From Nancy to Copenhagen to the World: The internationalization of Laurent Schwartz and his theory of distributions.

Michael J. Barany*, Anne-Sandrine Paumier, and Jesper Lützen
*corresponding author

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Addresses:
Barany: Dartmouth College, Society of Fellows, michael at mbarany.com
Paumier: Fondation Mathématique Jacques Hadamard, Institut des Hautes Études Scientifiques
Lützen: Department of Mathematical Sciences, University of Copenhagen

Abstract

Between 1947 and 1950, Laurent Schwartz (1915–2002) went from being almost unknown outside of France to being an international mathematical celebrity. This paper accounts for Schwartz's rapid ascent by focusing on the social, institutional, and mathematical contexts of his crucial trajectory from Nancy, via Copenhagen, to the world stage, culminating in his 1950 Fields Medal awarded in Cambridge, Massachusetts. We identify, based on new archival findings, the pivotal role of Danish mathematician Harald Bohr along this trajectory. Our analysis reveals the emerging dynamics of early postwar international mathematics, and explains how certain individuals and theories could rise to prominence in this period.

Keywords: Laurent Schwartz, theory of distributions, International Congress of Mathematicians 1950, Harald Bohr, Fields Medal, postwar mathematics, internationalization.
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Résumé

Inconnu jusqu'alors hors de la France, Laurent Schwartz (1915-2002) est devenu entre 1947 et 1950 un mathématicien célèbre sur le plan international. Cet article présente l'ascension rapide de Schwartz en se concentrant sur les contextes sociaux, institutionnels et mathématique de sa trajectoire, depuis Nancy, via Copenhague, jusqu'à la scène mondiale, culminant avec sa médaille Fields, attribuée en 1950 à Cambridge, Massachusetts. Nous identifions, à partir de nouvelles découvertes dans les archives, le rôle central du mathématicien Danois Harald Bohr, le long de cette trajectoire. Notre analyse révèle certaines dynamiques émergentes des mathématiques internationales dans l'immédiate après-guerre, et explique comment certains individus et théories ont pu prendre une place proéminente pendant cette période.

Introduction

Between 1947 and 1950, Laurent Schwartz (1915–2002) went from being almost unknown outside of France to being an international mathematical celebrity. His claim to fame was his theory of distributions, with which he proposed a generalization to the notion of function in mathematical analysis so that distributions (his generalized functions) always had well-defined derivatives. At first glance, Schwartz appears to have ridden his theory's obvious merits to greater and greater recognition, culminating in his receipt of a Fields Medal at the 1950 International Congress of Mathematicians. His rapid rise through the ranks of international mathematics could be attributed largely to his timely insight into the theory of differential equations and topological vector spaces, which mathematicians and mathematical physicists duly adopted for its conceptual advantages while crediting Schwartz as the theory's originator. On the testimony of Harald Bohr (1887–1951) from the Fields Medal presentation at the opening ceremony of the 1950 Congress, one could be forgiven for thinking Schwartz's fame derived naturally and inevitably from his theory's brilliance.

But first glances rarely tell the full story. A closer look reveals a rather different explanation for Schwartz's and distributions' international success, which was deeply embedded in the turmoil the international mathematical community confronted at the close of the Second World War. To be sure, Schwartz's idea for distributions could appear powerful and compelling, but it could hardly in 1947 (or even 1950) be called a fully fleshed-out theory. In the frayed and disarrayed channels of postwar mathematics, moreover, international recognition for even the most profound mathematical interventions was far from assured.[1] Indeed, Bohr's narrative entirely omitted what we have identified through archival research as perhaps the most important factor in Schwartz's early international recognition: the advocacy of Harald Bohr himself, with an important supporting role for Bohr's Copenhagen colleague and protégé Børge Jessen (1907–1993). While we dare not speculate about a counterfactual history where Schwartz and Bohr did not cross paths in 1947, it is clear to us that in the history that did indeed transpire Bohr was, again and again, the engine that helped drive Schwartz and distributions to international fame. That Bohr's role in this history has remained almost entirely unrecognized by historians or mathematicians is a testament to his thoroughgoing success at quietly and deliberately leaving his mark on the first postwar generation of mathematical leaders.[2]

With this paper, we integrate the evidence from early publications and reviews related to distributions with private letters that attest to Bohr's, Schwartz's, and others' personal efforts to bring distributions to the world. Our analysis pinpoints Bohr's precise role, relating it to developments in Schwartz's theory in the critical period from 1947 to 1950 when Schwartz entered the international stage. Mathematically, we show how problems from harmonic analysis (especially as related to Fourier transformations) dominated early discussions of Schwartz's theory, shaping both Schwartz's theoretical reception and his earliest international networks of interlocutors and advocates. We set this against the initial geography and chronology of Schwartz and his theory, identifying the steps that took both, as our title indicates, from Nancy to Copenhagen to the world.

While the pivotal progression of Nancy to Copenhagen to the world has been obscured by the celebratory amnesia of mathematicians' collective memory of Schwartz and distributions, this sequence's historical importance was clear to Schwartz at the time. Writing to Jessen not long after Bohr's death, Schwartz attested: "I have for Bohr an exceptional sympathy; he also remains linked for me to my first international colloquium [in Nancy], my first foreign voyage [to Copenhagen], and the Fields medal."[3] The leap from Nancy to Copenhagen was natural enough. Schwartz met Bohr and Jessen in Nancy and they then swiftly invited him to visit their home institution some months later. Schwartz may have seen a thematic relationship between Bohr's appearance in Nancy and Copenhagen and his later place at the podium presenting the 1950 Fields Medals, with each of Bohr's appearances signaling a milestone for Schwartz's international ascent. But we have excavated as well a direct line of historical development, through Bohr rather than Schwartz (and that Schwartz himself may not even have recognized) explaining Schwartz's Fields Medal with reference to Nancy and Copenhagen. If, as Paumier has put it, Nancy was the "springboard" for Schwartz and his theory (Paumier 2014, 131-137; Paumier 2016), Copenhagen was surely the vault and the 1950 International Congress of Mathematicians the first of several dramatic landings. A full reckoning shines a light on the gymnastic gyrations of early postwar international mathematics.

[1] On these tensions and difficulties in the contested reconstruction of international mathematics in this period, see Barany 2016a, chs. 3-4.

[2] Lützen (1982, 160) detected Bohr's intention to promote distributions, and Paumier (2014, 2016) and Barany (2016a) have more recently indicated a more thoroughgoing role, which we synthesize here along with previously unreported findings.

[3] "j'avais pour Bohr une exceptionnelle sympathie; il reste aussi lié pour moi à mon premier colloque international, à mon premier voyage à l'étranger, et à la médaille Fields." Schwartz to Jessen, 26 Dec 1953, AMDKU. See also Schwartz to Jessen, 24 Jan 1951 "You know how wonderful he [Bohr] has always been for me, and how he helped me in explaining the distributions to many people."

The Approach: Distributions and Bourbaki

Before Schwartz and his distributions traveled across the world, they first found recognition within France. In particular, Schwartz's early formation among the group of French mathematicians who called themselves the collaborators of Nicolas Bourbaki, as well as the Bourbaki collective's rapid embrace of Schwartz's theoretical program, provided a very favorable frame for Schwartz's rapid post-1947 international ascent. This section reviews Schwartz's initial proposal for a theory of distributions in his Bourbaki contexts, revising the account in Schwartz's (2001 [1997], esp. ch. 4-6) autobiography—which, composed largely from memory a half century after the events in question, is not always reliable as a historical record—in light of the small amount of extant documentation from the period. Regarding Schwartz's mathematics, we have preserved the terminology and notations from the original sources to the extent possible, indicating the most relevant conceptual and historical contexts without aiming for a comprehensive exposition. For the detailed "prehistory" of the theory and its various mathematical contexts, see Lützen (1982).

Nicolas Bourbaki was the pen name adopted by an upstart group of mathematicians in Paris in December 1934, who aimed originally to reform the French university curriculum in mathematical analysis but whose project grew into an iconic and influential program regarding the modern foundations of mathematics as a unified whole (see Beaulieu 1989; Beaulieu 1993; Corry 2004; Corry 2009). The Bourbaki collaborators, who mostly emanated from the elite École Normale Supérieure in Paris, developed an international outlook for their mathematics strongly informed by interwar foreign study (especially in Germany) supported foremost by Rockefeller philanthropy (Beaulieu 1989; Siegmund-Schultze 2001). Socialized among the Bourbaki at the École Normale and trained in their midst during World War II, Schwartz became one of the earliest non-founding members of the Bourbaki collaboration. This put him, after World War II, at the vanguard of an emerging generation of French mathematicians with an ambitious international agenda.[4]

After his studies at the École Normale and his military service, Schwartz spent the beginning of the Second World War in Toulouse in the free zone. There, he and his wife met with Bourbaki collaborator Henri Cartan, who advised them to go to Clermont-Ferrand, where the mathematics faculty of the Université de Strasbourg had relocated. Starting in 1940, Schwartz's doctoral studies in Clermont-Ferrand accompanied his induction into the idiosyncratic circle of Bourbaki, including his first participation in formal Bourbaki congresses. Under the collaborationist Vichy regime, and with an intensifying German occupation, Schwartz faced considerable risks in France as both a Jew and a Trotskyist. Shortly after defending his thesis on polynomial approximations of functions, he fled to the countryside and went into hiding. In the village of Saint Pierre de Paladru, some 40km north of Grenoble in Eastern France, Laurent Moïse Schwartz assumed the name Laurent Marie Sélimartin—a moniker he chose to be indistinguishable from the original in case he accidentally signed his true name.

With Paris liberated in September of 1944, Schwartz took up a temporary position in the capital. Among the many benefits of emerging from hiding, Schwartz could now communicate more freely with other mathematicians and begin to catch up with the mathematical literature (which remained significantly affected by the war). Inspired by fresh work by Choquet and Deny (1944), Schwartz began working on generalized solutions to partial differential equations and produced a short notice on the topic in response to Choquet's and Deny's article (Schwartz 1944). This led, in early November (though one cannot rule out late October), to what he later described as "the most beautiful night of my life" (Schwartz 2001, 232), during which he invented a precursor of distributions. His idea was to consider a certain type of operator as a generalized function. More precisely, he defined a convolution operator as a continuous linear operator T

[4] On this generational tension in French mathematical internationalism, see Siegmund-Schultze 2009.

from the space $D = C_c^\infty$ of infinitely differentiable functions with compact support (i.e. vanishing outside of a compact set) equipped with the usual Fréchet topology for which

$$T \cdot (\phi * \theta) = (T \cdot \phi) * \theta$$

where ϕ and θ are elements of D and $*$ indicates convolution. When f is a continuous function one can define a convolution operator as follows:

$$(T \cdot \phi)(x) = (f * \phi)(x) = \int_{-\infty}^{\infty} f(y)\phi(x-y)dy\,.$$

In this way one can identify a continuous function with a convolution operator. However, Schwartz remarked, there are convolution operators that cannot be identified with a continuous function in this way. For example, the identity operator corresponded to the Dirac (1930) δ function with the property:

$$\int_{-\infty}^{\infty} \delta(y)\phi(x-y)dy = \phi(x)\,.$$

"Function," however, would be a misnomer, as the Dirac δ could not be expressed as a conventional real-valued function. Convolution operators were thus, in this sense, more general than ordinary functions. Schwartz then defined the derivative T' of a convolution operator T as follows:

$$T' \cdot \phi = (T \cdot \phi)' = T \cdot \phi'$$

Since the test functions φ are by definition differentiable, this guarantees that T will have a validly defined derivative. The definition thus generalized differentiation of functions such that all operators have a derivative. Later, Schwartz proved that on every relatively compact open set every operator is in fact a finite sum of derivatives of continuous functions. He also gave a rather complicated definition of multiplication of an operator with a C_c^∞ function and he began to speculate about how he could generalize the Fourier transform to the new operators.

By December, Schwartz had already shared this idea with his Bourbaki interlocutors, who also discussed it among themselves.[5] However, he encountered some conceptual difficulties, especially regarding Fourier analysis, that forced him to modify his approach (Schwartz 2001, 234). Around the new year, not long after having arrived in Paris, Schwartz and his family packed their bags again and relocated to Grenoble. There, Schwartz filled the position at the University of Grenoble of Jean Küntzmann, who remained a prisoner of war in Germany. After settling in Grenoble, Schwartz determined that his conceptual problems might be resolved by replacing convolution operators with linear functionals. He thus defined a distribution as a continuous map into the real numbers from the space of compactly supported and infinitely differentiable test functions $D = C_c^\infty$. That is, he took the same space of test functions as before, but considered a different operation on that space. This corresponded to a subtle difference of notation, replacing the "dot" of the convolution operator with the function notation of a linear function.

A locally integrable function f (not necessarily in D) defines a distribution acting on $\phi \in D$ by

$$T(\phi) = \int_{-\infty}^{\infty} f(x)\phi(x)dx$$

and in general (i.e. whether or not it can be expressed as such an integral) a distribution is defined by the value it assigns to each test function. For example the Dirac δ maps each ϕ to $\phi(0)$. All distributions have derivatives of all orders defined by $T'(\phi) = -T(\phi')$, an equation that corresponds to integration by parts where the distribution is a locally integrable function. As with convolution operators, Schwartz

[5] Cartan to Weil, 10 Dec 1944, in Audin 2011, 96.

could show that all distributions were locally sums of derivatives of functions. He hoped his new formulation would allow a resolution to the problem of defining Fourier transforms on distributions, but this remained a topic of investigation. In his autobiography, Schwartz claimed to have resolved the Fourier problem in the spring of 1945 (Schwartz 2001, 238), but we shall see that fundamental questions about Fourier analysis with distributions remained at least until 1947.

As early as March 1945, the Assemblée de professeurs du Collège de France granted Schwartz the prestigious Cours Peccot, a series originating at the turn of the twentieth century for promising young scholars, for 1946 (CdF-Peccot). At the time, the only mathematician at the Collège de France was founding Bourbaki collaborator Szolem Mandelbrojt, and Schwartz's wartime Bourbaki connections and prior ties to the École Normale Supérieur can only have helped his selection. He chose to present on the topic "Une extension de la dérivation et de la transformation de Fourier," and this course also gave rise to Schwartz's first short article introducing distributions (Schwartz 1945). The June following the Cours Peccot, Schwartz gave a 4 hour presentation of his theory at Bourbaki's Summer Congress.[6] This, along with Schwartz's other Bourbaki interactions, led to the theory's incorporation into the planned table of contents for Bourbaki's grand *Éléments de Mathématique*—though this portion of the extended multi-volume work was never produced. More importantly, Schwartz was later asked to draw up a list of "Théorèmes utiles pour les distributions" (Useful theorems for distributions) for Bourbaki's volume on topological vector spaces (Bourbaki 1949, 35; Paumier 2014, ch. 1). The topological exegesis led, in turn, to a joint article with Jean Dieudonné where they introduced the LF-topology on the space of distributions (Dieudonné and Schwartz 1949).

While the collaborators of Bourbaki intended to include a fundamental treatment of Schwartz's work in their collectively-authored series, Schwartz's own definitive textbook on the subject (with the same French publisher and series as the Bourbaki volumes) remained unfinished. One can only speculate regarding the reasons for the delay, but Bourbaki almost surely had something to do with it. Bourbaki gave Schwartz a community of high-level interlocutors from the very beginning, reducing the pressure to develop his theory at length in expository formats suitable for publication. The prospect of including distributions in a long-deferred, and ultimately abandoned, exposition of the Bourbaki collaboration itself may have further distracted Schwartz from his own formulation. Indeed, Schwartz's Bourbaki interlocutors shared a running joke between 1946 and 1950 that Schwartz's treatise would always be finished 'next year' (Bourbaki 1949, 5; A-SP interview of Bernard Malgrange, 1 Dec 2010). Whatever the reasons, for our purposes it is important mainly to observe that Schwartz's theory acquired an international profile in the absence of a textbook introduction to the subject, which ultimately appeared in two volumes (Schwartz 1950-51). This limited who could speak for the theory and how, and placed greater emphasis on the personal connections cultivated by Schwartz, his Bourbaki collaborators, and (as we shall see) Harald Bohr.

The Springboard: the Colloque d'Analyse Harmonique, Nancy 1947

The occasion of the first international audience for the theory of distributions came at the initiative of Jean Delsarte, another founding member of Bourbaki, in the form of a gathering held in Bourbaki's unofficial home of Nancy from 15 to 22 June, 1947 (see Paumier 2016; Paumier 2014, ch. 3). The Colloque d'Analyse Harmonique was one of the first scientific meetings—the very first in mathematics—to take place under a postwar collaboration between the Rockefeller Foundation (with which Bourbaki had, by then, a relatively longstanding association) and the Centre National de la Recherche Scientifique, meant to stimulate the resumption of elite international science in France (Zallen 1989; Krige 2006, ch. 4). Schwartz served as one

[6] Cartan to Weil, 19 Jul 1946, in Audin 2011, 118.

of two secretaries for the meeting, which was largely directed by Jean Delsarte, then Dean of the Faculté des Sciences of Nancy, and officially co-organized by Szolem Mandelbrojt from the Collège de France in Paris.

In keeping with the Rockefeller Foundation's goal to decentralize elite science in France, half of the small and specialized CNRS international conferences were to take place in universities outside Paris. As for the topic of the harmonic analysis conference, it seems that Delsarte chose to promote the recent results of his young professors in Nancy, Laurent Schwartz and Roger Godement, who would there have the opportunity to speak in front of an international and elite audience. He likely also used his Bourbaki contacts to make his invitations to what was a very early occasion for postwar international science, with normal peacetime channels of international communication still being rebuilt.

Schwartz made a number of significant connections as a secretary and presenter for the meeting. For example, he was able to circulate a number of offprints before the event to all the participants, and included among these his first article on distributions, related to his Cours Peccot. This, he wrote, would "serve as a point of departure for the lecture I will give at the Colloquium."[7] His most important connection was meeting, for the first time, leading Danish mathematician Harald Bohr and his protégé Børge Jessen. Bohr, the younger brother of the physicist Niels, had gained a name in mathematics with his theory of almost periodic functions. After publishing three papers on this theory in *Acta Mathematica* in 1925-26, Bohr had traveled the world giving talks about this new generalization of harmonic analysis. In particular he undertook extended visits to Göttingen in 1928-1929 and to the United States in 1930-1931, forming robust relationships with many of the world's leading mathematical analysts in the process. The following year, Bohr published a book length presentation of his theory with Springer Verlag (Bohr 1932).

Of all the great names on the list of speakers at the Nancy colloquium, Norbert Wiener's stands out. The only speaker from across the Atlantic, he gave the first talk of the meeting.[8] In his autobiography, Wiener reminisced that "much of the meeting was to deal with my ideas" (Wiener 1964, 314). Though characteristically immodest, his assessment was not unfounded. His landmark 1930 article on "Generalized Harmonic Analysis" (Wiener 1930) set the terms for modern adaptations of Fourier methods in harmonic analysis, and many of the colloquium talks dealt with generalizations of Fourier series and the Fourier transform that built on Wiener's interwar intervention. For example, the stated purpose of Bohr and Jessen's presentation, entitled "Mean motion and almost periodic functions," (Bohr and Jessen 1949) was to "indicate the results" reported in a paper in *Acta Mathematica* by Jessen and Hans Tornehave (1915-1998) (Jessen and Tornehave, 1945), a student of the two presenters. The only contribution not offered in French, their presentation focused on generalizing Fourier methods to almost periodic functions.

Several of the presentations, however, dealt with generalizations in a direction much closer to the problems then occupying Schwartz with the theory of distributions. When used to solve differential equations, Fourier transforms present an obstacle insofar as one must guarantee the convergence of the Fourier integral. Typically, this required limiting oneself to functions that fall off rapidly at infinity. In a series of papers beginning in 1925, Hans Hahn and Wiener began to generalize the Fourier transform to functions subject to weaker growth conditions at infinity (e.g. Wiener 1925). A few years later Salomon Bochner generalized Wiener's method even further.[9] The idea was to multiply the function f in question by a

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[7] Schwartz to Jessen, 6 Jun 1947, AMDKU, "P.S. Je vous ai envoyé quelques tirages à part. L'article des Annales de Grenoble sert de point de départ à la conférence que je ferai au Colloque." Schwartz's talk in Nancy moves very quickly from basic definitions to new results, further suggesting that he presumed his audience had access to the 1945 Annales de Grenoble article (Schwartz 1949, 1-2).

[8] The meeting program is in Archives départementales de Meurthe et Moselle, liasse W 1018/96, reproduced in Paumier 2014, annexe K, 347-350. Wiener's Massachusetts Institute of Technology colleague, the French-trained mathematician Raphaël Salem, attended as well but was not listed in the program.

[9] He first presented his theory in the paper Bochner 1927 and later included it in his book Bochner 1932. For a history of this development see Lützen 1982, 73-9.

suitable negative power of its variable x and then take its Fourier transform. The Fourier transform of f could then be considered as some kind of generalized derivative of the Fourier transform of the product function, and such a generalized transform worked for any function that grew slower at infinity than some polynomial. Among the drawbacks to Hahn's, Wiener's, and Bochner's generalization of the Fourier transform was that it substituted a more versatile mathematical object for the one in question, rather than finding a setting or approach that preserved the original object where possible. This meant, among other things, that the inverse transform was of a completely different nature than the transform itself.

Bochner was not present at the Nancy meeting, but three other participants presented alternative ways to generalize the Fourier transform that took account of this difficulty. The Swedish mathematician Torsten Carleman's (1892–1949) talk entitled *Sur l'application de la théorie des fonctions analytiques dans la théorie des transformées de Fourier* presented a simple and symmetric theory that he had lectured on in 1935 and published during the war (Carleman 1944). He defined the Fourier transform on pairs of analytic functions in the upper and lower half plane respectively satisfying certain growth properties. The result of such a transform was again a similar pair of analytic functions, and as in the usual Fourier transform the inverse transform was essentially the same as the transform itself. For functions on the real axis that can be represented as the difference along that axis between two such analytic functions, Carleman's Fourier transform gave the same result as the classical transform where it exists. However, Carleman's transform also applied, for example, to locally integrable functions that grow more slowly than a polynomial at infinity for which the conventional Fourier transform did not apply. Carleman's methods were continued by Mikio Sato (1928-), who considered such pairs of analytic functions as so-called hyperfunctions (Sato 1958). In fact, this approach was later understood as a further generalization of the one Schwartz introduced at the 1947 colloquium (Kiselman 2002).

Another Swedish mathematician, Arne Beurling (1905-1986), presented a generalization similar to that of Carleman. The third Fourier generalization of note here was Laurent Schwartz's, which we shall discuss in detail in the next section. Schwartz's contribution, from which he prepared and separately submitted a standalone article in the *Annales de l'université de Grenoble* that September (Schwartz 1947-48), became the first entry in the colloquium proceedings. That volume, published in 1949, also featured (in order): Arne Beurling, Michel Plancherel, Torsten Carleman, Gaston Julia, Norbert Wiener, Harald Bohr and Børge Jessen, Szolem Mandelbrojt, Alexander Ostrowski, Jean Favard, Paul Lévy, and André Blanc-Lapierre. Roger Godement's talk was published in a supplement, often bound together with the volume. Henri Cartan, Jean Delsarte, and Michel Loève gave presentations that were not represented in the proceedings.

Many of those present in Nancy would contribute in one fashion or another to Schwartz's early advancement. We discuss the roles of Bohr and Jessen below, and have already alluded to the recurring interventions of the many Bourbaki collaborators in Schwartz's orbit, including at Nancy. Michel Loève, a mathematician trained in France but based at the time in London, facilitated Schwartz's visit to England the following year (Schwartz 2001, 302-303). Schwartz also received an invitation from Plancherel in 1948 to lecture in Zürich. During his trip, he also lectured about distributions in Lausanne at the invitation of Georges de Rham.[10] Paul Lévy, Schwartz's father-in-law and a professor at the École Polytechnique, facilitated Schwartz's professional ascent in Paris, though of course he did not require the Nancy colloquium to make Schwartz's acquaintance (Paumier 2014, §6.2). Above all, the colloquium secured for Schwartz a place in a distinguished international cohort of researchers in harmonic analysis, reinforcing the credibility and prestige of his program to rewrite the foundations of differential equations.

[10] De Rham Papers. We thank Christophe Eckes for identifying and sharing these materials with Paumier.

A Reverberation: Spherical (tempered) distributions

Schwartz's paper for the 1947 Nancy colloquium, titled "Théorie des distributions et transformations de Fourier" (the title in the proceedings Schwartz 1949 differed only in dropping the 's' from 'transformations'), presented Schwartz's distinctive generalization of Fourier transforms. As noted previously, the challenge of presenting a workable Fourier transform for distributions preoccupied Schwartz during much of his earliest work on the theory. In his autobiography (Schwartz 2001, 238), he claimed to have resolved this difficulty in the late spring of 1945, not long after he formulated his ultimate definition for distributions themselves. However, despite a reference to Fourier transforms in its title, this formulation and the reasoning behind it were not in evidence in Schwartz's first paper on distributions (Schwartz 1945), which appeared in 1946 after his Cours Peccot.

In that first article, Fourier series and integrals come up as an extended example within the concluding section on "Topological structure in the space of distributions" (ibid., 71-74), in order to illustrate the notion of convergence in the space of distributions Schwartz had just defined. For instance, Schwartz asserts that "the Fourier series $\Sigma a_n e^{inx}$ is convergent as soon as $a_n = O(|n^{\alpha}|)$, for arbitrary α real > 0"[11] and also that the Fourier integral $\int_{-\infty}^{+\infty} f(x)e^{itx}dx$ converges if $|f(x)| = O(|x^{\alpha}|)$ for $\alpha > 0$ when $x \to \pm\infty$. He observes, without presenting details, that "There exists a reciprocity formula, but it is more complicated than in the case where *f(x)* is summable."[12] The article ends with the open possibility:

> But it is also possible to extend greatly the field of application of Fourier and Laplace transforms, and to define the transformations of all distributions whatever their irregularity and their behavior at infinity: one is then obliged to introduce a new family of distributions with a handling clearly more complicated and less intuitive.[13]

Schwartz did not reveal any particulars about this more complicated theory, but it seems improbable that he had the approach from his 1947 presentation in mind. The latter method is hardly more complicated than manipulating ordinary distributions, and arguably more straightforward. He seems, moreover, to have believed in 1946 that Fourier analysis could be conducted in a more complicated space that includes all ordinary distributions, whereas his 1947 approach worked by restricting to a subspace of distributions. At the close of the initial article on distributions, Schwartz promises a future paper on mean-periodic functions that would make advantageous use of these notions.[14] The memoir to which he appears to refer, titled "Théorie générale des fonctions moyenne-périodiques," does discuss his distribution theory and gives a few more details about the Fourier transform (Schwartz 1947, 862-863). But once again, he considers only some specific cases, and gives a theorem that characterizes Fourier transforms of distributions with compact support (at the time, using the language of a "distribution à noyau compact," i.e. "distribution of compact kernel") using Paley-Wiener results.

In the late 1970s, Lützen interviewed Schwartz and asked him what he had meant by his remarks about the Fourier transform in his initial distributions paper. By then, Schwartz was already confident (as in his much later autobiography), that he knew his 1947 solution already in 1945, and he even believed to have had a precursor to the latter approach in his theory of convolution operators. Confronted with the exact

[11] "... la série de Fourier … est convergente, dès que $|a_n|= O(|n|^{\alpha})$, α réel > 0 quelconque."

[12] "Il existe une formule de réciprocité, mais elle est plus compliquée quand dans le cas où *f(x)* est sommable." Schwartz 1945, 73.

[13] "Mais il est également possible d'élargir beaucoup le champ d'application des transformations de Fourier et Laplace, et de définir les transformations de toutes les distributions, quelle que soient leur irrégularité et leur comportement à l'infini : on est alors obligé d'introduire une nouvelle famille de distributions d'un maniement nettement plus compliqué et moins intuitif." Schwartz 1945, 74.

[14] "Je publierai prochainement un mémoire sur les fonctions moyenne-périodiques où ces diverses notions sont avantageuses."

wording of the paper, Schwartz was surprised and admitted that it looked as though he did not have his idea from the Nancy colloquium at the time. He was, however, incredulous that he had overlooked the solution for so long.

What, then, was the solution Schwartz found so obvious in retrospect? Recall that Schwartz defined distributions as continuous linear functionals on the space of compactly supported infinitely differentiable functions. For distributional Fourier analysis, Schwartz considered instead the smaller set of distributions that extended as continuous linear functionals on the larger space of infinitely differentiable functions that (rather than being compactly supported) merely fell off at infinity faster than any power of 1/x. Schwartz called this latter space $\mathcal{S}$, and showed that its dual $\mathcal{S}$' permitted a distributional Fourier transform F defined as the dual of the Fourier transform in $\mathcal{S}$:

$$F(T)(\phi) = T(F(\phi))$$

With this definition and for this space of distributions, the Fourier transform was its own inverse, just as in classical Fourier theory.

Schwartz called the new type of distributions used in Fourier analysis "spherical distributions," denoting their dual space with a script "$\mathcal{S}$" (for "spherical") and the space of distributions itself as $\mathcal{S}$'. The terminology referred to the observation that such distributions (in n dimensions) were precisely those that could be extended continuously to a distribution on the n-sphere. This terminology did not, however, last long. By 1951, Schwartz claimed to have realized that "the sphere does not play a particular role" and he began referring instead to "distributions tempérées" (Schwartz 1951, 93), which were first translated into "temperate distributions" (*Mathematical Reviews* review of Schwartz 1951 by Segal, who gives the new and old terminology correspondence in his review), and then mostly by "tempered distributions."[15] Today, few remember the "spherical" origins of Schwartz's notation and many assume it refers to the "Schwartz space." We find evidence of this latter eponymous usage as early as the mid-1950s, though the expression "Schwartz space" has, from that period onward, been used in different contexts to describe a variety of spaces associated with Schwartz and distributions, not just the spaces $\mathcal{S}$ or $\mathcal{S}$'.

At the Nancy colloquium, Schwartz's intervention was greeted with considerable interest. Again with characteristic self-regard, Wiener (1964, 318) recalled that:

> Of all the Nancy people, the one whom I saw most often was Laurent Schwartz. …He had generalized still further the field which I had already treated in my Acta paper on generalized harmonic analysis. He reduced it to that highly abstract basis which is characteristic of all the work of the Bourbaki school to which he belonged.

Not everyone with a stake in these developments, we should note, was as impressed. Bochner checked out Princeton University's copy of the colloquium proceedings as soon as it returned from the bindery in March, 1951.[16] Around that time, he also prepared a notably critical review of Schwartz's long-awaited two-volume textbook on distributions (Schwartz 1950-51).[17] Bochner (1952) considered Schwartz's achievement of a self-inversive Fourier transform to be a trivial consequence of the space of testing functions, and concluded that the book as a whole was not discernibly innovative. By the time Bochner's review appeared, however,

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[15] The first occurrence of "tempered distributions" we found is in the review of Bruhat 1956, by Mackey (MR0084713; 18,907i), in which he specifies that "Here tempered means being not too badly unbounded in a precise sense suggested by Schwartz's definition of tempered distribution."

[16] Barany has inspected the original borrowing slips, which remain tucked inside the volume in Princeton University's library.

[17] Bochner's obituary of Harald Bohr in the same volume (pp. 72-75) mentions Schwartz's generalized Fourier analysis, also rather skeptically: "a partial answer to the problem, perhaps." We thank Reinhard Siegmund-Schultze for calling our attention to this passage.

Schwartz's reputation as a mathematical pioneer was well established. As we shall now see, nobody did more to promote that reputation than the colloquium's Danish delegation, led by Harald Bohr.

The Vault: Schwartz's invitation to Copenhagen

Of beer's many great contributions to the social and institutional lives of mathematicians, one of the greatest was by way of the Carlsberg Foundation's leading role in financing a new building for a mathematics institute at the University of Copenhagen, which opened in 1934.[18] With Harald Bohr as its first chairman, the new mathematics institute stood as a notable counterpart to Niels Bohr's adjacent Institute of Theoretical Physics. Harald Bohr continued as chairman through the war, although his Jewish descent forced him to flee to Sweden from 1943 to 1945 (where his salary was underwritten by the Rockefeller Foundation). Before the Second World War, Bohr had worked hard to promote international cooperation. During the Nazi regime in Germany, he vocally opposed discrimination against Jewish mathematicians and actively assisted efforts to aid refugee scholars (see e.g. Sørensen 2014). These institutional and political efforts solidified the international connections he had made previously through his mathematical research, and helped him emerge from the war as a widely respected leader of the discipline.

In 1948 Bohr handed over the post as chairman of the mathematics institute to his student Børge Jessen, who had continued Bohr's work on almost periodic functions and had contributed to several other areas of mathematical analysis and geometry. Bohr and Jessen regularly traveled and collaborated together, so that by the time of Bohr's death in 1951, Jessen readily stepped into his mentor's place as the leader of Danish mathematics and an active figure in the international mathematical organizations until his retirement in 1977. During the period 1955–1969, moreover, Jessen served as the head of the Carlsberg Foundation, at the time the most important private supporter of science in Denmark. He wielded this position to great effect for strengthening domestic mathematics and making connections. Indeed, Jessen's incoming correspondence from this period is full of letters from foreign scientists offering their thanks for cases of Carlsberg beer that Jessen had sent them.

Along with most of the other foreign participants in the Nancy colloquium, both Bohr and Jessen extended their trip in order to participate in a 24 June meeting organized by the Société Mathématique de France at UNESCO House in Paris on the topic of creating a new International Mathematical Union (see Barany 2016a, 170-176). Whether and how to constitute such a union, in view of what mathematicians widely regarded as an utterly failed interwar International Mathematical Union, was an open and vigorously debated question at the close of World War II. In the summer of 1947, Bohr found himself as one of a relatively small number of prominent figures deliberately courted by both French and American mathematical leaders, who were respectively vying to control the new process of organization. The Rockefeller-funded Nancy colloquium itself had international aims and motivations, and the meeting at UNESCO House assured that postwar international cooperation and comity figured especially prominently in Bohr's conception of his visit to France.

Laurent Schwartz, in this respect, made for an especially attractive prospect. The young French mathematician was undeniably charismatic, a captivating expositor who could engross and entertain his audiences at the blackboard and bar table alike. No doubt the Danes saw something of themselves in him, as they too were highly regarded lecturers.[19] Schwartz presented, moreover, and notwithstanding Bochner's

[18] The Danish 'institut' can correspond to what in American academia one refers to as 'institute' or 'department,' more typically the latter. We shall translate it as 'institute' here with that proviso.

[19] The annual teaching prize of the University of Copenhagen is called the "Harald of the year" in commemoration of Harald Bohr. See also Leon Cohen's (Princeton Oral History) recollection, "There was another man visiting here who was very good at that [lecturing]: Harald Bohr, the brother of the physicist. I still have a lively picture of Bohr's lectures. Bohr lectured with three objects in his left hand: a piece of white chalk, a piece of red chalk, and a cigar. I

later assessment, a theory that was plainly novel (at least in outward appearance) and potentially important. Distributions gave the 60-year-old Bohr a compelling gateway to the new Bourbaki style of functional analysis, in part because Schwartz offered numerous conceptual bridges to more familiar topics. While Schwartz justified the theory of distributions to his Nancy audience as an approach to the difficult problem of generalized Fourier analysis, its explanation (noted above) in terms of the more widespread Dirac calculus gave the theory a wide range of applications, at least in principle, to fundamental questions in mathematics and mathematical physics.[20]

In the weeks and months after Nancy, Bohr latched onto this presentation of the theory in terms of its connection to physics, which was also the dominant view in the paper Schwartz circulated in advance of the colloquium, and downplayed its more technical features and purely mathematical applications. On Bohr's return to Copenhagen, recalled Asger Aaboe (then a student at Copenhagen and later a well-known Yale historian of mathematics), Bohr "spoke very enthusiastically about [the theory of distributions]. Suddenly Dirac's delta function could be dealt with in a decent way" (Ramskov 1995, 365). Another student from the time, Bent Fuglede, recounted to Lützen: "I clearly remember the enthusiasm with which Harald Bohr and Børge Jessen spoke about Schwartz' theory of distributions when they returned from a visit to France in 1947 (I think), where they had met Laurent Schwartz and heard about the subject."[21] Schwartz himself fondly remembered Bohr's embrace that summer, noting in his autobiography that "Harald Bohr became enthusiastic about the theory of distributions, which he studied in detail, exchanging many letters with me" (Schwartz 2001, 301).[22]

Already during the Nancy meeting, Bohr and Jessen had told Schwartz that they intended to invite him to Copenhagen, though Schwartz made no mention of the plans when thanking Jessen for sending him some offprints in early July.[23] By mid-July, Bohr and Jessen had made the necessary arrangements and sent the formal invitation, in English (see appendix). The letter reported that Bohr and Jessen's colleagues at both the University of Copenhagen and the Technical University (Danmarks tekniske Højskole, which they rendered "Ecole Polytechnique" in the letter) "got enthusiastic about the possibility of seeing you here and of hearing some lectures of you on your extraordinary theory of distributions." Praising Schwartz's Nancy exposition, Bohr and Jessen hoped Schwartz would prepare a general presentation of his theory with minimal prerequisites, to be followed by details in further lectures.

Even before Schwartz could respond to the invitation, Bohr was busily at work promoting distributions internationally. Bohr noted in the invitation letter that the prominent applied mathematician Richard Courant might join him at his summer home in Fynshav that summer, and indeed he did.[24] Courant

think I wasn't the only one who waited for him to put the chalk in his mouth and write on the blackboard with the cigar. It never happened. He was an excellent expositor."

[20] We have each elsewhere discussed the background, origins, and effects of Schwartz's framing in terms of mathematical physics, as well as electrical engineering (by reference to the Heaviside operational calculus), which appears to have originated in Schwartz's Cours Peccot. See Lützen (1979; 1982, ch. 4), Paumier (2014, esp. 150-154), Barany (2016a, esp. ch. 2).

[21] Private e-mail from Bent Fuglede to Lützen, 7 Jul 2015.

[22] No mathematical correspondence between the two is preserved in the H. Bohr papers at the Mathematics Department of the University of Copenhagen, nor in the Fonds Laurent Schwartz at the École Polytechnique, although not all of Schwartz's surviving collection of correspondence from that period has been deposited in his archive to date. The letters arranging Schwartz's visit to Copenhagen were all written by Jessen, except the first one that was signed by both Jessen and Bohr. It would not be out of character for Schwartz to conflate his correspondence with Jessen and Bohr in his autobiography, and in any event he could safely assume that letters to Jessen would be available to Bohr as well.

[23] Schwartz to Jessen, 10 Jul 1947, AMDKU.

[24] Harald Bohr and Jacob Nielsen owned summerhouses on the Island of Als, in the northern part of Schleswig that was re-united with Denmark in 1920 after having been ruled by Germany since 1864. Other Danish and foreign mathematicians often visited Bohr during the summer months while he stayed at the summer house (Ramskov 1995, 224-226).

was a leader of the internationalist wing of the German mathematical community, based in Göttingen, in the early interwar period. He continued his international efforts after his dismissal by the Nazis and his resettlement at New York University, where the institute he helped establish now bears his name (Shields 2016). He visited Bohr from 24-27 July, in conjunction with a tour of German institutions funded by the United States Navy to advise on postwar reconstruction.[25] Indeed, when planning the visit Bohr anticipated that Courant would be his first guest from abroad since the end of the war.[26] Bohr's account to Courant of Schwartz's work must have been impressive, because Courant wrote soon thereafter to Schwartz to remark on Bohr's "enthusiasm" regarding the "strikingly general and illuminating" theory, and to request copies of his papers on the subject for use at his institute in New York.[27]

Schwartz accepted the invitation to Copenhagen on 1 August, writing that "I have received your letter with the greatest pleasure (but your very exaggerated praise makes me blush!). I am naturally very happy to go to Denmark and very flattered by this invitation!"[28] This began a correspondence between Schwartz and Jessen over the details of the trip, with Bohr weighing in from his summer house about the arrangements. We have listed these letters and their topics in the appendix.

Most of the organizational correspondence is as dull as one might expect, but a few matters call for comment. In the letter of 1 August, Schwartz proposed to postpone the visit to the end of October or the beginning of November and in his answer Jessen agreed to arrange the visit in the week from 27 October to 2 November. Schwartz's lectures were originally planned for the Tuesday, Thursday, and Friday of that week, but in the letter of 6 October Jessen informed Schwartz that the plan had to be changed due to a general election that had rather suddenly been called for the Tuesday. This election was caused by the foreign political situation after the war. After the First World War a plebiscite had been held in Schleswig in order to draw a new border between Denmark and Germany. As a result, the northern part of Schleswig was reunited with Denmark in 1920, but some Danes were not satisfied with the outcome and blamed the rules of the plebiscite. In particular, many Danes were disappointed that the largest town Flensburg remained German. After the Second World War Danish nationalists suggested that the border should be changed again, and in 1947 the Prime Minister made such a proposal to the parliament. However, the majority rejected such a measure in a vote of no confidence and so an election was called for 28 October.

Another change was made by Schwartz in his letter of 13 October. He had received a letter from Lars Gårding, a young mathematician at the University of Lund in southern Sweden (just across the Sound from Copenhagen), who had heard of Schwartz's impending visit. Gårding suggested that Schwartz visit Lund, as well, and Schwartz agreed to extend his trip for that purpose. Schwartz's final itinerary is detailed in the appendix.

In his letter of 13 October, Schwartz suggested the following titles for the three talks in Copenhagen:

1. <u>Generalities on the theory of distributions</u>. This talk does not enter into any details and is limited to the statement of the principal results without demonstration.
2. <u>The products of distributions and their application.</u>
3. <u>Fourier transform and harmonic analysis.</u>[29]

[25] Courant to Bohr, 19 May 1947 and 30 Apr 1947, Courant Papers. Courant travel diary, p. 29, Courant Papers, box 25, folder 13.

[26] Bohr to Courant, 13 Apr 1947, Courant Papers.

[27] Courant to Schwartz, 11 Aug 1947, Courant papers, box 79, folder 18.

[28] "J'ai reçu votre lettre avec le plus grand plaisir (mais vos éloges bien exagérés me font rougir!). Je suis naturellement très heureux d'aller au Danemark, et très flatté de cette invitation!" Schwartz to Jessen, 1 Aug 1947, AMDKU.

[29] "1. <u>Généralités sur la théorie des distributions</u>. Cette conférence n'entrerait dans aucun détail et se bornerait à l'énoncé des principaux résultats, sans démonstration.
2. <u>Les produits de distributions et leurs applications.</u>
3. <u>Transformation de Fourier et Analyse harmonique.</u>" Underlining in the original.

Before Schwartz's first talk Jessen announced (in French) that questions would be postponed for an informal gathering on Saturday at 11 AM at the Department of Mathematics. For the last talk, Jessen prepared an effusive note of thanks in French to read at the end, which captures (among other things) the hosts' view of Schwartz's stay in Copenhagen:

> The audience have already expressed by their applause their recognition of the marvelous lectures we have heard.
>
> It remains for me only to express to M. Schwartz a very cordial thanks on behalf of the Ecole polytechnique and the Institute of Mathematics of the University for having made the long voyage to come and explain to us your ideas.
>
> You have done this with such a spirit and such an ardour that we will never forget your lectures. You have made it so that we all now know what a distribution is, and we all have a very clear impression of the many applications that one can make of these new entities in analysis.
>
> Naturally one must accustom oneself to this new technique to be able to apply it, but from what you have said — as well as what you have not said — you have created in us a great curiosity to know more, and we await with great interest the final publication of your lovely research.[30]
>
> For the love with which you have presented your subject, and for all your personality these lectures have been a great inspiration. Many thanks again.[31]

Bohr, for his part, wrote to Courant in the morning before the informal gathering for questions to say "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."[32]

A Scandinavian Somersault: Between Copenhagen and Lund

Schwartz's visit was exceptionally fruitful, in part due to his audience's exceptional preparation. He reported to his wife that he "had an agreeable surprise" on arrival, finding that "Bohr had worked so much with the opuscule from the Annales de Grenoble that it was yellow (the opuscule, not Bohr)[33] and he made it the subject of his course at the Faculty … The students from the University became passionate about the subject and came to speak with me like connoisseurs; I am more and more bashful about my delay [in producing a definitive textbook on distributions]."[34] One of those students, Bent Fuglede, recalled to JL that "I became very occupied with the theory, and during Schwartz's visit to Copenhagen Bohr and Jessen were so kind to

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[30] This refers to the long-delayed textbook that was the subject of much Bourbaki teasing, discussed above.

[31] "Les auditeurs ont déjà par leurs applaudissement exprimé leur reconnaissance des merveilleuses conférences que nous avons écouté.
Il me reste seulement d'exprimer à M. Schwartz les remerciments [sic] très cordiaux de la part de l'École polytechnique et de l'Institut mathématique de l'Université d'avoir fait le long voyage pour venir nous expliquer vos idées.
Cela vous avez fait avec un tel esprit et une telle ardeur que nous n'oublierons jamais vos conférences.
Vous avez obtenu que nous savons maintenant tous ce que c'est, une distribution, et que nous avons une impression très claire des nombreuses applications que l'on peut faire de ces êtres nouveaux dans l'analyse.
Naturellement il faut s'accoutumer à cette nouvelle technique pour pouvoir l'appliquer, mais de ce que vous avez dit - aussi que de ce que vous n'avez pas dit – vous avez crée en nous une très grande curiosité d'en savoir plus, et nous attendons avec la plus grand intérêt la publication finale de vos belles recherches.
Par l'amour avec lequel vous avez présenté votre sujet, et par toute votre personnalité ces conférences ont été une grande inspiration. Encore mille fois merci. " Manuscript preserved at the AMDKU.

[32] Bohr to Courant, 1 Nov 1947, Courant Papers.

[33] This bit of humor is derived from the ambiguity of the French word *il,* which can mean either "it" or "he" depending on context (and which is here translated "it").

[34] "J'ai eu une agréable surprise. Bohr avait tellement travaillé sur l'opuscule des annales de Grenoble qu'il en est jaune (l'opuscule, pas Bohr) et il a fait dessus son cours à la Faculté et... aux professeurs des lycées. Les étudiants de l'université se sont passionnés sur le sujet et sont venus me parler en connaisseurs ; je suis de plus en plus honteux de mon retard." Laurent Schwartz to Marie-Hélène Schwartz, 5 Nov 1947, excerpted in Schwartz 2007, 114.

introduce me to Schwartz.… I got the impression that Schwartz very much appreciated his stay in Copenhagen which ended with a fine dinner at Wivex [in the amusement park Tivoli] arranged by the Danish Mathematical Society (as far as I remember) with many participants, but first of all because of the interests his work had given rise to in Copenhagen."[35]

The pattern of deeply impressing students and young researchers continued across the Sound in Lund, with Schwartz's host Gårding having been particularly shaped by the visit. For Gårding, Schwartz's lectures stood out as a postwar bookend to the equally formative visit of John von Neumann before World War II, both having made "a lasting impression" (Gårding 1998, 230; see also Gårding 1970, 69). As in Copenhagen, the faculty in Lund featured an especially strong group of analysts, led at the latter institution by Marcel Riesz (1886-1969) and Nils Zeilon (1886-1958). On his return to Paris, Schwartz wrote to Jessen to say that the Lund leg of his trip was "also very nice" and that he had spoken in French, with Marcel Riesz summarizing (sometimes at great length) in Swedish after each 20 minute segment of Schwartz's exposition.[36] Also as in Copenhagen, Schwartz ingratiated himself to his hosts with his personality as much as with his mathematics. Jessen had warned Schwartz that Riesz and his students tended to carry their discussions late into the night, and Schwartz confirmed in his 11 November report to Jessen that this had been his experience—with one such exchange lasting until 2 AM. In his autobiography, Schwartz (2001, 302) amplified what they had been doing so late in the night:

> Marcel Riesz was a solid drinker, occasionally pushing to the limits. One night we stayed in his house until two in the morning, with a glass of liquor in front of each of us. His glass kept filling itself. I didn't touch mine, so every time he looked at it, he saw it was full and thought I had served myself. Later he told Gårding that I was an 'excellent drinker'. I remained very close to them.

While undoubtedly impressed, like the others, with Schwartz's personality, Marcel Riesz was less sold on the theory of distributions. Lars Hörmander (1931-2012), who studied under Riesz and Gårding in Lund and who would become known as one of the most important early students of distributions, wrote to Schwartz after reading the latter's autobiography to say it "made me feel a bit guilty." Hörmander explained that he had minimized distributions in his thesis because Riesz, on hearing of his enthusiasm for the theory, gave a variety of objections and told him "that I was still so young that there was hope my taste would improve as I grew up."[37] In their biography of Schwartz, Trèves, Pisier, and Yor tell the following anecdotes:

> In 1948 [sic] Laurent Schwartz visited Sweden to present his distributions to the local mathematicians. He had the opportunity of conversing with Marcel Riesz. Having written on the blackboard the integration-by- parts formula to explain the idea of weak derivative, he was interrupted by Riesz saying, "I hope you have found something else in your life." Later Schwartz told Riesz of his hopes that the following theorem would eventually be proved: every *linear* partial differential equation *with constant coefficients* has a fundamental solution (a concept made precise and general by distribution theory). "Madness!" exclaimed Riesz. "This is a project for the twenty-first century!" The general theorem was proved by Ehrenpreis and Malgrange in 1952. At the end of the twentieth century, there were proofs of it in ten lines." (Trèves, Pisier, and Yor 2003)

Schwartz, for his part, found Riesz's skepticism refreshing. Summing up his trip to his wife, he wrote:

> But I remain very troubled by the excessive success of distributions. The success in Denmark exceeds the permitted bounds. It risks bringing disappointment later! My newborn theory is handsome and well made, it is nice and will perhaps have a beautiful future, but one must let it grow. All the same, it isn't Jesus Christ, and the compliments of the Magi-kings from across the world

[35] Continuation of the e-mail mentioned above (note 21). Schwartz and Fuglede later coauthored a short note (Fuglede and Schwartz 1966).
[36] Schwartz to Jessen, 11 Nov 1947. AMDKU.
[37] Hörmander to Schwartz, 13 May 1999, quoted in Hörmander 2003, 61.

> worry me a little; wasn't he later crucified? (I think thus that Riesz heard tell of Steinhaus and Stone in America, Garding in Sweden, Bohr in Denmark; a Pole in Copenhagen heard of a presentation by Weyssenhoff in Krakow!) The judgement of Riesz is much more reasonable. He is not enthusiastic, advising me to watch out for what is nothing but … wordplay, and to judge in the future; for now he considers it a very beautiful theory, which wants for the effort of becoming deeper over several years, and believes in its success. He is also close with Beurling, who he says thinks the same thing.[38]

Schwartz's return to France offered him a number of opportunities, scientific and otherwise. Among the latter, in his unpublished autobiography Gårding recounted that Schwartz loaded up on "flour, cheese, sugar," and other items to compensate for the fact that "The food situation in France was not yet normalized after the war" (see Gårding n.d.). On his way home, Schwartz continued his scientific discussions with a number of distinguished figures who, by chance, shared his train. As he recounted to his wife:

> Returning from Sweden, I traveled with some physicists (such as Copenhagen quantician Möller [sic]) and Louise [sic] Meitner (universally known German jewess, a rather tender and nice old lady). They were going to Paris for the Rutherford commemoration, and we held a little seminar in wave mechanics. They were violently interested in distributions and wanted to be able to resolve certain mathematical contradictions in w. mech. I absolutely must study that, which might be the most beautiful application of distributions. But these contradictions are horrific to such a point that I can't see how distributions manage to perfect them better.[39]

So Schwartz found himself in the company of some of the leading physicists of the time: Lise Meitner (1878-1968) from Stockholm's Högskola, co-discoverer of fission, Christian Møller (1904-1980) from the Niels Bohr Institute, Copenhagen University, well known for his work on relativity and quantum physics, and George Hevesy (1885-1966) who had moved from the Niels Bohr Institute to Stockholm in 1943, the same year he was awarded the Nobel Prize for his discovery of the element Hafnium (named after Copenhagen), as well as the younger Cécile DeWitt-Morette (1922-2004) who went to Princeton the following year and later became famous for her foundation of an influential summer school at Les Houches in the French Alps. While Schwartz was aware from an early stage of the connection between distributions

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[38] Laurent Schwartz to Marie-Hélène Schwartz, 5 Nov 1947, excerpted in Schwartz 2007, 117-118. "Mais je reste très troublé du succès excessif des distributions. Le succès au Danemark dépasse les bornes permises. Cela risque d'amener des déceptions plus tard ! Mon nouveau-né est beau et bien fait , il est sympathique et aura peut-être un bel avenir, mais il faut le laisser grandir. Ce n'est tout de même pas Jésus-Christ, et les compliments des rois Mages venus de toute la terre m'inquiètent un peu ; s'il est plus tard crucifié ? (Pense donc que Riesz en a entendu parler par Steinhaus et Stone en Amérique, Garding en Suède, Bohr au Danemark ; un polonais de Copenhague en a entendu un exposé de Weyssenhoff à Cracovie !) Le jugement de Riesz est beaucoup plus raisonnable. Il n'est pas enthousiaste, me conseille de me garder de ce qui n'est que … jeu de mots, et jugera dans l'avenir ; il considère seulement dès maintenant que c'est une très belle théorie, qui vaut la peine d'être approfondie pendant quelques années et croit au succès. Il est aussi intime de Beurling, qui pense dit-il la même chose."

[39] Letter from Laurent Schwartz to Marie-Hélène Schwartz, November 5, 1947. Quoted in Schwartz 2007, 116. "Au retour de Suède, j'ai voyagé avec des physiciens (dont Möller [sic], quanticien de Copenhague) et Louise [sic] Meitner (juive allemande universellement connue, une vieille femme bien tendre et sympathique). Ils allaient à Paris pour la commémoration de Rutherford, et nous avons fait un petit séminaire de mécanique ondulatoire. Ils sont violemment intéressés par les distributions et espèrent pouvoir résoudre par là certaines contradictions mathématiques de la Méca ondul. Il faut absolument que j'étudie cela, ce pourrait être la plus belle application des distributions. Mais ces contradictions sont horrifiantes à un tel point que je ne vois pas comment les distributions les mieux perfectionnées y parviendraient." We include here, for reference, Schwartz's briefer summary of this encounter to Jessen (11 Nov 1947, AMDKU): "Even my return trip to France was not a waste of time. In the same train were Lise Meitner, Møller, Hevesy who we going to Paris for the commemoration of Rutherford, and Miss Morette; we were able to discuss well about certain equations of the wave mechanics (difficult to justify!)"

and the Dirac calculus, this appears to us as the start of what would be a prolonged engagement with distributions in the finer points of quantum physics. That program resulted, some years later, in Arthur Wightman and Gårding's celebrated distribution-theoretic foundation for quantum mechanics (see Wightman 1996).

The voyage to Copenhagen and Lund opened up, for Schwartz, a wide vista of international science. He declared to Jessen that "I have returned absolutely delighted by this trip. Delighted with the extended scientific contacts as well as contact with the Danish life and manners. I told you that I had made pretty much my first trip abroad; but now I desire to continue!"[40] Indeed, the trip made such a strong impression on Schwartz that, in his autobiography, it grew from a single week to a three week experience:

> I received an invitation to Copenhagen to give a series of lectures in October 1947…. It was my first really important university trip, and also one of my first escapades. I stayed in Copenhagen for three weeks, and was received there with the famous Danish hospitality. Memories of the war were still vivid in 1947, and everything appeared wonderfully fresh to me. (Schwartz 2001, 301)

Taking Flight: Distributions' Transatlantic Transmission

Schwartz had not even left Copenhagen for Lund when his Danish hosts began planning to spread the gospel of distributions beyond Europe. Bohr's remarks to Courant about Schwartz's theory and personality, quoted above, came in the context of Courant's invitation for Bohr to visit him in the coming year. In that same letter, Bohr listed Schwartz's theory among a number of prospective topics for lectures at Courant's institute at New York University. Bohr made plans on the same trip to lecture at other locations as well, including the Institute for Advanced Study in Princeton, NJ, Norbert Wiener's Massachusetts Institute of Technology, Brown University (home to contacts including Otto Neugebauer), and McGill University in Montreal (where Tornehave worked at that time).[41] As he explained to his former student Asger Aaboe, Bohr intended from the start to make distributions a centerpiece of his trip: "We have had several extremely interesting visits by foreign mathematicians, in the first line the young French mathematician Prof. Laurent Schwartz; I intend to propagandize strongly for his eminent contribution to the classical differential and integral calculus in the United States, which however may prove unnecessary."[42]

Despite the handful of Americans who saw Schwartz in Nancy, Bohr's U.S. propaganda tour from March to May of 1948 (with a stopover just north of the border in Montreal) featured, so far as we can determine, the first lectures in North America on distributions by anyone with firsthand acquaintance with Schwartz. This is not to say Schwartz lacked for influential second-hand advocates, including those noted above about whom he heard on his Copenhagen-Lund trip. The globetrotting Marshall Stone, in particular, took Schwartz's theory as far as Madras, India, in 1949 (Stone 1949). His remarks at a Symposium on Harmonic Analysis there appear to be based on Schwartz's first brief article on distributions. Stone went on to teach one of the first North American courses on distributions, at the University of Chicago in 1951-1952, based in part on Bourbaki collaborator Claude Chevalley's slightly earlier course at Columbia University.[43]

Nonetheless, there was something distinctive in Bohr's presentation, and the special zeal he showed for the theory of distributions. Tornehave reported on the scene in Montreal:

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[40] "Je reviens absolument enchanté de ce voyage. Enchanté du contact scientifique prolongé aussi bien que du contact avec la vie et les mœurs danoises. Je vous ai dit que je faisais à peu près mon premier voyage à l'étranger; mais j'ai maintenant le désir de continuer!" Schwartz to Jessen, 11 Nov 1947 AMDKU.

[41] See Bohr to Veblen, 1 Nov 1948; Bohr to Veblen, 31 Jan 1948, IAS Mathematics, box 3, folder 3; Bohr to Courant 17 Jan 1948, Courant Papers.

[42] Letter H. Bohr to Asger Aaboe, Nov 1947, translated in Lützen 1982, 160. JL made this English translation in the 1970s when visiting Asger Aaboe at Yale University. We do not know if the letter has been preserved since then.

[43] Stone Papers, box 17, "Course on Distributions" folder. See Barany 2016a, 289-290.

> …we were gathered at the old professor Williams at McGill University. In Professor Williams's apartment where there were no special mathematical facilities, Harald Bohr told the entire party about distributions. He wrote with large letters on large sheets of paper so that everyone could see it, one sheet after the other, and everyone listened fabulously attentively. (Ramskov 1995, 365; cf. Tornehave 1985, 22)

Bohr's evangelism was also distinctive for his twinned efforts to promote *both* distributions *and* their inventor. Inspired by his conversations with Bohr, Courant wrote to Schwartz in April and noted Bohr's gripping account of "your beautiful theory of differentiation," adding that "Harald was very enthusiastic about the idea of your visiting this country, and he thinks much mutual advantage could come of it."[44]

At the time of Bohr's visit to Montreal, Williams was in the midst of planning for the second Canadian Mathematical Congress, which would take place in Vancouver in the summer of 1949. Williams was keen to invite Bohr to be the Congress's featured speaker, but Bohr instead used the opportunity to promote both Schwartz and Jessen.[45] From Williams's subsequent attempts to learn more about Schwartz, we can infer that Bohr may have been one of Williams's sole sources of information about the French mathematician.[46] But Bohr's good word was enough, and Schwartz was invited not only to address the main Congress but also to offer a short course on distributions for university researchers and graduate students in advance of the meeting. Jessen, for his part, gave a lecture during the Congress itself, and spent most of the rest of the 1949 calendar year in the United States (principally at the University of Chicago in the spring and the Institute for Advanced Study in the fall).[47]

In Vancouver, Schwartz met for the first time Paul Dirac, for whose eponymous calculus Schwartz claimed to supply a foundation. The repercussions of that meeting, so far as we can tell, were relatively minor. More significantly, the meeting represented Schwartz's first scientific trip outside of Europe, as well as his first journey by airplane, which took him (over many short hops) between Paris and Montreal, followed by a transcanadian train trip (Schwartz 2001, 305-307). That particular itinerary was not, in fact, Schwartz's intended one. Like Jessen, Schwartz attempted to build out his 1949 itinerary with invitations to American institutions including Chicago (via Stone), the IAS (another outgrowth of Bohr's promotion), Harvard, and Courant's institute. Unlike Jessen, however, Schwartz had to abandon the U.S. portion of his travels due to visa complications that would metastasize into an infamous scandal in the latter months of 1949 and the first half of 1950 (Barany 2016a, 228-245). The contacts he made in the process of those failed arrangements, however, were not for nothing. Indeed, initial discussions regarding these invitations occurred at the very time Schwartz was being considered (by some of the same people) for a prestigious invitation as an hour speaker for the 1950 International Congress of Mathematicians (see Barany 2016a, ch. 4), and may well have contributed to his selection.

Schwartz had lectured once previously in an anglophone setting, having visited London in January 1948 on an invitation precipitated by the Nancy Congress. His linguistic struggles in Copenhagen motivated him to improve his English (Schwartz to Jessen, 11 Nov 1947, AMDKU), and he prepared for his London trip by translating his talk and rehearsing it extensively. "My first talk in London went perfectly" he recalled in his autobiography. "I barely needed to read my text, which I practically knew by heart. Everyone understood me perfectly! Alas, at the question session afterwards, I could not understand a single thing that was said to me" (Schwartz 2001, 303). The long train trip from Montreal to Vancouver and the stay in

[44] Courant to Schwartz, 26 Apr 1948, Courant Papers, box 79, folder 18.
[45] On Williams' invitation of Bohr, see Bohr to Coxeter, 20 Oct 1948, Coxeter Papers, box 3, file 6.
[46] Williams to Kline, 2 Aug 1949, AMS Records, box 36, folder 4.
[47] CMC 1951; Bohr to Courant, 4 Jan 1949; Jessen file, IAS Members, box 67; Jessen-Courant correspondence in Jessen Papers, box 12; Jessen-Stone-Albert correspondence in Jessen Papers, box 23.

Vancouver itself, however, gave Schwartz an extended and more relaxed setting to improve his English, though it never reached a level with which Schwartz was entirely comfortable (ibid., 306).

The Vancouver lectures themselves helped open up Schwartz to a wider anglophone audience. His lecture notes were distributed in English alongside an English translation of a recent article written for French electrical engineers introducing the theory (Courant Papers, box 79, folder 19). On the basis of these lectures, the young Canadian mathematician Israel Halperin produced a short pamphlet introducing the theory of distributions to mathematical physicists, which appears to have circulated fairly well in North America (Halperin 1952). Between 1947 and 1949, both directly and indirectly, Bohr thus brought Schwartz and distributions across an ocean. All this made way for Schwartz's definitive landing one year after the Vancouver meeting, at the 1950 International Congress of Mathematicians.

The Landing: The 1950 International Congress of Mathematicians and the Fields Medal

Even before Schwartz left Paris for Vancouver, he would have learned of his invitation as a foreign hour speaker in analysis at the 1950 International Congress of Mathematicians, at Harvard University in Cambridge, Massachusetts. Bohr contributed indirectly to this invitation by trumpeting Schwartz to the mathematicians of the Institute for Advanced Study, including the chair of the committee to select foreign hour speakers in analysis, Marston Morse. Bohr had one more intervention in Schwartz's favor yet to make, however, and it was perhaps his most decisive.

Much about the Fields Medals is shrouded in secrecy.[48] We still do not know, for instance, how exactly Harald Bohr came to chair the committee to select the 1950 medalists, though his personal friendship with the Congress's president Oswald Veblen and his broad appeal in the international mathematical community no doubt contributed to the appointment. Fortunately for the historical record, as Barany and Lützen recently discovered, Bohr could not restrain himself from gossiping about the deliberations in letters to Jessen. Even more fortunately, as Barany discovered even more recently, 1936 Fields Medalist and 1950 committee member Lars Ahlfors allowed his department staff to mingle his set of confidential committee correspondence with other files retained for the International Congress's organization (in which the Harvard mathematics department played a central role for geographical and institutional reasons). These two sets of documents permit an unprecedented insight behind the curtain of what has become mathematicians' most iconic prize.

In November of 1948, the American organizers of the International Congress sent letters to Harald Bohr, Lars Ahlfors, Karol Borsuk, Maurice Fréchet, William V. D. Hodge, Andrey N. Kolmogoroff, Damodar D. Kosambi, and Marston Morse informing them that the Congress's president (Oswald Veblen), organizing committee chair (Garrett Birkhoff), and Secretary (John R. Kline) had appointed them to the Fields Medal committee. Each committee member received a copy of a memorandum from Fields himself laying out the goals of his proposed medal, which was created posthumously in his name in 1932 and first awarded in 1936. None of the committee members was among those appointed in 1936 to select the subsequent pair of medalists.[49] Deliberations began by postal correspondence in January of 1949, at the same time as other committees for the Congress (including others on which Ahlfors and Morse served) deliberated over whom to invite as speakers.[50] Owing to the delays of international mail, however, the Fields committee

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[48] Riehm and Hoffman (2011) provide a thorough synthesis of extent scholarship along with original findings from the archives of Fields and a few others.

[49] Schieldrop 1937, 48. That committee consisted of Hardy (chair), Alexandroff, Hecke, Julia, and Levi-Civita, with Lefschetz and Nevanlinna as alternates.

[50] Bohr to Committee, 21 Jan 1949, Harvard Mathematics. On the hour speaker deliberations, see Barany 2016a, ch. 4. Though Marshall Stone, discussed above, was both active in international mathematics and an early promoter of distributions, he did not participate directly in the Fields Medal discussions. His closest influence may have been to

continued to deliberate long after the spring and early summer, when most of the speaking invitations had been settled by domestic committees of the American Mathematical Society. Some of the delay came from Bohr's decision to wait for a reply from Kolmogoroff, who ultimately did not participate in deliberations, before moving ahead without him in June. Kolmogoroff's non-participation was symptomatic of the Congress organizer's general difficulties in contacting Soviet mathematicians (see Barany 2016a, ch. 3).

Even before Bohr formally initiated the committee's discussion in January, 1949, he exchanged letters with Morse and Fréchet about how to limit the potentially large field of candidates. This was to be, one must recall, only the second ever cohort of Fields Medals, and the committee had little to draw upon beyond the vague guidance of Fields's proposal, a copy of which was enclosed with their letters of appointment to the committee.[51] It is not even clear whether the committee considered the report of the 1936 committee chair Constantin Carathéodory: though members would have heard it at the 1936 Congress and been able to read it in the proceedings (Carathéodory 1937), no one referred to it in the course of the committee's deliberations. In any case, Carathéodory's remarks were focused on the medalists' work and did not offer explicit guidance to future committees. Bohr, Morse, and Fréchet began to think that they might narrow the field by considering only young candidates who had distinguished themselves since the previous Congress.[52] They did not, however, agree on just what would qualify as young (the current age limit of 40 was not codified until 1966), and with the previous Congress more than a decade in the past the latter criterion was only somewhat helpful.

Bohr made youth a central theme in his first formal letter to the committee, where he solicited views on procedure as well as proposals for candidates and mathematical fields to consider.[53] Youth was, again, a focus of Bohr's next official committee letter in June, but now it had a more specific instrumental purpose for the chair.[54] Most committee members took Bohr's hint to focus on younger mathematicians, and among the initial names they proposed the two candidates to be mentioned by multiple committee members were Laurent Schwartz and his fellow Bourbaki collaborator André Weil. Though he did not write this explicitly, there can be no doubt that Bohr recognized two basic realities in this pairing: they could not ultimately give the medal to two French mathematicians, especially two so closely linked; and of the two, Weil was undoubtedly the more accomplished. Instead, Bohr used the question of age to attempt to rule Weil out of consideration. Latching onto Morse's two proposed candidates, Bohr declared "Selberg and Schwartz" to be "exceptionally natural" representatives of the younger set of nominees, while separating out Weil (who, born in 1906, was just shy of nine years Schwartz's senior) as someone who "even if he is still rather young" seemed to Bohr "in this special connection as too 'old' and, may I say, too generally recognized." Though Weil was only a few years older than Jesse Douglas had been at the time of his 1936 medal, and had certainly won the bulk of his fame in between the last Congress and the upcoming one, Bohr appeared determined to use age to drive a wedge between the two French frontrunners so that his favored star Schwartz could rise above the fray.

Once again, to judge by the names suggested, most of Bohr's committee took the hint and, apart from Borsuk, each named either or both of Schwartz and Selberg in the next round of nominations, which Bohr summarized to the committee at the end of September.[55] Weil continued, however, to hold a strong

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reinforce Schwartz's merit to Ahlfors and Morse as a participant on the committee to select foreign hour speakers in analysis.

[51] E.g. Kline to Ahlfors, 9 Nov 1948, Harvard Mathematics.

[52] Fréchet to Morse, 14 Nov 1948, Morse Papers, box 6. Morse to Bohr, 23 Nov 1948; Morse to Fréchet, 23 Dec 1948, Harvard Mathematics. At least one further letter (Bohr to Morse, 16 Nov 1948) is referenced in this correspondence but does not appear to have been preserved.

[53] Bohr to Committee, 21 Jan 1949, Harvard Mathematics.

[54] Bohr to Committee, 1 Jun 1949, Harvard Mathematics.

[55] Bohr to Committee, 30 Sep 1949, Harvard Mathematics.

position, topping the lists of Fréchet, Hodge, and Kosambi. In that letter, Bohr reiterated at multiple points his emphasis on Schwartz and Selberg, and even tested out some formulations he would later use in his official medal presentation eleven months later, that "both of these two young mathematicians have at the same time given most important contributions to classical mathematical problems and opened new most promising lines for investigation in the future." Hodge wrote particularly forcefully in Weil's favor, while Ahlfors emerged as his leading opponent. Ahlfors objected to Weil not on the grounds that he was undeserving, but rather that he was *too* deserving, and that selecting Weil would make it appear (erroneously) that the committee had simply tried "to make a ranking list of the world's mathematicians and give awards to the top men on the list." That is, the hypothesis that most mathematicians would consider Weil to be among the two best in the world while few would say the same of Schwartz and Selberg here ruled Weil out of consideration for a prize that is now often regarded as recognizing some of the world's leading mathematicians. Attempting to close in on a consensus that would leave Schwartz in and Weil out, Bohr closed his September letter by asking committee members to say whether they were comfortable selecting Schwartz and Selberg as the medalists, or whether they wished to propose any others.

Bohr's hopes for a quick conclusion proved premature. Hodge wrote in response that, Ahlfors's objections notwithstanding, Weil's merits were such that the committee "might be shirking our duty" by denying him a medal.[56] Were Weil to be ruled out, Hodge hoped Oscar Zariski (who was older than Weil but regarded as having come to prominence more slowly) would be considered as someone who "has done more for geometry than anyone in the past fifteen years." Enclosing Hodge's letter in full to the committee and excerpting another set of reservations from Fréchet, Bohr tried again in October to close the argument for Selberg and Schwartz as "both being what may be called really young mathematicians who have given contributions of exceptional importance to old classical problems and at the same time opened the way for most fruitful further research."[57] In an aside to Jessen, Bohr forecast that it "will probably end with Selberg and Schwartz" but that matters were "a bit confused."[58] Citing strong opinions on both sides of the Weil question, Bohr supposed that "you know exaggerations can sometimes help by canceling each other out." As more responses arrived, Bohr quipped to Jessen that the medal "Ends probably with Schw. and Selb.; but it will require blood and tears."[59]

Indeed, once more, Bohr failed to cement his consensus. Ahlfors, for his part, was swayed by Hodge and proposed giving medals to all four of Selberg, Schwartz, Zariski, and Weil.[60] In his November letter to the committee, Bohr firmly rebutted Ahlfors's suggestion of four medals and summarized the range of responses to his previous letter so as to put Schwartz (as well as Selberg) in the best possible light.[61] Kosambi, like Hodge, continued to insist on the case for Weil, though Bohr did not share with the committee (but did share with Jessen) that Kosambi considered it "ridiculous" if a medal did not go to the older Frenchman.[62] Analyzing the responses as a whole, Bohr again attempted to mollify the Weil camp by arguing for age as a blunt desideratum to avoid having to weigh not just Weil but a large number of other "distinguished mathematicians of [Weil's] age and acknowledged standard." Responding to Kosambi's reservations, Bohr then launched a detailed defense of Selberg's merit before asking the committee, yet again, to endorse Schwartz and Selberg as the medalists.

56 Hodge to Bohr (copied to committee), 7 Oct 1949, Harvard Mathematics.
57 Bohr to Committee, 18 Oct 1949, Harvard Mathematics.
58 Bohr to Jessen, 24 Oct 1949, AMDKU.
59 Bohr to Jessen, 6 Nov 1949, AMDKU.
60 Ahlfors to Bohr, 1 Nov 1949, Harvard Mathematics.
61 Bohr to Committee, 23 Nov 1949, Harvard Mathematics.
62 Bohr to Jessen, 24 Oct 1949, AMDKU.

This time, Bohr's advocacy had the intended effect. In early December, he updated Jessen with the news that "Fields Medals (confidential) run now certainly to Selberg and Schwartz, I must prepare a report (and in fact look forward to it)."[63] With that report, Bohr made sure that the selection really counted. He did this in part by praising the medalists' work, but he also made a special effort to promote the medal itself. Indeed, the Fields Medal in 1950 was not an especially well-known prize even among mathematicians, and it would take more than a decade before it approached anything like the regard it generally holds today (Barany 2015). Thus, proclaiming the importance of the Fields Medal was part and parcel of Bohr's effort to promote Schwartz and distributions.

The committee, Bohr reported at the opening ceremony of the International Congress, had decided to follow the example of the previous committee and award the prize to "two really young mathematicians, without exactly specifying however, the notion of being 'young'" (Bohr 1952, 127). After his introductory remarks Bohr, introduced the work of Selberg. Having himself done substantial research on Riemann's ζ-function, Bohr set Selberg's results into perspective. In order to highlight the surprising nature of Selberg's elementary proof of the prime number theorem he quoted Hardy's opinion, expressed in a talk given in 1921 in Copenhagen, to the effect that it was very unlikely that such a proof would ever be found. But there was a clear gap in enthusiasm between Bohr's celebration of Selberg and his praise of Schwartz, which ran about twice as long in the printed proceedings. Bohr declared:

> … one of the greatest merits of Schwartz's work consists … in his creation of new and most fruitful notions adapted to the general problems the study of which he has undertaken. While these problems are themselves of classical nature, in fact dealing with the very foundation of the old calculus, his way of looking at the problem is intimately connected with the typical modern development of our science with its highly general and often very abstract character. Thus once more we see in Schwartz's work a confirmation of the words of Felix Klein that great progress in our science is often obtained when new methods are applied to old problems. (Bohr 1952, 130)

Bohr continued with a very pedagogic and precise introduction to distributions. One can hear his excitement when he declared: "The new notion – once invented – is so easy to explain that I cannot resist the temptation, notwithstanding the general solemn nature of this opening meeting, to go into some detail" (Bohr 1952, 131). He called Schwartz's first paper on the theory of distributions (Schwartz 1945) "one of the classical mathematical papers of our times." His attribution of the paper to 1948 suggests he was referring to the paper's review in the *Zentralblatt für Mathematik,* where it was reviewed jointly by Gustav Doetsch alongside Schwartz's *Annales* article (dated 1947) based on his Nancy talk, which appeared in 1948 and was given that date in the *Zentralblatt.* (Since he read the original paper in the summer of 1947, Bohr's 1948 attribution is otherwise inexplicable.) Bohr added,

> I think that every reader of this cited paper, like myself, will have felt a considerable amount of pleasant excitement, on seeing the wonderful harmony of the structure of the calculus to which the theory leads and on understanding how essential an advance its application may mean to many parts of higher analysis, such as spectral theory, potential theory, and indeed the whole theory of linear partial differential equations… (Bohr 1952, 133).

He compared Schwartz's generalization of the concept of functions to the generalization of the real numbers to the complex numbers and pointed out that the aim of any such generalization "is, in principle, the same and of a double kind; on the one hand, and this is the primary purpose, one aims at getting simplifications in the treatment of problems concerning the old notions through the greater freedom in carrying out the operations, provided by the new notions, and on the other hand one may hope to meet with new fruitful problems concerning the new notions themselves. In both these respects the theory of Schwartz may be said

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[63] Bohr to Jessen, 3 Dec 1949, AMDKU.

to be a great success" (Bohr 1952, 133). He mentioned how distributions could justify "different 'symbolic' operations often used in an illegitimate way by the technicians" and expressed the thought that "distributions may find their place even in the more elementary courses of the calculus in universities and technical schools" (Bohr 1952, 133).

At the end of his presentation of Schwartz's work, Bohr mentioned that Schwartz was preparing a "larger general treatise on the theory of distributions, the first, very rich, volume of which has already appeared." In particular, Bohr referred to the introduction in which Schwartz "emphasizes the fact that ideas similar to those underlying his theory have earlier been applied by different mathematicians to various subjects – here only to mention the methods introduced by Bochner in his studies on Fourier integrals – and that the theory of distributions is far from being a 'nouveauté revolutionaire.' Modestly he characterizes his theory as 'une synthèse et une simplification'" (Bohr 1952, 133). But Bohr made sure to emphasize, somewhat hyperbolically, the value he saw in such syntheses and simplifications. "However," he concluded,

> as in the case of earlier advances of a general kind – to take only one of the great historic examples, that of Descartes' development of the analytic geometry which, as is well known, was preceded by several analytic treatments by other mathematicians of special geometric problems - 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. No wonder 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. (Bohr 1952, 133)

Schwartz had been invited by the organizing committee of the International Congress to give a plenary lecture. In the program it was announced with the title "Distributions and principal applications," which gives the impression of an introductory elementary talk (Schwartz 1952, 220, footnote). However, in fact, he gave a rather technical talk focused on some recent findings related to his theory. There may be several reasons for this change of tack. First and foremost, Harald Bohr's talk had already explained the basic ideas of distribution theory to the audience. And indeed Schwartz began his paper with the declaration: "We assume that the fundamentals of the theory of distributions are known." Schwartz may also have felt some pressure to show the power of his theory beyond the elementary facets from Bohr's exposition, and he had a brand new result that fit the bill: his so-called kernel theorem (see Paumier 2014, ch. 4, pp.139-175).

This theorem was another example of the fact, emphasized by Bohr, that problems in classical analysis can be simplified by the new theory. Indeed, in classical analysis it was well known that a locally summable function $K(x,y)$, called the kernel, defines an linear operator mapping a function f with compact support into a locally summable function g defined by:

$$g(x) = \int_{\mathbb{R}} K(x,y)f(y)dy.$$

However, not all operators can be written in this way as an integral operator. For example the identity operator cannot be represented in this way. However, and this is the content of the kernel theorem, if one allows the kernel to be a distribution, one can in a sense represent all linear operators from D to D' as integral operators. For example the Dirac function δ gives rise to a kernel $\delta(x-y)$, which acts as the identity operator. In order to make this theorem precise one has to interpret the integral in a suitable way. In a sense this theorem provides a bridge between Schwartz's early theory of convolution operators and his later theory of distributions, showing that operators can be represented by distributions.

Conclusion

Paumier and Barany have discussed in detail elsewhere how the International Congress launched Schwartz to widespread acclaim and generated a sharp increase in international interest in his theory across Europe, North America, South America, and beyond (Barany 2016a, ch. 5; Barany 2016b; Paumier 2014, ch. 2, 3, 4).

As noted, Schwartz himself was aware of the crucial progression from Nancy to Copenhagen to the International Congress that marked his and his theory's embrace on the world stage. In the present article, we have assembled the historical evidence related to that sequence so as to emphasize the direct connections and consequences that made it possible and significant. What looks at first like an inevitably growing recognition of a brilliant theory begins to show, on closer examination, the crucial importance of individual circumstances, of being in the right place at the right time, and of having the right allies to advocate and promote ideas and careers.

The most important such ally, we have found, was Danish mathematician Harald Bohr, who has before our investigations been seen as a relatively minor player in the story of Schwartz and distributions (when he has been noticed at all). We have shown that after their initial Bourbaki-mediated encounter in Nancy, Bohr actively and deliberately promoted Schwartz and his theory across Europe and North America. His interventions helped both the mathematician and the theory travel across borders and oceans, allowing Schwartz to emerge at the start of the 1950s with a large and enthusiastic international audience for his mathematics.

Bohr's significance here is only magnified if one considers that, by the time of the 1950 International Congress, he knew his remaining time in this world to be short.[64] When Bohr crossed the Atlantic to deliver his address praising Schwartz, he did so believing (correctly, as it turned out) that it would be his final time on those shores and his last opportunity to intervene in his field's international professional community. As Barany has argued, it seems likely that Bohr's elevation of Schwartz was a deliberate effort to anoint a standard-bearer for postwar international mathematics (Barany 2016a, 1-3). Both man and theory, we have seen, gave Bohr hope for the discipline's future.

If Bohr is our story's exceptional figure, we must also stress what our account shows about the rules of postwar international mathematics. As local, regional, and national mathematical communities rebuilt institutional infrastructures on a variety of scales in the immediate wake of the second World War, those who benefited most from these refashioned arrangements were those with robust connections to the few people and institutions who continued to command significant resources, prestige, and academic authority. Private foundations—the American Rockefeller Foundation and Danish Carlsberg Foundation in particular—were especially important change-points for the conversion of personal and professional connections and authority into resources and infrastructures. Also crucial were the credentials that a few (Harald Bohr, most importantly, among them) built from working effectively and largely above the fray during the contentious interwar period of international mathematics. As Barany has argued (Barany 2016b), the very early postwar reconstruction of international mathematics makes clear the professional and institutional ramifications of a deeply personal discipline at its most personal.

Finally, we have shown a connection between the specific content and claims about Schwartz's mathematics and his international ascent that is different from what one might ordinarily suppose. In contrast to accounts that see the essential features of the theory of distributions to have been sorted out in 1945-1947 to have won Schwartz widespread recognition in their single worked-out form, we observe that the theory continued to change as Schwartz found new audiences in 1947-1950 and that the theory took different forms in different contexts. In particular, Schwartz's success in 1947 rested significantly on his ability to make a specific intervention in Fourier analysis that interacted and productively contrasted with other interventions at the Nancy colloquium. He developed his spherical or tempered distributions well after setting the basic definitions for his theory, though in his memory he later yoked those two developments more closely together. Bohr's interpretation of the theory, moreover, was the version that spread farthest and quickest in the pivotal years of Schwartz's rise, and Bohr's theory was reducible neither to the version of Schwartz's

[64] Jessen to Courant, 30 Jan 1951, Courant Papers box 55 folder 2.

1945 or 1947 papers, nor to some other self-contained statement from Schwartz. Lastly, Schwartz's own presentation at the 1950 International Congress of Mathematicians represented a self-conscious departure from Bohr's framing, emphasizing instead distributions' primordial origins in the theory of linear operators.

From Nancy to Copenhagen to the world, the itinerary of Schwartz and his theory between 1947 and 1950 remains a remarkable episode in the history of mathematics. Rarely does a newborn theory find so grand a stage so quickly and thoroughly. We have here looked beneath the theory itself to the complex interpersonal stagecraft that brought it to prominence. On close analysis, one finds a seemingly miraculous ascent to have been driven by far more people, institutions, chance encounters, and long-brewing designs than first meet the eye.

Appendix: Copenhagen Correspondence and Itinerary

Jessen and Bohr to Schwartz, 14 July, 1947

Copenhagen, July 14, 1947

Dear Professor Schwartz,

We would have written to you at our return to Copenhagen to thank you and Mrs. Schwartz most heartily for all your kindness and hospitality during our stay in Nancy; but we have waited until we could give the invitation to you to visit Copenhagen of which we spoke more informally in Nancy a more definite form.

Now we are most happy to do so; all our colleagues at the University and at our Ecole Polytechnique, got enthusiastic about the possibility of seeing you here and of hearing some lectures of you on your extraordinary theory of distributions. Since this theory is of so great interest both in pure and applied mathematics our Mathematical Institute of the University has joined with the Mathematical Institute of the Ecole polytechnique (of which our colleague Rich. Petersen is the director) in the invitation. We hope very much that you will be able to follow our invitation and come to Copenhagen for instance some time in September and give three lectures on your theory. From your lecture in Nancy we got a strong impression of your extraordinary gift for clear and simple exposition; nevertheless we wish to emphasize that our mathematical circle is rather heterogeneous and that it would therefore be very welcome if you would make the exposition as elementary as possible and not suppose any special knowledge of the audience. If it would suit you we would propose that the first lecture should be of a more general nature giving a survey of your theory and its applications intended for a somewhat larger audience while the more detailed exposition could be postponed to the other lectures.

To cover the expenses for your travel and stay here we have received grants to the amount of 1000 danish [sic] crowns. We do not know whether it will be possible for you to take danish Crowns out of Denmark and it has not much sense to find out the present rules since they may be changed before September, but we are confident that we shall be allowed by the danish authorities to send you the money to cover the travel expenses (aller et retour) in french currency. The expenses for a stay of (say) a week here will probably not be more than 200 crowns. We would be grateful to hear at an early date how much money you would wish to have transferred to France.

Our address during the vacation is Fynshav, Als, Denmark which in the summer is an accumulation point of mathematicians (J. Nielsen, Fenchel, Siegel and perhaps Courant and Turan, besides ourselves).

With the best wishes, also to Mrs. Schwartz, and looking forward to your visit,

Sincerely yours,

The following letters are preserved in the Archive of the Department of Mathematical Sciences at the University of Copenhagen (letters from Jessen in copies). Schwartz's letters are all written in French, Jessen's all in English except the last one.

14 July: Bohr and Jessen to Schwartz (quoted above).

1 August: Schwartz to Jessen, about dates and transfer of money.
12 August: Jessen to Schwartz, about dates and money.
17 August: Schwartz to Jessen, about money matters.
6 October: Jessen to Schwartz, about dates and time of talks; Schwartz is invited to stay with Jessen and his family.
13 October: Schwartz to Jessen, accepts offer to stay with Jessen; about trip to Lund and exact titles of talks.
20 October: Jessen to Schwartz, about schedule and return voyage to France, the only letter from Jessen in French.
11 November: Schwartz to Jessen, thanks for the visit; about his stay in Lund and return trip.

Final itinerary, 1947.

Sunday	26 Oct. Arrival in Copenhagen, 8pm.
Monday	27 Oct. General talk, 4pm.
Tuesday	28 Oct. General election.
Wednesday	29 Oct. Talk, 4pm.
Thursday	30 Oct. Talk, 4pm.
Friday	31 Oct. Meeting at the Royal Danish Academy of Sciences and Letters (with an address by Harald Bohr).
Saturday	1 Nov. Schwartz answers questions, 11am.
Sunday	2 Nov. To Lund.
Monday	3 Nov. Talk in Lund.
Tuesday	4 Nov. Talk in Lund.
Wednesday	5 Nov. Departure for France.
Thursday	6 Nov. Back in France.

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Author Biographies
Michael J. Barany is a postdoctoral fellow in the Dartmouth College Society of Fellows. In 2016, he defended his PhD in Princeton University's Program in History of Science on *Distributions in Postwar Mathematics,* thus joining his esteemed coauthors in the select company of those awarded doctorates on the basis of studies of Laurent Schwartz and the theory of distributions. Details of his publications and research interests can be found at http://mbarany.com.

Anne-Sandrine Paumier is a postdoctoral researcher at the Institut des Hautes Etudes Scientifiques (France). Her current studies of the development of mathematics at the IHES and on the Plateau de Saclay extend her investigations into the history of twentieth-century French mathematics contained in her PhD, *Laurent Schwartz and the collective life of mathematics*, and several articles including, most recently (with D. Aubin) "Polycephalic Euclid? Collective aspects in the history of mathematics in the Bourbaki era", in Sørensen, Remmert, and Schneider (eds) *Historiography of Mathematics in the 19th and 20th Centuries*.

Jesper Lützen is professor of history of mathematics at the Department of Mathematical Sciences, University of Copenhagen. His first research monograph, *The Prehistory of the Theory of Distributions* (Springer, 1982), introduced him to the subject of the paper in this volume. His last book *Mechanistic Images in Geometric Form* (Oxford University Press, 2005) dealt with Heinrich Hertz's Principles of Mechanics. He is currently doing research on the history of mathematical impossibility theorems.

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