volumes of the textbook stressed connections and analogies to existing theorems and methods.

At most sites of mathematical research in North America and Western Europe and at many points outside of these regions, either *Mathematical Reviews*, the *Zentralblatt*, or both provided mathematicians their primary view of recent work in their discipline.[43] Lengthy and favorable reviews in both journals assured that within two years of Bohr's address every researcher in Schwartz's immediate areas of study and many in areas farther afield could be assumed to have at least a basic awareness of the theory of distributions, and to be able to find appropriate resources for learning more. These review journals also accounted for the most important indirect effect of Schwartz's textbook. The journals' editors commissioned article and book reviews from authors who were experts in areas closely related to those treated in the text in question. As a result, reviewers of new works on the theory of functions and differentiation were often mathematicians familiar with and sympathetic to Schwartz's project for placing

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[41] *Zentralblatt* reviews from zbmath.org, Zbl 30.12601, 37.07301, 42.11405.

[42] "In diesem Buch baut der Verf. die von ihm in früheren Arbeiten ohne ausführliche Beweise dargestellte Theorie der Distributionen, die die Analysis auf eine neue Grundlage stellt, systematisch auf. … Von dieser Theorie werden wichtige Anwendungen auf Differentialgleichungen gemacht. … Das Kapitel schließt mit einer Anwendung auf die direkten Methoden Variationsrechnung, die die Kraft der neuen Theorie aufs beste demonstriert." From Zbl 37.07301.

[43] The Soviet review journal *Referativnyi Zhurnal, Matematika* played a similar role in the Eastern Bloc after 1953. Western review journals regularly reviewed Soviet literature, as well.

distributions at the foundation of the theory of differential equations. Indeed, thanks to his newfound notoriety in American circles Schwartz himself was often asked to review works on themes related to his theory.

As Anne-Sandrine Paumier has observed, such reviewers used the short evaluative format of abstracts in *Mathematical Reviews* to tie a large number of new works to Schwartz's theory, including in some circumstances where the author did not appear to intend the work as an intervention in the theory of distributions, or where the term distributions was not even mentioned in the work itself.[44] A similar phenomenon is evident in the *Zentralblatt*, with different key figures and a greater role for Doetsch. To make such connections, reviewers referred to Schwartz's results and terminology and, most importantly, cited Schwartz's textbook as necessary background or further reading for those interested in the articles or books under review. Reviewers thus sustained the appearance of a wide-reaching and coherent theory with many active researchers by using the circumscribed format of review journals to assimilate and synthesize a wide range of new works. Annual direct citations of Schwartz's volumes peaked in *Mathematical Reviews* at 15 (in 1955), and together they were invoked in 52 separate *Mathematical Reviews* entries between 1951 and 1958.[45]

On the one hand, then, Schwartz's textbook delivered what he had long promised: an ostensibly complete and perspicuous exposition of the full theoretical basis for his theory of distributions. In this respect, it served as a ready reference and a guarantor of the justification it promised for the theory of differential equations. On the other hand, and in many ways independent of its particular contents, the textbook provided an anchor with respect to which

[44] See Paumier, *Laurent Schwartz*, § 2.4.4.
[45] Citation figures are from MathSciNet.

theorists of distributions asserted the mutual coherence and relevance of their work and extended their theories' real and imagined reach. If Bohr's presentation and Schwartz's Fields Medal signaled and helped perpetuate Schwartz's growing prominence in the international community of mathematicians, the textbook and its appearance in review journals likewise signaled and nurtured his theory's growing place in the mathematical discipline's postwar theoretical landscape.

Expository Translations, West and East

While Schwartz's textbook served for most mathematicians (including those who did not work primarily in French) as the definitive reference on the foundations of the theory of distributions, a range of introductory texts in other languages elaborated variations of the theory for different audiences. These made the theory of distributions recognizable in a much broader range of settings, languages, and disciplinary idioms than would have been possible from Schwartz's writings alone. Bohr's address was, itself, such a translation. Another based primarily on Schwartz's pre-textbook writings was Marshall Stone's brief account of the theory in *The Mathematics Student*, a journal of the Indian Mathematical Society aimed at advanced students and teachers of mathematics.[46] This had its origins in a December 1949 address Stone delivered for a conference of the Indian Mathematical Society, the earliest documented lecture on the theory in Asia (of which I am aware). Stone was not, however, even the first in India to have encountered the theory, since Homi J. Bhabha of the Tata Institute for Fundamental Research in Bombay would have learned about it from Schwartz himself in Vancouver the summer before

[46] M. H. Stone, "The Algebraization of Harmonic Analysis," *The Mathematics Student* 17 (1949): 81-92, on 89-90. While dated with the lecture's year of 1949, the article appeared in 1950.

Stone's trip.[47] The bulk of Stone's article was on the work of Norbert Wiener and Hermann Weyl, which "led to the recognition and exploitation of algebraic aspects" of the "mathematical structure" of harmonic analysis. Schwartz's theory appeared as a coda—a "remarkable theory" of which "our theme demands explicit mention." Stone showed how to take derivatives of distributions and then explained how these justified "the type of purely algebraic calculation which the physicist and the engineer have always been eager to employ."

Among the earliest to draw on Bohr's framing of distributions was Marston Morse, who summarized the latest trends in mathematical analysis for the *Journal of the Franklin Institute* in 1951.[48] Though the Congress's proceedings were still far from publication, Morse likely had Bohr's presentation from the 1950 Congress in mind when composing his review, as his presentation hewed closely to Bohr's. Like Bohr, Morse prefaced his extended treatment of Schwartz's theory with a brief description of the work of his co-medalist Selberg, whose work "strictly within the orbit of classical mathematics is the exception rather than the rule today."[49] Selberg's "elementary proof of the prime number theorem" resolved an old question about an object of ancient mathematical interest (prime numbers) by finding new possibilities in old concepts and methods. He did not so much change the content of mathematics, Morse implied, as ingeniously rearrange it.

By contrast, Schwartz's "synthesis and simplification," claimed Morse, gave a theory relevant to engineers and physicists which was "at the same time simpler and in many respects

[47] Bhabha, who like Schwartz was variously considered a communist or fellow traveler, was a leading figure in India's atomic energy program. See Indira Chowdhury and Ananya Dasgupta, *A Masterful Spirit: Homi J. Bhabha, 1909-1966* (New Delhi: Penguin, 2010).
[48] Marston Morse, "Trends in Analysis," *Journal of the Franklin Institute* 251, no. 1 (Jan 1951): 33-43, on 35-38.
[49] Morse, "Trends," 35.

more complete than that mathematical world which it aims to replace." As such, it was "an excellent example of a natural growth from the body of classical mathematics."[50] Like Bohr's, Morse's brief summary of the theory moved from justifying the Dirac function to the definition of differentiation through integration by parts. In closing, Morse placed particular emphasis on a point which Schwartz had stressed early and often, that distributions "include the minimum number of new mathematical entities" to account for all ordinary functions and their derivatives.[51]

Schwartz's first transatlantic invitation to present his theory had taken him in 1949 to Vancouver, Canada, for the second meeting of the Canadian Mathematical Congress. There, he joined Bhabha, Paul Dirac (of the eponymous function), and Antoni Zygmund in presenting a series of lectures introducing researchers to their studies in mathematical physics and related areas of pure mathematics.[52] One of Schwartz's hosts at that meeting was Israel Halperin, an ardent leftist who had endured a lengthy imprisonment in Canada in 1946-1947 on suspicion of Soviet espionage, and with whom Schwartz would later collaborate to advocate for mathematicians' political liberties.[53] Starting from Schwartz's Vancouver lectures on

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[50] Morse, "Trends," 35, 38.

[51] Morse, "Trends," 37. This claim implicitly mirrored Bohr's comparison of distributions to the system of complex numbers, which represents the minimum number of new mathematical entities to include all of the real numbers in an algebraically closed field. I discuss Schwartz's claim that his extension was minimal in this sense in chapter 2.

[52] *Comptes Rendus du Deuxième Congrès Canadien de Mathématiques / Proceedings of the Second Canadian Mathematical Congress, Vancouver, 1949* (Toronto: University of Toronto Press, 1951), ix-xii. This meeting is discussed at the conclusion of chapter 2. Zygmund, along with Argentine expatriate Alberto Calderon, would play a significant role in incorporating distributions into mainstream research on partial differential equations starting in the 1950s.

[53] Following a 2013 Freedom of Information request to the FBI, I was notified in 2014 that the U.S. Government retains more than 950 pages of records concerning Halperin. They are still (as of summer 2016) in the process of declassification. A letter in the Stone Papers suggests those in

distributions, Halperin produced a short pamphlet on the theory aimed at mathematical physicists, which he circulated largely through informal channels in that community.[54] Halperin's pamphlet began with the Heaviside and Dirac functions and asserted that distributions gave "rigorous content and validity to the formulae of operational calculus" associated with those two figures and their functions.[55] Though Bochner, who had few kind words for Schwartz's textbook, grumbled that Halperin followed Schwartz in overemphasizing the unique redemptive qualities of distributions for the Dirac function, he nonetheless dubbed the pamphlet "readable and informative."[56] Like Morse, Halperin underscored the theory's economy as a complete and rigorous abstraction. Like Stone and Schwartz, he signaled its utility in justifying informal or un-rigorous but nevertheless useful approaches from mathematical physics for solving partial differential equations.

These North American presentations of distributions, strongly shaped by Bohr's evangelism, tended to take distributions' simplicity for granted.[57] This held whether readers praised the theory as simple and profound (as Bohr did) or dismissed it as merely simple (as did Bochner). European expositors, by contrast, tended to attribute a greater difficulty to the theory

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the U.S. Government with access to such classified files believed him guilty of espionage, though he was never convicted in Canada.

[54] Israel Halperin, *Introduction to the Theory of Distributions* (Toronto: University of Toronto Press, 1952). The pamphlet was in sufficient demand to merit a second printing in 1960. Georgii Shilov, discussed below, supervised the pamphlet's translation into Russian in 1954: Israel Halperin, *Vvedenie v teoriiû obobshchennykh funktsii*, trans. M. S. Agranovich, ed. G. E. Shilov (Moscow: Izdatel'stvo Inostrannoi Literatury, 1954).

[55] Halperin, *Introduction*, 1.

[56] Salomon Bochner, "Brief Mention: Introduction to the theory of distributions, by Israel Halperin," *Bulletin of the American Mathematical Society* 58, no. 6 (Nov 1952): 679-680.

[57] This interpretation was also the dominant one in South America, as discussed below, largely due to the prevalence of North-South exchanges and the dominance of French allies of Schwartz among prominent European mathematicians to visit the continent.

and offered a variety of alternative approaches to it.[58] Polish mathematician Jan Mikusiński set the basic terms of this interpretation in a series of articles in French from 1948 to 1950.[59] In the first, he recast Schwartz's 1945 paper in terms of "much more general abstract spaces than those of functions" by prioritizing the notion of weak convergence.[60] A sequence of functions was said to converge weakly to another function if their effects as operators converged to the effect of the latter function when applied to auxiliary functions.[61] Mikusiński asserted that distributions were best understood in the context of the operator theory he had been developing since the war, wherein they exemplified a more general phenomenon with wider potential applications than was evident from Schwartz's focus on mathematical physics and differential equations. The Polish mathematician's next two papers, on Heaviside's operational calculus, did not discuss distributions directly but introduced "derivatives in the operator sense" as part of an extended interpretation of the theory of partial differential equations.[62] Schwartz, reviewing the articles in English in *Mathematical Reviews*, explicitly compared this approach to his 1945 exposition, and

[58] Several of these interpretations are surveyed in Paumier, *Laurent Schwartz*, § 2.4.4.
[59] See Krystyna Skórnik, "Professor Jan Mikusiński—the 20th anniversary of his death," *Linear and nonlinear theory of generalized functions and its applications, Banach Center Publications* 88 (Warsaw: Institute of Mathematics, Polish Academy of Sciences, 2010), 17-29.
[60] Jan G. Mikusiński, "Sur la méthode de généralisation de M. Laurent Schwartz et sur la convergence faible," *Polska Akademia Nauk. Fundamenta Mathematicae* 35, no. 1 (1948): 235-239.
[61] There were many notions of convergence from functional analysis and operator theory, but weak convergence was typically contrasted to "point-wise convergence," where functions converged if their values converged at each point in the function's domain. Since distributions eschewed the point-values of functions altogether this latter notion would have been untenable.
[62] Jan G. Mikusiński, "Sur les fondements du calcul opératoire," *Polska Akademia Nauk. Instytut Matematyczny. Studia Mathematica* 11 (1950): 41-70, quote on 56. Jan G. Mikusiński, "Une nouvelle justification du calcul de Heaviside," *Atti della Accademia Nazionale dei Lincei. Memorie. Classe di Scienze Fisiche, Matematiche e Naturali. Sezione Ia. Matematica, Meccanica, Astronomia, Geodesia e Geofisica. Serie VIII* 2 (1950): 113-121.

so claimed Mikusiński's work for the theory of distributions.[63] Where Mikusiński framed his results in operator theory as bringing greater generality to Schwartz's narrow innovation but also holding independent interest, Schwartz used his platform as reviewer to place Mikusiński's work firmly within the theory of distributions, as a specialized alternative interpretation to Schwartz's own.

Mikusiński's 1948 article, with its emphasis on weakly converging sequences of generalized functions, became the centerpiece of George Temple's series of writings on the subject, which he characterized as an effort to domesticate distributions for an audience of physicists and engineers. Temple's first attempts to tame Schwartz's theory came in a pair of addresses he delivered in 1952 as President of the London Mathematical Society. The first, presented that March in French to the Italian Istituto Nazionale per le Applicazioni del Calcolo, was swiftly published and reprinted by his hosts in an Italian journal and the Istituto's in-house reprint series.[64] The second, and better known, was Temple's official Presidential Address to the Society that November, published the following year in the Society's journal.[65]

That Temple, a mathematician speaking to mathematicians, should describe his intended audience as physicists and engineers was not so contextually confused as it may appear. After a war in which mathematicians had tried with mixed success and much frustration to commune with their scientific counterparts as part of both military and civilian war efforts, even those

[63] MR0036949 (12,189d), MR0036950 (12,190a) accessed on MathSciNet. The former article was received by *Studia Mathematica* in 1949 and was given this date in Schwartz's review.
[64] George Temple, "La théorie de la convergence généralisée et des fonctions généralisées et leurs applications à la physique mathématique," *Consiglio Nazionale delle Ricerche, pubblicazioni dell'Istituto per le applicazioni del calcolo* 356 (1953): 1-12, quote on 1. Reprinted from *Rendiconti de Matematica e delle sue applicazioni, Serie V* 11, no. 1-2 (1952): 111-122.
[65] George Temple, "Theories and Applications of Generalized Functions," *Journal of the London Mathematical Society* 28, no. 2 (1953): 134-148.

mathematicians not so directly involved in the conduct of hostilities took part in a disciplinary conversation steeped in the challenges and promises of bridging divides with non-mathematicians. Professional societies on both sides of the Atlantic and both sides of the conflict were important organs for coordination between mathematicians and state war interests.[66] As the presiding officer of a leading such society, Temple inherited an institutional position whose remit was oriented as much away from academic mathematicians as toward them. Claiming to mathematicians that he was speaking to non-mathematicians let Temple signal the form and intent of his intervention, and lay claim to an important kind of justification within his professional mathematics community—that of fostering relevance for physicists.

For Temple, Schwartz's "invention" represented no less than "One of the great events in the contemporary history of mathematics," and "provided a simple and rigorous calculus which unifies in one system a very wide variety of special techniques" in the theories of Fourier analysis, partial differential equations, and the calculus of variations.[67] Temple offered what he called "an alternative and simplified exposition" of distributions, with an emphasis on the "immediate and striking advantage" of the existence of derivatives of all orders.[68] This permitted "the rehabilitation of the delta-functions" (such as Dirac's function, customarily denoted with a lowercase delta) and "regularizes" the "standard but unjustified technique" of using so-called elementary solutions to partial differential equations.[69] Throughout, like Halperin and Stone, Temple portrayed the theory of distributions as an exciting and unifying framework to underwrite methods already widely used in mathematical physics. Unlike the North Americans,

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[66] See chapter 1.
[67] Temple, "Theories and Applications," 134.
[68] Ibid., 134-135.
[69] Ibid., 142, 144.

Temple suggested that this profound framework needed a mediator less caught up in obscuring abstractions.

This latter perspective was especially pronounced in Temple's attempt, two years later, to present his approach to the theory's foundations in a self-contained article for the *Proceedings of the Royal Society of London*. Having recently taken up the prestigious Sedleian Professorship of Natural Philosophy at the University of Oxford, Temple positioned his contribution "so as to make the important discoveries of Schwartz available to the physicist and engineer."[70] As he explained, although the theory was "undoubtedly of great practical importance for the applied mathematicians,… unfortunately for them the theory is highly abstract."[71] The end result, as Temple depicted it, was both simple and liberating. Generalized functions evaded "the tiresome restrictions" associated with their ordinary cousins, and were "destined to play a considerable part in mathematical physics."[72]

As it did for Temple, Mikusiński's version of distributions informed a 1955 series of articles in English in the prominent Dutch journal *Indagationes Mathematicae* by Dutch mathematician Jacob Korevaar, who at the time had recently joined the faculty of the University of Wisconsin in Madison.[73] It also, unsurprisingly, appeared in Mikusiński's own extended

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[70] George Temple, "The Theory of Generalized Functions," *Proceedings of the Royal Society of London. Series A, Mathematical and Physical Sciences* 228, no. 1173 (1955): 175-190, on 175.
[71] Ibid., 175.
[72] Ibid., 190.
[73] Jacob Korevaar, "Distributions defined from the point of view of applied mathematics." *Koninklijke Nederlandse Akademie van Wetenschappen. Indagationes Mathematicae* 17 (1955): 368-378 (I. Fundamental Sequences), 379-389 (II. Derivatives and antiderivatives, Laplace transformation), 483-493 (III. Convergence. Convolution. Definite integral. Inverse Laplace transformation), 494-503 (IV. Multiplication and division. Substitution.), 663-674 (V. Integral of a product. Fourier series. Connection with Schwartz' theory). The first four were reviewed in French in *Mathematical Reviews* by Schwartz and the last in English by Israel Halperin, who praised the series as "interesting and readable" (*Mathematical Reviews,* MR0074491, access via

introduction to distributions, published in English by the Polish Academy of Sciences in 1957, which drew as well on nearly a decade of the author's prolific publications on the theory, principally in French in Polish journals, many of which Mikusiński produced as part of his 1954-58 seminar on distributions.[74] The volume on distributions joined Mikusiński's better-known textbook on the operational calculus, first published in Polish in 1953 and translated into Russian in 1956, German in 1957, and English in 1959.[75]

The wide availability of these texts made Mikusiński a particularly widely-cited authority in Central and Eastern Europe.[76] In the West, the close association between Mikusiński and Temple led many to downplay the novelty and ambition of the former's approach. Schwartz, for instance, lumped the two together in his autobiography under the heading of "Other distributions," dismissing both as having pursued simplifications of distributions for engineers that instead made the theory more difficult.[77] Where Mikusiński offered what he framed as a more general and mathematically potent formulation of what Schwartz produced in the theory of distributions,

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MathSciNet). Korevaar acknowledged support from the Office of Naval Research, making this one of several expositions of distributions directly funded by varying arms (especially military ones) of the United States government. Cf. NORN 222(60)/(NR 041-221), which funded Schwartz's lectures and the production of notes in Berkeley in 1961. I discuss civilian National Science Foundation support for work on distributions below.

[74] J. Mikusiński and R. Sikorski, "The elementary theory of distributions. I." *Polska Akademia Nauk. Instytut Matematyczny. Dissertationes Mathematicae. Rozprawy Matematyczne* 12 (1957), 54 pp. Mikusiński's work was regularly reviewed in *Mathematical Reviews* by Schwartz, Halperin, Korevaar, and other expositors of the theory from the first half of the 1950s. Mikusiński's seminars, which followed Mikusiński in this period from Wrocław to Warsaw, are discussed in Skórnik, "Professor Jan Mikusiński."

[75] Jan Mikusiński, *Rachunek operatorów*, Monografie Matamatyczne 30 (Warsaw: Polskie Towarzystwo Matematyczne, 1953); Russian translation by A.I. Plesner (Moscow: Izdat. Inostr. Lit., 1956); German translation by G. Tesch, (Berlin: Deutscher Verlag der Wissenschaften, 1957); English translation (New York: Pergamon Press, 1959).

[76] E.g. E. Stiefel, "Einführung in die Theorie der verallgemeinerten Funktionen (Distributionen) als mathematisches Werkzeug zur Behandlung linearer Regelungen," *Bull. Schweizerische Elektrotechn. Vereins* 15 (1957): 3-8 (see MR0100525).

[77] Schwartz, *Mathematician Grappling*, 242.

Temple found a recognizable way of grafting Schwartz's theory to a simplified calculus for engineers. Temple found Schwartz too abstract; Mikusiński found him insufficiently so, or at least not abstract in the best possible way. By drawing attention to one possible interpretation of Mikusiński's work in a highly visible and widely accessible manner, Temple gave Western Bloc mathematicians a considerably narrower view of weak convergence in relation to distributions than Mikusiński's closest interlocutors found in Eastern Europe.

The half-decade after the 1950 Congress saw a handful of other expositions in languages other than French or English that drew more directly on Schwartz's approach or those whose work he claimed as the theory's chief antecedents. West German mathematician Heinz König offered, in 1953, an adapted foundation for the theory of distributions that more strongly emphasized the algebraic features of Bochner's framework.[78] Schwartz's theory found voice in Romanian and Hungarian a few years later.[79] The most comprehensive and influential adaptation of the theory by and for mathematicians was that of Israel Gelfand and Georgii Shilov, whose monumental five-volume *Generalized Functions* first appeared in Russian in 1958 and was translated into German, French, and English within a decade.[80] These followed a long series of

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[78] Heinz König, "Neue Begründung der Theorie der 'Distributionen' von L. Schwartz," *Mathematische Nachrichten* 9 (1953): 129-148 (see MR0055425).

[79] Gheorge Marinescu, "Équations transposées dans la théorie des distributions," (Romanian, with Russian and French summaries), *Acad. Repub. Pop. Romîne. Bul. Şti. Secţ. Şti. Mat. Fiz.* 7 (1955): 65-74 (cited and reviewed as MR0071725); István Fenyő, "Elements of the theory of distributions," *A Magyar Tudományos Akadémia Matematikai és Fizikai Tudományok Osztályának Közleményei* 6 (1956): 231-248 (see MR0095404).

[80] E.g. I. M. Gelfand and G. E. Shilov, *Obobshchennye funktsii i deistviia nad nimi* (Moscow: Gosudarstv. Izdat. Fiz.-Mat. Lit., 1958); *Verallgemeinerte Funktionen (Distributionen)* (Berlin: Deutscher Verlag der Wissenschaften, 1960); *Les Distributions*, trans. G. Rideau (Paris: Dunod, 1962); *Generalized Functions*, trans. Eugene Saletan (New York: Academic Press, 1964). Note that the titles of the translations reflect Schwartz's francophone nomenclature, Temple's anglophone nomenclature, and a combination with Temple's term followed by Schwartz's in parentheses in German.

articles by the pair dating to 1953, mostly but not exclusively in Russian in Soviet journals, that explored and explained various aspects and reformulations of generalized functions in analysis and the theory of partial differential equations.[81] Gelfand and Shilov, in turn, built on a thriving Soviet school of functional analysis and differential equations.[82] In the 1930s, that tradition of Soviet analysis included Sergei Sobolev, who later fell away from international mathematics due, in part, to his military work and the restrictions on travel and publication that came with it, but whose earlier results on differential equations many later claimed to have furnished the most significant insights underlying Schwartz's own theory.[83]

Halperin's, Temple's, and others' domestications of the theory of distributions showed that physics-inclined mathematicians saw at least a theoretical virtue in distributions for mathematical physicists, although they remained vague about how distributions might change their theories at a technical or conceptual level. In this respect, these mathematicians reaching out to physicists participated in a kind of disciplinary relationship with a long and similarly ambiguity-laden lineage. For much of their history as ostensibly autonomous disciplines, mathematics and physics have enjoyed a certain productive promiscuity. Not all mathematicians

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[81] Citations from Mathematical Reviews: I.M. Gelfand and G.E. Shilov, "Fourier transforms of rapidly increasing functions and questions of uniqueness of the solution of Cauchy's problem," *Uspekhi Matem. Nauk*, N.S., 8, no. 6 (58) (1953): 3-54 (MR0061694); Gelfand and Shilov, "On a new method in uniqueness theorems for solution of Cauchy's problem for systems [of] linear partial differential equations," *Dokl. Akad. Nauk SSSR*, N.S., 102 (1955): 1065-1068 (MR0072339; here as elsewhere I have reproduced the title as translated in *Mathematical Reviews,* which in this instance omitted a preposition); Gelfand and Shilov, "Quelques applications de la théorie des fonctions généralisées," *J. Math. Pures Appl.* 9, 35 (1956): 383-413 (MR0082068); Shilov, "Generalized functions and their applications in analysis," *Uspekhi Mat. Nauk*, N.S., 11, no. 6 (72) (1956): 217-226 (MR0083111).

[82] See Loren Graham and Jean-Michel Kantor, *Naming Infinity: A True Story of Religious Mysticism and Mathematical Creativity* (Cambridge: Belknap, 2009).

[83] See Kantor, "East and West." I discuss Schwartz's self-positioning with respect to Sobolev in chapter 2.

concerned themselves with physical phenomena and not all physicists found a need to keep abreast of recent mathematical developments, but for a wide variety of intellectual and professional reasons there has long been a robust commerce between certain mathematical and physical theories and theorists.[84] So it was not unusual for a new mathematical theory, especially one framed from the start as an intervention in physics and engineering, to draw critical attention from mathematical physicists.

Several such mathematical physicists attempted in the early 1950s to develop this framing explicitly—trying, with mixed success, to rewrite the foundations of some physical theories in distributional terms, and to explore the consequences of doing so. Many undertook such efforts after first or second-hand encounters with Schwartz himself, including at the 1950 Congress. Others learned of the theory from institutionally proximate mathematicians or from other mathematical physicists, including via putatively discipline-bridging expository works like Halperin's and Temple's. With their efforts to carry out systematically what Schwartz and others

[84] While philosophical literature on this phenomenon has come from perspectives prioritizing both mathematics and physics, the bulk of the most trenchant historical literature on the professional and intellectual relations between mathematics and physics comes out of analyses centered on a parallel phenomenon, that of the articulation of relationships and differences between experimental and theoretical physics. See esp. Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein,* 2 vols. (Chicago: University of Chicago Press, 1986); Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997); Peter Galison, *Einstein's Clocks, Poincaré's Maps: Empires of Time* (New York: Norton, 2003); Kaiser, *Drawing Theories Apart*; Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press, 1984); Suman Seth, *Crafting the Quantum: Arnold Sommerfeld and the Practice of Theory, 1890-1926* (Cambridge: MIT Press, 2010). The relevant philosophical literature spans much of the modern discipline of the philosophy of science, especially at its peak of preoccupation with physics in the middle decades of the twentieth century. Notable touchstones include Eugene P. Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," *Communications on Pure and Applied Mathematics* 13 (1960): 1-14; Pierre Duhem, *La Théorie Physique: Son objet et sa structure* (Paris: Chevalier & Rivière, 1906).

suggested could be distributions' most significant promise, they created yet another body of literature on distributions in a variety of languages and for a different audience than Schwartz himself could reach directly. Their efforts to bring distributions to bear on physics required re-formalizing and sometimes reconceiving physical theories and methods, despite Schwartz's initial stipulations that distributions simply justified those theories and methods without changing them at a practical level.[85]

The most comprehensive and lasting formulation of distributions among physicists came from Lars Gårding and Arthur Wightman, whose collaboration on the foundations of quantum field theory dated to the early 1950s but whose most influential publications on the matter emerged only later.[86] By the mid-1950s, mathematical physicists in Europe and in North and South America had published a wide range of expositions in journals and conference volumes taking particular formalisms and phenomena and explaining how to represent them with distributions.[87] Mathematicians interested in distributions followed these developments actively,

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[85] Klaus-Heinrich Peters, "Mathematische und phänomenoligische Strenge: Distributionen in der Quantenmechanik und -feldtheorie," in Karl-Heinz Scholte and Martina Schneider, eds., *Mathematics meets physics: A contribution to their interaction in the 19th and the first half of the 20th century* (Frankfurt am Main: Verlag Harri Deutsch, 2011), 373-393. See also Peters, *Der Zusammenhang von Mathematik und Physik*.

[86] A.S. Wightman, "How It Was Learned that Quantized Fields Are Operator-Valued Distributions," *Fortschritte der Physik* 4 (1996): 143-178. See L. Gårding and A. Wightman, "Representations of the Anticommutation Relations," *Proceedings of the National Academy of Sciences* 40, no. 7 (1954): 617-621 and "Representations of the Commutation Relations," *Proceedings of the National Academy of Sciences* 40, no. 7 (1954): 622-626; both articles employ the theory of operators without framing it in Schwartz's terms.

[87] E.g. Werner Güttinger, "Quantum Field Theory in the Light of Distribution Analysis," *Physical Review* 89, no. 5 (1953): 1004-1019; J. Serpe, "Remarques sur l'application de la théorie des distributions à la théorie quantique des champs," *Physica* 20 (1954): 733-742; Alberto González Domínguez, "Distribuciones y funciones analíticas," in *Symposium sobre algunos problemas matemáticos que se están estudiando en Latino América, Punta del Este 19-21 Diciembre 1951* (Montevideo: Centro de Cooperacion Cientifica de la UNESCO para America Latina), 91-106; Alberto González Domínguez, "Sobre algunas integrales divergentes

assimilating them into the theory through commentaries and citations in *Mathematical Reviews,* among other means.[88] Where physics-oriented mathematicians presented distributions as a simple and complete justification for existing practices, these physicists drew attention to important ambiguities in the theory. Most significant, perhaps, was the lack of a clear and consistent formulation of what it meant to multiply two distributions together.[89] From his earliest presentations, Schwartz had elaborated a clear technical and conceptual apparatus for multiplying distributions by different kinds of ordinary functions. At the same time, however, he had asserted that a general theory of multiplication for arbitrary distributions would be impossible if it were to satisfy certain fundamental mathematical principles which mathematicians usually attributed to abstract multiplication. For some aspects of physical theory, however, multiplying functions (or their generalizations) could not be avoided, and so a range of authors elaborated conditions or interpretations under which mathematically consistent and physically meaningful multiplications might be assured.

Before 1950, Schwartz, Bohr, and a few others promoted the theory of distributions on the basis of a suggestive framework and a great number of promissory notes. For those promises

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de la electrodinámica cuántica," in *Segundo symposium sobre algunos problemas matemáticos que se están estudiando en Latino América, Julio, 1954* (Montevideo: Centro de Cooperacion Cientifica de la UNESCO para America Latina, 1954), 53-60 (MR0072748); Jan Rzewuski, "Quantization of a certain class of non-local field theories," *Acta Phys. Polon.*, 12 (1953): 100-122 (MR0069040); R. Sauer, "Anwendung der Distributionstheorie auf das Problem des Überschall-Tragflügels," in Henri Villat (ed.), *Mémoires sur la mécanique des fluides offerts à M. Dimitri P. Riabouchinsky par ses anciens élèves à l'occasion de son Jubilé scientifique* (Paris: Service de documentation et d'information technique de l'aéronautique, 1954), 289-308 (MR0063223); Fritz Penzlin, "Distributionentheoretische Behandlung von Anfangswertproblemen relativistischer Wellengleichungen," *Wissenschaftliche Zeitschrift der Friedrich-Schiller-Universität Jena* 5 (1955/1956): 137-149 (MR0082636).

[88] See Paumier, *Laurent Schwartz*, 93-105.

[89] See Paumier, *Laurent* Schwartz, 104-105, on Schwartz's reframing of interventions by König, Ishihara, and Bogoliubow regarding multiplication.

to become realities in research and teaching, a wide range of scholars in a variety of settings had to rewrite and reconceive the theory in their respective institutional and intellectual contexts. With this work of rewriting, in the form of lectures, articles, books, and reviews, numerous expositors and theorists established what appeared to be a broad and largely coherent body of theory with many applications and relevance to a great many people and problems in physics and mathematics. What their work obscured was the great extent to which their respective interpretations often diverged in emphasis, application, foundations, and practice. The apparent success of the theory of distributions in the written record of the early 1950s thus owes both to the wide-ranging visibility of certain forms of writing and to the simultaneous effects of other forms' obscurity, particularly at the level of technical details and minute conceptual reckoning. Schwartz's writing offered the tools and conditions for a far-reaching theory, but it was only the rewriting of others that put Schwartz's promises into practice.

Classrooms and Seminars

Central to Bohr's 1950 portrayal of distributions was his view that the theory was particularly perspicuous. As he, Schwartz, and others attempted to show in a range of venues, a mathematician could produce a comparatively brief introduction to a suitably tailored version of the theory of distributions for such diverse audiences as electrical engineers, quantum physicists, measure and operator theorists, or even simply those with a background in undergraduate calculus. Such an appearance of idiomatic pluralism helped distributions' advocates portray Schwartz's theory as especially foundational and widely applicable. As more and more mathematicians came to share this perception, a significant share found cause to undertake further efforts to adapt and teach the theory in different settings, which in turn reinforced the initial pluralistic image. In this respect, distributions' classroom expositors recapitulated a process very similar to those who wrote and circulated the published expositions just discussed. In mathematical seminars and classrooms, students and researchers undertook the sustained effort of translation and sense-making required to cast a wide variety of mathematical notions in distributional terms. This work, more than any purportedly inherent feature of the theory itself, made distributions widely and powerfully applicable to postwar mathematical research.

Most such work did not leave an especially accessible paper trail, if indeed it left one at all. The notes mathematicians made and the discussions they shared in seminar and classroom settings were often fleeting by design.[90] Schedules and announcements only occasionally survive in institutional or personal archives. Two kinds of more durable traces, however, permit accounts of some of what transpired. First, presenters in seminars and courses often kept private notes in varying levels of detail with the intent of using them on future occasions. Second, seminars and courses were often also the occasions for taking, editing, and ultimately reproducing and circulating lecture notes (some of which became textbooks) for broader audiences. The presenter himself often furnished such notes, but in other settings one or more of the younger students or researchers in the audience prepared at least an initial version of the notes for circulation.

Postwar Paris witnessed an efflorescence of research seminars devoted to framing the latest developments of international mathematics through the concepts and styles of the seminars' participants.[91] The organizers of the most prominent such seminars arranged for presentations to be edited and published, and through postal exchanges and personal travel mathematicians

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[90] See Barany and MacKenzie, "Chalk."
[91] See Anne-Sandrine Paumier, "Le séminaire de mathématiques: un lieu d'échanges défini par ses acteurs. Incursion dans la vie collective des mathématiques autour de Laurent Schwartz (1915-2002)," *Philosophia Scientiæ* 19, no. 2 (2015): 171-193.

circulated these typescripts to many of the day's leading centers of mathematical research. Schwartz participated actively in, among others, the widely-subscribed Bourbaki seminar, where observers lauded his oratorical prowess. Following that seminar's custom, he offered detailed accounts of exciting work from those abroad, but derived afresh through the theory of distributions or other favored frameworks.[92]

These elite research seminars complemented a full roster of lecture courses associated with the expanding Paris metropolitan university system, whose student population more than doubled in the decade following the war.[93] Here, again, Schwartz won acclaim for his "extraordinary capacity for pedagogical prestidigitation" in his memorable and well-attended lectures on mathematical physics, which introduced a large cohort of young mathematicians and physicists to his approach in the years after his 1953 move from Nancy to Paris.[94] In addition to the inexpensive volumes of lecture notes Schwartz prepared in tandem with these courses, his students and colleagues prepared and published their own companion volumes of notes on the basis of Schwartz's lectures and related materials.[95]

Where Schwartz himself was not present to expound on his theory, lecturers made more direct use of his 2-volume textbook. While Hans Rademacher offered some framings and

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[92] Paumier *Laurent Schwartz*, § 5.5.2.
[93] See Michael Seidman, *The Imaginary Revolution: Parisian Students and Workers in 1968* (New York: Berghahn Books, 2004), ch. 1.
[94] Quotation from Michel Demazure, "Quelques souvenirs de Laurent Schwartz," in Anné, Bourguignon, and Viterbo, eds., *Supplément*, 33-35, on 33. Schwartz's skills as a lecturer and expositor are consistently affirmed in the minutes of the École Polytechnique for discussions of his multiple unsuccessful candidacies for positions between 1947 and 1952. He was eventually appointed to a professorship in 1959.
[95] Laurent Schwartz, *Théorie élémentaire des distributions* (Paris, 1955; see MR0080707); Martineau and Trèves, *Éléments de la Théorie des Espaces Vectoriels Topologiques et des Distributions, fascicule I: Éléments de la Théorie des Distributions* (Paris: Centre de Documentation Universitaire, [1955]).

elaborations of his own when teaching the theory at the University of Pennsylvania, he expected his students to have access to both volumes and he announced his intention to "follow those books more or less" and to "call upon the participants of this course to fill in details."[96] His introductory lecture began with the notion of linear operators before giving the example of the Dirac function. Just as distributions allowed the Dirac function to have a derivative, he explained, "In this way a great number of difficulties are circumvented, in the theory of differential eq[uation]s, Fourier series, a[nd] s[o] o[n]."[97]

For his 1951-1952 seminar on distributions at the University of Chicago, by contrast, Marshall Stone drew more directly on his personal connections and access to unpublished or hard-to-find source material.[98] In the first part of the seminar, in October-November 1951, Stone lectured to some two dozen advanced students.[99] These lectures were based in part on Schwartz's published writings, but Stone's collection of materials for the course included notes on articles (including Mikusiński's), manuscripts, a letter on a specific question relating to the theory, and even an outline for Bourbaki collaborator Claude Chevalley's own course on distributions from Columbia University. "In classical analysis," Stone began his first lecture, "the operation of differentiation is pathological, primarily because it is discontinuous." Distributions, he explained, made differentiation continuous, thus simplifying "many problems of the differential calculus."

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[96] "Laurent Schwartz's Theory of Distributions," unpublished manuscript course notes, p. 4, Rademacher Papers, box 10, folder 2.
[97] Ibid., p. 3.
[98] Stone Papers, box 17, "Course on Distributions" folder. Quotations below are from course materials from this folder. Stone's manuscript partial translation of Schwartz's 1945 article on distributions is in box 38, folder 15.
[99] These included Mischa Cotlar, who would later help promote the theory in Latin America, and Jacob Feldman who went on to complete a dissertation on operator theory under Kaplansky and Segal in 1954.

In the first quarter of the course, Stone aimed to present a minimum of proofs and formalities so as "to spend more time on the applications for the benefit of that larger audience most likely to be interested in using the new theory." For the remainder of his first lecture, Stone outlined the theory of duality in normed vector spaces, which he deemed the theory's essential foundation. In the course's second term, he then returned to these foundational details, following Chevalley's outline. Finally, in April-May 1952, five advanced students took turns presenting a curated series of articles, theses, and unpublished manuscripts written in English, French, and German over the course of five two-hour sittings. Some of these materials were obtained through the university library, but others Stone personally solicited from their authors.[100]

In the United States, Stone, Rademacher, Chevalley, and others who taught the theory of distributions hewed closely to Schwartz's program. They structured their courses so as to prepare advanced undergraduates for further work in mathematics or mathematical physics. By introducing a theory at once eminently teachable and unquestionably on the discipline's cutting edge, these professors used the pedagogy of distributions as a capstone and transition. In Manchester, by mid-decade, James Lighthill cast "Temple's mitigation of Schwartz's theory" in a similar role for his curriculum in mathematical physics.[101] Lighthill and his course moved at the end of the decade to the University of Cambridge, where Lighthill would soon assume the prestigious Lucasian Professorship of Mathematics.[102] His textbook, based on the Manchester

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[100] E.g. Stone to Deny, 22 Feb 1952, Stone Papers, box 17, "Course on Distributions" folder.
[101] "Lectures in Applied Mathematics (Honours) Schedules 1955-56," 26, Lighthill Papers, folder C1.
[102] Lighthill succeeded Paul Dirac in the professorship, and was succeeded in turn by Stephen Hawking. The professorship was famously held by Isaac Newton and first occupied by Newton's teacher Isaac Barrow. See Kevin C. Knox and Richard Noakes, eds., *From Newton to Hawking: A History of Cambridge University's Lucasian Professors of Mathematics* (Cambridge:

course and published by Cambridge University Press, was in sufficient demand to merit annual printings for its first three years, and biannual printings for two further cycles.[103] Like Schwartz's popular course across the English Channel, Lighthill's was aimed squarely at mathematical physicists and applied mathematicians, and eschewed advanced prerequisites from the theory of functions to a greater extent than its Paris counterpart.

The Distributions Road Show

The same apparent features that made the theory of distributions an appealing subject for an advanced undergraduate course made it all the more promising as a topic of special lectures from visiting mathematicians. After 1950, with appetites whetted by expositions of its basic principles, young mathematicians evinced considerable demand for expert instruction in the theory's conceptual background and technical details. Thanks to its association with atomic physics (via Dirac and his associated formalisms) and its increasing place in the theory of partial differential equations, it had a ready appeal for a variety of audiences. At the same time, it did not take long for a knowledgeable instructor to teach the theory's fundamentals. For an able young mathematician, distributions may well have ranked among the few theories where it was possible at least to approach the kind of basic competency required to conduct largely independent research involving the theory over the span of months rather than years.

Even before 1950, Schwartz delivered lecture series on his theory in Copenhagen and Vancouver, while Bohr preached its gospel in New York. Schwartz's lecture notes from

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Cambridge University Press, 2007), although Lighthill receives just passing treatment in this volume.

[103] M. J. Lighthill, *Introduction to Fourier Analysis and Generalised Functions* (Cambridge: Cambridge University Press, 1958). Printing years taken from a 1978 reprint. Materials relating to the volume's preparation and printing do not appear in the catalogue for the press's archive.

Vancouver extended the reach of those presentations as far as Tübingen before Halperin's account based on the Vancouver lectures appeared.[104] After 1950, Schwartz traveled yet farther and wider to teach his theory to enthusiastic audiences in a variety of venues over periods ranging from days to months. The longer sojourns included Yugoslavia in 1951, Brazil in 1952, Mexico and Tunisia in 1953, India in 1955, Colombia and Argentina in 1956, India again in 1957, and Argentina again in 1958.[105] In India, Colombia, and Argentina, his hosts compiled, edited, and published student lecture notes.[106] Far from remaining on the periphery, these lecture notes circulated alongside those from Schwartz's metropolitan courses and could be found in several major mathematics libraries. For instance, the first to borrow the copy of Schwartz's lecture notes from Argentina in the library of Princeton's Institute for Advanced Study was George Temple, then visiting from Oxford.[107]

Schwartz's intercontinental itinerary was peculiar both in its extent and in the collection of institutions that hosted him. Most striking, perhaps, is the absence of the United States from his travels between the 1950 Congress and 1960, when he next returned—a highly anomalous hiatus for a mathematician of his stature from the home of many of the world's leading postwar centers of mathematical research. The reason was the fallout from the Schwartz Affair, discussed

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[104] Güttinger, "Quantum Field Theory," 1004.
[105] See Anné, Bourguignon, and Viterbo, eds., *Supplément*, 19.
[106] Laurent Schwartz, *Lectures on Complex Analytic Manifolds* (Bombay: Tata Institute of Fundamental Research, 1955); Laurent Schwartz, *Ecuaciones Diferenciales Parciales Elipticas* (Bogota: Universidad Nacional de Colombia, 1956); Laurent Schwartz, *Lectures on Mixed Problems in Partial Differential Equations and Representations of Semi-Groups* (Bombay: Tata Institute of Fundamental Research, 1958); Laurent Schwartz, *Matemática y Física Cuántica* (Buenos Aires: Universidad de Buenos Aires, 1958).
[107] Laurent Schwartz, *Matemática y Física Cuántica: Notas Tomadas en el Curso Dictado con el Auspicio de UNESCO durante los Meses de Julio a Octubre* (Buenos Aires: Facultad de Ciencias Exactas y Naturales, Departamento de Matemáticas, Universidad de Buenos Aires, 1958), library index card consulted at the Institute for Advanced Study, Princeton, NJ.

in chapter 4, which signaled to potential hosts that a visit from Schwartz would be a difficult and uncertain arrangement, and left a paper trail in the relevant visa offices that led to this impression's confirmation for those few that tried to host him anyway. His later opposition to the Vietnam War and involvement with Bertrand Russell's effort to draw international attention to alleged war crimes in Vietnam further frustrated Schwartz's aims to visit the United States, to an extent Schwartz likely did not realize—blaming his difficulties instead on U.S. authorities' unwillingness to look past his early Trotskyism.[108] Although Schwartz's formal work with Trotskyist groups in France did not last long, his internationalism, anticolonialism, and interest in the developing world would shape his travels throughout his career. His enthusiasm for butterfly collecting further directed him toward tropical locales.[109] However, the greatest draw of his particular collection of hosts may simply have been their ready-established local interest in his theory—combined, to be sure, with the availability of funds to permit such extended voyages.

In South America, Schwartz followed on the heels of André Weil and Jean Dieudonné, two Bourbaki collaborators who joined visitors from American universities in the early postwar years in introducing many of the latest European and American mathematical developments to the region.[110] Weil and Dieudonné may have been among the earliest visitors to South America—in their case, São Paulo—familiar with distributions, but they were likely not the only such mathematicians in the region around that time. As discussed in chapter 3, Marshall Stone was a regular visitor to South America, including in the summer of 1947 when he may well have

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[108] See chapter 4; FBI Schwartz File.
[109] Schwartz, *Mathematician Grappling*, 316 [324-325].
[110] See Leopoldo Nachbin, interview with Alzira Abreu, Sheila Kaplan, and Vera Rita da Costa, 1991, http://www.canalciencia.ibict.br/notaveis/livros/leopoldo_nachbin_59.html (accessed 2016); Rute da Cunha Pires, *A Presença de Nicolas Bourbaki na Universidade de São Paulo* (EdD Dissertation, Pontificia Universidade Católica de São Paulo, 2006).

been aware of theory. José Luis Massera, a young and promising mathematician from Uruguay, returned home in 1948 from a Rockefeller Foundation fellowship in the United States that included time at New York University that overlapped with Bohr's journey there.

Leading South American mathematicians were also well represented at the 1950 Congress. Institutions from Argentina, Brazil, Chile, Colombia, Cuba, Mexico, Panama, Peru, Uruguay, and Venezuela sent official delegations, and representatives from Argentina, Brazil, Cuba, and Uruguay also participated in the organizational conference for the revived International Mathematical Union held in conjunction with the Congress.[111] At least two such delegates, Alberto González Domínguez of Argentina and Leopoldo Nachbin of Brazil, were among several whom Julio Rey Pastor characterized in 1954 as the "enthusiasts of Lorenzo Schwartz's theory" who occupied key positions in the South American research community.[112] These also included the young Argentine mathematician Roque Scarfiello, who thanks to a French government fellowship already had the rare distinction of having worked with the theory under Schwartz's direct guidance in France.[113]

Along with fellow Bourbaki collaborator Charles Ehresmann, Schwartz served as a visiting expert on one of UNESCO's first Technical Assistance missions in the field of mathematics, to Rio de Janeiro in 1952. Working with officials from UNESCO's regional headquarters in Montevideo, Schwartz presented courses in his host city and gave a variety of

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[111] *1950 ICM Proceedings*; UNESCO NS/84 (Unesdoc 126553eb), 69.
[112] Julio Rey Pastor, "La matemática moderna en Latino América," in *Segundo Symposium sobre Algunos problemas matemáticos que se están estudiando en Latino America, Villavicencio-Mendoza 21-25 julio 1954* (Montevideo: Centro de Cooperacion Cientifica de la UNESCO para America Latina, 1954), 9-30, on 15-16.
[113] Norberto Fava, "Roque Scarfiello (1916-2008)," *Revista de la Unión Matemática Argentina* 50, no. 1 (2009), i-ii.

expository lectures elsewhere in Brazil and other parts of South America.[114] For his South American hosts who had been at the 1950 Congress, Bohr's presentation provided a ready script for arguing that a visit from the "world-famous young investigator," Schwartz, "will thus be highly beneficial for all who work in mathematics in our country."[115] In Rio de Janeiro, he was particularly impressed by Leopoldo Nachbin, who would build a distinguished career as a leading Brazilian mathematician on the back of the theory of distributions, and would later present his own courses and publish extended treatments of the theory.

Schwartz's UNESCO mission, at least in principle, involved not just teaching mathematics but guiding the formation of self-sustaining mathematical institutions. In his own mission report to UNESCO, Ehresmann argued that the theory of distributions had a role to play in this broader purpose as well.[116] Amidst rapid industrialization and several states' pursuit of nuclear technology, Ehresmann insisted on the crucial importance of fostering teaching and research involving the most modern mathematical subjects, with the theory of distributions the only one of his examples of particular specificity or recent advent.[117] Mathematicians like Ehresmann, having promiscuously promoted the theory of distributions among multiple audiences in the years surrounding 1950, could readily point to a broad community of putative users in fields considered most relevant by non-mathematician officials both within host governments and universities in the developing world and in organizations like UNESCO.

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[114] Mission report, 1952, Laurent Schwartz, Unesdoc 159434.
[115] Laguardia to the Dean of the Faculty of Engineering, 12 Jul 1952, in Martha Inchausti, ed., *Una Vida Dedicada a la Matemática: documentos del archivo Laguardia* (Montevideo: Archivo General, Universidad de la República, 2007), 58-59.
[116] Mission report, 1952, Charles Ehresmann, Unesdoc 159433.
[117] On Argentina's nuclear program, see Diego Hurtado de Mendoza, "Autonomy, even Regional Hegemony: Argentina and the 'Hard Way' toward Its First Research Reactor (1945-1958)," *Science in Context* 18, no. 2 (2005): 285-308.

Where Schwartz's Latin American itineraries relied on a combination of personal contacts and intergovernmental infrastructures like UNESCO's, his reception in India drew more directly on personal contacts with institutionally central mathematicians. Elite mathematicians in India had already encountered distributions by way of Stone's address to the 1949 Symposium on Harmonic Analysis of the Sixteenth Biennial Conference of the Indian Mathematical Society.[118] Having met Bhabha in Vancouver earlier in 1949, Schwartz went on to visit Bhabha's Tata Institute in Bombay in both 1955 and 1957, and on both occasions the institute compiled and published notes from his lectures.[119]

While Schwartz spent a great deal of time and effort lecturing on distributions abroad, the distributions road show also ran in the other direction, with mathematicians from the periphery taking extended trips to North American and European centers in order to study and sometimes to teach the theory of distributions, as well as to collaborate with metropolitan experts. Nachbin was Schwartz's most direct counterpart in the mesh of center-periphery relationships forged around the theory of distributions. Established even then as (in Schwartz's words) "by far the best mathematician of Brazil," Nachbin who would go on to teach the theory of distributions (among other subjects) on a continuing basis both in Brazil and at the University of Rochester in New York, in addition to a suite of shorter-term appointments at other institutions.[120] Nachbin's

[118] Stone, "The Algebraization of Harmonic Analysis."
[119] The Tata Institute of Fundamental Research has received relatively scant attention from historians of science in comparison with its importance as a node of elite theoretical science in the developing world, evident in archives of the period. On this lacuna, see Indira Chowdhury, "A Historian among Scientists: Reflections on Archiving the History of Science in Postcolonial India," *Isis* 104, no. 2 (2013): 371-380.
[120] Schwartz quote from Mission report, 1952, Laurent Schwartz, Unesdoc 159434. As evidence of Nachbin's international stature, see e.g. the 1963 effort to recruit him to Oxford discussed in Atiyah to Nachbin, 31 May 1963; Dieudonné to Nachbin, 26 Jun 1963; Schwartz to Nachbin, 10 Jul 1963; Nachbin Papers, LN.T.001, documents 7, 9, 10.

exceptional fluency in advanced mathematics was aided considerably by the two years he spent as a U.S. State Department and then Guggenheim Foundation fellow at the University of Chicago, starting in 1948 and ending with a trip to the 1950 International Congress of Mathematicians. In Chicago, Stone had assembled one of the world's leading departments of mathematics, a department that disproportionately included professors like Weil with significant Latin American experience. Between his stay in Chicago and the many (especially French) mathematicians with whom he crossed paths in Brazil, Nachbin was among the best connected Latin American mathematicians to his discipline's international community, such as it was at mid-century.

In July, 1955, Nachbin wrote to the Rockefeller Foundation's Harry Miller to follow up an inquiry about his chances for a fellowship with the express purpose of pursuing further studies under Schwartz in Paris. Nachbin had just returned from the Annual Meeting of the Brazilian Association for the Advancement of Sciences, where he was enlisted "to give a lecture on advancements of Mathematics in Brazil, a task which at the same time is difficult because people not mentioned are usually offended, and easy because there is little Mathematics going on around here."[121] As a founding member of the Instituto de Matemática Pura e Aplicada in Rio de Janeiro, Nachbin had spearheaded what many saw as the first Brazilian mathematics journal to have an international profile and in just the past year Nachbin had hosted distinguished visitors from the United States, France, and the leading institutions of his continent for courses and seminars.[122] But "build[ing] up a mathematical atmosphere" in Brazil was hard work, and Nachbin, feeling stifled, sought renewed stimulation abroad.

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[121] Nachbin to Miller, 13 Jul 1955, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.
[122] 1954 IMPA annual report, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.

Schwartz reacted warmly to Nachbin's proposal.[123] Miller, for his part, worried that the trip would take Nachbin away from his central place in Brazil's mathematical infrastructure, but the director of Nachbin's institute assured Miller that the first priority should be advancing research, which a fellowship would promote.[124] Nachbin added that a change of Brazilian presidential regime would mean a shakeup in the top academic institutions, so his current role should not be taken for granted.[125] He received his fellowship and exchange visitor's visa in the summer of 1956 and set off for short stop-overs at a UNESCO-sponsored symposium in Mexico on Algebraic Topology organized by Princeton's Solomon Lefschetz and at his old department in Chicago that August and September en route to Paris.[126]

It was only after Nachbin arrived in Chicago that he received word from Schwartz that the seminar was not to proceed as planned.[127] By 1956, as discussed above, a growing collection of researchers in France, Britain, and the United States had begun laying the ground work for a distribution-theoretic interpretation of quantum field theory. Schwartz resolved to change tack with his seminar that year and to join with physicist Maurice Levy for an intensive study of partial differential equations and distributions in relation to quantum field theory, an area for which Nachbin professed little interest, despite Schwartz's encouragement for him to join. Instead, Nachbin sought Miller's permission to change course and to stay in the United States until the subsequent edition of Schwartz's seminar, taking advantage in the meantime of the suite

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[123] Schwartz, 5 May 1956, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.
[124] Gama to Miller, 20 Mar 1956, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.
[125] Nachbin to Miller, 10 Feb 1956, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder. Juscelino Kubitschek had taken office at the end of January, 1956.
[126] Greenfieldt to U.S. Consul, 5 Jun 1956; fellowship summary sheet, 28 May 1956; Nachbin to Lefschetz, 18 Jul 1956, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.
[127] Nachbin to Miller, 18 Sep 1956, with Schwartz enclosures, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.

of experts on differential equations Chicago planned to host. Miller, for his part, was wary of each further day Nachbin might spend abroad. As far as he was concerned, Nachbin was urgently required for "the organization of advanced training of young mathematicians [in Brazil] who are certainly going to be needed in increasing numbers in connection not only with technological developments, but also in connection with the atomic energy experimental program and activities growing out of it."[128] For the Rockefeller Foundation, Nachbin was an indispensable instrument of nation building for a country whose rapid progress depended on highly trained mathematicians capable of working across the areas of applied mathematics, physics, and engineering.[129]

This rationale suited Nachbin well enough, so long as it afforded him the resources to pursue his studies abroad. He stressed to Miller that "Any pioneering work is very slow and tiresome" in Rio de Janeiro, and that without stimulating research he would lack the intellectual nourishment to teach, "feeling like in prison" instead.[130] A protracted exchange of pleas and deflections between Nachbin and Miller was resolved only when a Guggenheim fellowship freed Nachbin to extend his North American voyage without further Rockefeller sponsorship. Nachbin was more practiced than most at this sort of sponsor shopping. Fellowship opportunities for promising Latin American mathematicians were few and interconnected, but these connections

[128] Miller to Nachbin, 21 Nov 1956, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.
[129] This reasoning can be found across multiple fellowship files from the period. Cf. Miller to Fernandez, 23 May 1958, RF Fellowships: Argentina, box 72, "Ricabarra, Rodolfo Alfredo" folder: "Our regular fellowships, which are awarded to youngish scientists who have already clearly demonstrated their ability to do original investigative work, are for only a 12-month period, as the purpose of the fellowship is to give a qualified scientist the opportunity to get stimulation, ideas, and orientation which will serve him to a greater or lesser degree during the remainder of his scientific career, after he returns home." Similar ideas are also notable in the fellowship renewal documents for Nachbin's protégé Elon Lages-Lima (see RF Fellowships: Brazil, box 51, "Lima, Elon Lages" folder).
[130] Nachbin to Miller, 30 Jan 1957, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder.

made it easier for the most promising young mathematicians to piece together support from multiple sources, including occasional stipends or subventions from host institutions and conferences, in order to take them abroad for longer than any one sponsor might intend.

Nachbin had declined one such grant already obtained for the Mexico conference at the start of his travels upon receiving his Rockefeller fellowship, and the Institute for Advanced Study in Princeton offered to consider him for a supplemental grant for the end of his trip.[131] Though short-term visiting foreign mathematicians could not typically lead military and U.S. government projects, it was also routine for them to receive support from contracts held by host faculty or institutions. Before finishing his Guggenheim year at the Institute for Advanced Study, Nachbin worked under Irving Kaplansky's Office of Ordnance Research contract at Chicago.[132] Securing each grant took time and effort, however, and Nachbin's opportunities were far from limitless. It would take him another half decade to reach the Schwartz seminar in Paris, by then as a lecturing expert.

Unsurprisingly, despite Schwartz's extensive intercontinental outreach and the presence of enthusiastic acolytes abroad like Nachbin, the lion's share of independent research on central features of the theory of distributions remained concentrated in France, even as mathematicians much farther afield gained familiarity and practice with the theory. Among the most important early wholesale adopters of the theory were doctoral students who made distributions central parts of their dissertation research. Schwartz's own students in this period were numerous and many attained significant influence in the French and international mathematical communities.

[131] Nachbin to Lefschetz, 18 Jul 1956, RF Fellowships: Brazil, box 61, "Nachbin, Leopoldo" folder. Morse to Nachbin, 19 Aug 1957, Oppenheimer to Nachbin and Morse to Oppenheimer, 21 Jun 1957, IAS Member Files, box 100, "Nachbin, Leopoldo" folder.
[132] Nachbin to Oppenheimer, August 28, 1957, IAS Member Files, box 100, "Nachbin, Leopoldo" folder.

These included Alexandre Grothendieck, Bernard Malgrange, Jacques-Louis Lions, André Martineau, and François Trèves.[133] Through personal encounters, he won over other young students and researchers like Nachbin, and perhaps most famously Lars Hörmander of Sweden.[134]

His influence extended farther through dissertations closely advised by those few who knew Schwartz or his theory well. These included those of Thomas Henry Crowley, whose 1954 dissertation at The Ohio State University acknowledges the aid of Alberto Calderon, and of Leon Ehrenpreis, who studied at Columbia University under Chevalley.[135] This latter pairing is significant in being among the first not directly associated with Schwartz to seek and win sustained research funding explicitly dedicated to the study of distributions.

The final year of Ehrenpreis's dissertation research on the theory of distributions in locally compact spaces had been funded by a 1952-1953 grant from the National Science Foundation with Chevalley as Principal Investigator.[136] The NSF renewed its support for Ehrenpreis for 1954-1956, covering the entire two years of his salary with an additional allowance for incidental costs and institutional overhead, which supported Ehrenpreis's research at the Institute for Advanced Study in that period.[137] The presence of a researcher on a dedicated grant already made the Institute for Advanced Study among the leading centers of research on

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[133] See Paumier, *Laurent Schwartz*, 17. Elsewhere in her dissertation, Paumier explores the contributions of several of these French figures to the theory's reception.
[134] Lars Hörmander, "A Tribute to Laurent Schwartz," in Anné, Bourguignon, and Viterbo, ed.s, *Supplément*, 59-62.
[135] Thomas Henry Crowley, *A Problem in the Theory of Distributions* (PhD Dissertation, The Ohio State University, 1954); Leon Ehrenpreis, *Theory of Distributions in Locally Compact Spaces* (PhD Dissertation, Columbia University, 1953).
[136] NSF 5-G205. From 12 Mar 1954 cover letter for subsequent grant, IAS Mathematics Grants, box 2, "NSF 1954-1957" folder.
[137] Waterman to Oppenheimer, 22 Jul 1954, IAS Mathematics Grants, box 2, "NSF 1954-1957" folder.

distributions outside of France, but others who worked on the theory would join Ehrenpreis during his stay as well.

Ehrenpreis was not the only recipient of the NSF's largess at the Institute, which also benefited handsomely from contracts with a variety of military sponsors.[138] His was, however, among the more specific and focused projects at a time when the NSF funded such generic and non-committal proposals from established researchers as "Studies in Mathematics."[139] His proposal, which ran to a mere five typewritten pages, hewed closely to questions and frameworks from the theory of analytic functions and their topology. It is most remarkable for what it does not include: any indication that distributions might be an unknown or insignificant area for focused research, in need of any broader justification beyond a plausible set of theory-specific questions and approaches to their resolution.

By the mid-1950s, then, distributions had become a routine feature of the mathematical landscape. Doctoral students could write entire dissertations on distributions and young researchers could win funding for multi-year projects to study minute problems within the theory. Researchers found in the theory the motivation and the means for commanding institutional and material support to travel across and between continents. Lecturers taught the theory as part of advanced undergraduate curricula for prospective mathematicians and mathematical physicists. A flurry of research articles attested to a burgeoning interest in the theory of distributions not only as a justification for established methods but as a rich and vibrant subject of research in its own terms. Experts could debate its finer points—such as how to define multiplication within the

[138] These are documented throughout IAS Mathematics Grants.
[139] NSF-G1974, IAS Mathematics Grants, box 2, "NSF 1954-1957" folder.

theory—and propose variants or improvements on the theory's principles, definitions, or framings.

In less than a decade, distributions went from novel to promising to normal. This transformation was not the result of a narrow circle of experts recruiting converts and then gradually extending the theory and its audiences. Nor was it the result of a simple but profound observation growing through teaching and research into a sophisticated and self-sustaining body of theory. Rather, from the start, the theory of distributions harbored a range of justifications and implications for a diverse collection of audiences in a variety of institutional and geographic settings. Across the many institutional and intellectual spaces within and through which they worked, theorists of distributions built a theory that was integrated by parts.

It was neither possible nor necessary that mathematical physicists in Manchester meant the same thing by generalized functions as operator theorists in Warsaw, function theorists in Chicago, or mathematical analysts in Rio de Janeiro. Distributions varied, in this period, not just along a spectrum from suggestive metaphors to detailed programs of research. Scholars' different articulations of the theory reflected the different frameworks and communities of the interlinked national and regional communities in which it was elaborated, debated, and pursued. In Brazil, those like Ehresmann and Nachbin portrayed it as an ingredient in modernization—both of mathematics and of a nation. In England, Temple made it out to be a foreign theory whose utility and grace emerged only upon a suitable domestication. In France, the theory was strongly identified with its charismatic originator. In the United States, to which Schwartz could not travel with ease, the theory found a range of institutional homes and advocates.

Divergent interpretations of the theory let scholars in new places with varied interests find something in the theory of distributions worth pursuing, and those same scholars followed their lines of inquiry to very different ends, enrolling other people and ideas along the way. Communities of distribution theorists and their respective interpretations of the theory came together, to the extent that they did, through plentiful but circumscribed opportunities to travel and through the circulation of publications and reviews. But most of all, they came together through metaphors. Though the theory took many forms in many places in its first decade, its users could deploy common analogies and images to convince themselves that they were studying the same thing, and to effect meaningful exchanges amongst themselves. In the nascent intercontinental community of mathematicians, their conviction of common ground necessarily came before the long and hard work of reconciling diverse ideas and institutions.

Schwartz frequently made his theory out to be something whose full justification lay in a long-forthcoming textbook, but at the 1950 Congress Bohr conjured an image of the theory truer to its historical realization in its first decade. Declaring the theory to be “so easy to explain” that its promulgation was not just simple, not just somehow profane (“notwithstanding the general solemn[ity]” Bohr’s explanation disrupted) but palpably irresistible, on that epoch-defining August day he made the theory out to be little more than the partially integrated web of metaphors by which it traveled across the world.

Conclusion: Intercontinental Mathematics

In Harald Bohr's promised land, new mathematical theories could spread throughout the world on the wings of a brilliant insight, a deft exposition, and a promising avenue for further study. Mathematicians did not enter this promised land all at once in the mid-twentieth century, but for more and more mathematicians in more and more places stories like those of Schwartz and the theory of distributions became less and less improbable. If no young theory ever really reaches every mathematician of the world, most mathematicians today can expect that the right kind of result, shared in the right way, might find a large audience on multiple continents in a short span of time. If not all mathematicians now live in Bohr's promised land, they all can imagine it, many have been there themselves, and the discipline's elite call it home. For those with the necessary combination of institutional access and theoretical insight, sharing a new idea across the world is not a dream but an expectation.

Consider just how far away, how very implausible, Bohr's vision was from the perspective of most mathematicians in 1950. While mathematicians had long pretended to a certain kind of universalism, their recent history had been defined by four decades of conflict and division. Though mathematicians remembered the period before World War I as comparatively peaceful and cooperative, their forebears' world was also undeniably small. Its latest mathematical theories could reputedly fit in the head of a single great mathematician, like Henri Poincaré, and its leading institutions lay predominantly in just one broad region of just one continent. The few years after World War II did not, for their part, portend any sort of transcendent unity. Tell a mathematician in Bohr's audience that within the next half-decade mathematicians would begin to trade interpretations of Schwartz's theory regularly and effectively across the Iron Curtain and he could cite a wealth of recent experience to suggest otherwise. Tell that mathematician that South America would emerge as a critical hub for the theory's research and teaching and he might not be able to name a single prominent mathematician or institution from the continent, much less countenance the notion that a place so far from mathematicians' European metropoles might be important to any theory of significance. After the American organizers struggled so mightily to host the 1950 International Congress of Mathematicians at Harvard, think of the leap for Hyderabad (2010), Seoul (2014), or Rio de Janeiro (2018) to claim pride of place for the world's mathematicians. Indeed, the latter's preeminence today owes in no small part to the legacy of Schwartz's disciple Leopoldo Nachbin and the institute Nachbin helped found at mid-century, as well as to decades of governmental and philanthropic programs that moved mathematicians between Nachbin's institute and the rest of the world.

These places index a mathematical discipline that was strikingly transformed after World War II into something I have called intercontinental mathematics, defined by new infrastructures of travel and communication that allowed mathematicians to organize their profession on an unprecedented scale. But intercontinental mathematics refers as well to that discipline's subject matter: mathematical theories that mathematicians engaged and communicated in dynamically interacting research communities across multiple continents. These two senses of intercontinental mathematics—as a discipline and its subject—arose in tandem and reinforced each other. New institutions and infrastructures supported new kinds of theories, whose users in turn shaped their institutional activities and infrastructural demands around their theoretical work.

The American organizers of the 1950 International Congress of Mathematicians met the disciplinary side of intercontinental mathematics at every turn. They used new economic and political resources to marshal support for their enterprise in and beyond Europe, and turned nascent ties to non-European countries into founts of legitimacy. As they tried to include all the world's mathematicians, they came to find the task beyond their reach for a variety of often-unanticipated reasons. Their response reveals a crucial feature of the intercontinental system they helped build. Rather than dwell on frustration after frustration, they aggressively sought to recast history and to rally mathematicians and sponsors under emphatic but ambiguous imperatives like that of being "truly international." Such imperatives meant many different things to the organizers' different audiences, and could have different implications at different times. Corralling these differences under a single bold rubric, the Congress's organizers sustained a sense of common purpose and collective endeavor that allowed them to charge ahead in spite of the many insurmountable obstacles before them. Ambiguity united them, let them tie communities together, helped them muddle through while planting the seeds of more robust formations.

In this regard, convening a congress on an intercontinental scale was a lot like forming an intercontinental research community around a mathematical theory. Mathematicians' theoretical struggles did not come in the form of denied visas or thwarted bids for ICSU sponsorship, but their persistent challenges in achieving common understandings of complex and difficult new ideas required similar kinds of maneuvers within the forms and genres of mathematical communication. Intercontinental research communities required interconnected but geographically dispersed groups of individuals to decide to study the same topic, to communicate their findings to each other, and to recognize their work as part of a coherent area of study. The community that amassed in the early postwar period around the theory of distributions met each of these requirements by using evocative metaphors and suggestive framings to foster interest and an appearance of common enterprise. Detailed, meticulous work by committed researchers on the theory's finer points would be important, to be sure, but that was not the work that made the theory travel so far and so fast, across languages and continents in the early Cold War.

Integrating distributions into one's day-to-day mathematical work required difficult learning and adaptation. But integrating them into one's worldview required little more than evocative metaphors like those in Schwartz's 1945 paper and Bohr's 1950 address—metaphors derived from quotidian practice but which mathematicians used in wide-ranging contexts both familiar and strange. Schwartz and Bohr promoted distributions by replacing the difficulty of mastering the theory with the lesser difficulty of appreciating it, of acting as though all one needed was a well-chosen rearrangement of symbols and a change in signs. Indeed, in this period this kind of metaphor-inflected appreciation was, in many settings, all one could hope for and all that was required. The rise of the theory of distributions bears witness to the outsized effect that short summaries in review journals, brief lecturing visits by traveling scholars, and cursory conceptual introductions in regional publications could have for organizing researchers separated by distance, training, and idiom. These medium-dependent connections made it possible for the theory to move from the French provinces to multiple continents in less than a decade.

Such intercontinental movement was only possible because so many different people were able to develop their own understandings and uses of the theory of distributions while believing themselves to be understanding and using one and the same theory. I have claimed that intercontinental mathematical communities and their respective theories were partially integrated, with mathematicians using common tropes and metaphors to bridge their diverse practices and ideals. But this historiographical wordplay on "integration by parts" is not just that. For the theory of distributions and its core metaphor of integration by parts, in a significant way, took part in a broader shift in what it meant to learn a new and fruitful theory in the postwar period.

Theories were not always identifiable as amalgamations of useful metaphors, well-chosen perspectives, and associated collections of formal claims and methods. They were not always recognizable through headings and citations in review journals, through specialized seminars and satellite meetings at major conferences. The particular kinds of sociality that would characterize distributions *as a theory* came about in very particular postwar circumstances. The synecdochic pair of distributions and integration by parts, moreover, became iconic in this period. As a highly visible and widely spread new theory, promoted and understood as such, the theory of distributions offered a model for those who would build new theories and new institutions. Controlling metaphors and definitive perspectives became what it was to appreciate the theory of distributions, and appreciating such a theory in the seemingly global cacophony of notions and methods was a newly important condition of incorporating it into one's research and teaching. Integration by parts is thus, here, not simply a metaphor for or representative of postwar mathematics but also, at least in part, a piece of a historical transformation.

Early postwar contexts of communication fit some theories and mathematicians better than others, and ensured that the most communicable aspects of any given theory came to dominate how mathematicians understood and shared what they did. Distributions were distinctive in the extent to which the kinds of metaphors and wordplay that traveled so well in lectures and review journals were central features of the theory's earliest articulation. From Schwartz's first publication on the subject and reverberating through his and others' early expositions, the theory depended on deft reframings of familiar terms and operations like differentiation and integration by parts. But the theory's contexts of articulation also shaped its presentations, so that the litany of the Dirac function's derivatives and the metaphoric use of integration by parts became increasingly central explanatory resources for a significant number of the theory's expositors by 1950, in ways that reverberated through scholarly work connected to the theory for the decade that followed.

Among the chief challenges to the historiography of twentieth-century mathematics has been the difficult work of assessing the relationships between different technical productions. The early history of distributions offers a reminder that such technical reconciliation was an ongoing and difficult task for historical mathematicians themselves. Such mathematicians, moreover, did not always need to succeed fully in such reconciliations in order to sustain significant programs of research. Indeed, in many respects such a goal would have been impossible. Historians are now rightly accustomed to finding the tensions, contradictions, and ambiguities that pervade past institutions. These have become potent means of explaining the historical development of projects whose ideals must inevitably be reconciled with the complex realities of social existence. Correspondingly, rather than seeking coherence in past theories, historians of modern mathematics can heed what was often the lived incoherence of those theories, as well as the necessarily superficial but nonetheless important ways such theories and their research communities cohered nonetheless.

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