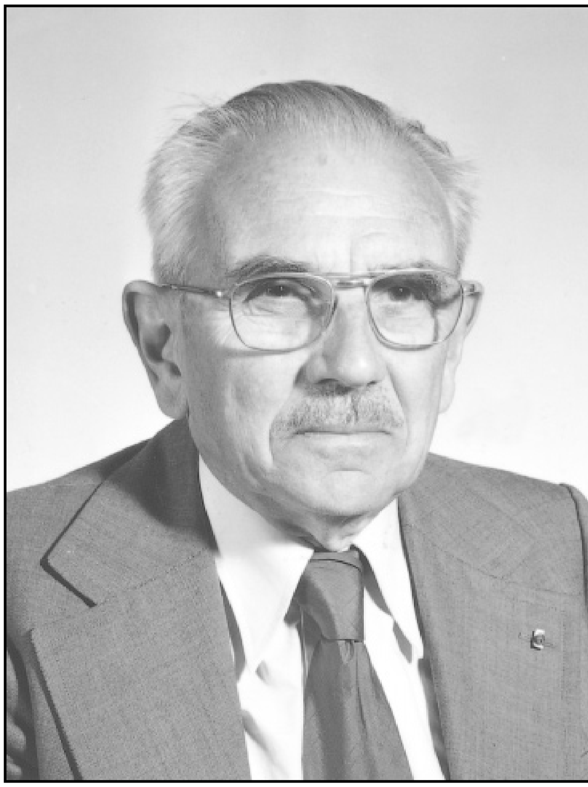
JEAN LERAY



7 NOVEMBER 1906 · 10 NOVEMBER 1998

JEAN LERAY was one of the major mathematicians of the twentieth century, whose revolutionary ideas have transformed several areas of mathematics. His collected works—actually a *Selecta* of his work—consist of three thick volumes. That their editor, the French mathematician and academician Paul Malliavin, should have had to ask three distinguished mathematicians from three completely different fields, Armand Borel, Peter Lax, and Guennadi Henkin, to write introductions for the three volumes, gives an indication of the fundamental importance and breadth of Leray's work.

Before making an attempt at describing his work, it is important to give some indications of the intellectual context "l'entre-deux-guerres," the twenties and thirties, in France—in which Leray's formative years occurred, and of the dramatic circumstances that he endured during World War II.[1]

A Short Account of Leray's Life and Career

Jean Leray was the son of two primary school teachers, the *instituteurs* who symbolized the Republic against the Old Regime. His wife, Marguerite Trumier, whom he married in 1931, was also the offspring of two *instituteurs*. She became a mathematics teacher in high school. He believed strongly in those values and in the system that had allowed a boy with modest origins to receive the best possible education. The Lerays had three children, Jean-Claude, an engineer, Françoise, a research biologist, and Denis, a doctor.

Jean Leray was successful at the entrance exam of the Ecole normale supérieure (ENS) in 1926, and became a student there in the class of 1926.[2] World War I had left the French universities, and particularly the University of Paris, in very bad shape. Contrary to what had happened in other countries, France sent its young men to first line combat zone irrespective of their education. A consequence was that many of the promising young scholars who would have become the junior professors of the twenties were dead.[3] Among the faculty in mathematics at the University of Paris, very few professors were still active,[4] and if

[1] To write this notice, I have made extensive use of the special issue of the *Gazette des mathématiciens* "Jean Leray," edited by Jean-Michel Kantor [K].

[2] In France, the class year is the year of acceptance in the school; ENS is not a degree-granting institution, but attached to the University of Paris, somewhat like a Harvard house, or a Cambridge or Oxford college, but one that would have only the very best students. Its curriculum covers roughly the last two years of college and a master's degree.

[3] Fifty percent of Ecole normale supérieure students of the classes 1911 to 1914 died during the war (see [A]).

[4] For an overview of this period, see [A].

they were, they were mostly not working on the forefront of contemporary mathematics—with some exceptions like the great geometer Elie Cartan.

The new generation of mathematicians that entered Ecole normale in the twenties was outstanding; among them were André Weil, Jean Dieudonné, Henri Cartan (Elie's son), Claude Chevalley, and Jean Leray. Weil and Leray, who were born the same year (1906) and both passed away in 1998, were probably the most influential.

The main challenge in mathematics at the time appeared to be the fulfillment of the program that the German mathematician David Hilbert had outlined. This led to the birth of Nicolas Bourbaki in the early thirties, as a small group of French mathematicians chose to call themselves—a pseudonym to be used for the part of their work that they would do collectively.[5] Their goal was to renovate research and exposition of mathematics, notably by putting *structures* at the heart of their approach, in continuation of the development of abstract algebra that had occurred in Germany since the beginning of the century. These young men were very much at odds with most of their professors, and had very little mathematical respect for them. At the beginning of their careers, they were the mavericks of French mathematics.

Leray chose a different path. Although he was asked to be part of Bourbaki, and he played a role in the meetings that led to its creation, he quickly drifted away. Whereas they were interested in pure mathematics, mostly algebra and geometry, he got involved in analysis, *applied* analysis at that. He chose as his advisor the mechanician Henri Villat and worked on mathematical problems arising from fluid dynamics—Villat's specialty.[6]

He defended his thesis[7] in 1933 and wrote several major and very influential papers in this domain between 1933 and 1939. In one of them, he defined the notion of a "weak" solution of a partial differential equation, and proved what is, still now, one of the very few results on the existence of solutions of the Navier-Stokes equation. Another of his celebrated results during that period was obtained in collaboration with the Polish-Jewish mathematician Julius Schauder. It is a fixed point theorem, essential for those applications in analysis that both authors had in mind, but which drew its inspiration from algebraic topology. He became a professor in the University of Nancy in 1938.

---

[5] The founding members of Bourbaki were Cartan, Chevalley, Del Sarte, Dieudonné, Ehresmann, de Possel, and Weil.

[6] In France as in some other countries, theoretical mechanics is an independent field, covering some parts of applied mathematics. In the Académie des sciences, mathematics is represented in two sections: the pure mathematicians are in "Géométrie," the applied mathematicians are in "Mécanique."

[7] Doctorat ès sciences.

When the war was declared in 1939, Leray, who was a reserve army officer like almost all university graduates at the time, was called to active duty and served as a lieutenant in an artillery unit. After France's military collapse in May–June of 1940 and the subsequent infamous armistice that the French government signed on 17 June, Leray became a prisoner of war on 24 June and was sent to a prisoner's camp in Austria (Oflag XVIIA) on 2 July. He remained there almost five years, until the camp was liberated on 10 May 1945.

The prisoners in the camp were mostly educated men, career or reserve officers, many of them still students. As in several other camps, a "university" was created and Leray became its rector. Classes were taught, exams were given, and degrees granted, with some degree of recognition by French authorities of the time. As for research, to fight the feeling that he might be losing the best productive years of his life, Leray wanted to resume his work. But he was confronted with a dilemma. If he continued working in fluid mechanics, he might be forced to collaborate with the German war effort. Instead, he decided to pursue some ideas in algebraic topology that he had foreseen during his collaboration with Schauder.[8]

By 1942, he was able to submit to Henri Villat three short research announcements for the *Comptes rendus de l'Académie des Sciences* and a full exposition of his work for publication in *Journal de mathématiques pures et appliquées*. The announcements came out in 1942, the paper itself in 1945 (it was divided in three parts, with the subtitle "Cours de topologie algébrique professé en captivité").[9]

Leray was elected to become a professor in the University of Paris in 1943 (a position he could of course not take before 1945), and a corresponding member of the Académie des sciences in 1944.[10] Oflag XVIIA was liberated by Allied troops in May 1945. After he came back to Paris, he continued his work in topology, elaborating on some of his ideas. This led to another series of short research announcements and longer papers, published between 1946 and 1950. In those papers, two fundamental notions were introduced—that of a "sheaf" and that of a "spectral sequence"—which allowed him to prove or reprove several important results.

---

[8] By history's tragic irony, Julius Schauder, who had been hiding in Lwov, was arrested and taken to a camp in 1943—a concentration camp, of course, where he died soon after.

[9] A course in algebraic topology taught in captivity.

[10] It is interesting to see how quickly Leray rose to a status of eminence in the academic world, not only by becoming a professor in Paris at a comparatively early age, but also by becoming a corresponding member at thirty-eight, and then a full member at forty-seven, of the Académie des sciences. The Bourbaki members, including those enjoying a similar scientific status as Leray, reached the status of "académicien" in the seventies.

Unfortunately, the radical novelty of his work, and the poor quality of the exposition, hindered its acceptance by the mathematical community, in spite of the considerable attention it drew. In the United States, "Most people . . . found Leray's papers obscure."[11] In fact, the only mathematicians who were able to understand Leray's ideas were, paradoxically, the Bourbaki members André Weil and Henri Cartan and some young mathematicians or students close to Cartan, notably Jean-Louis Koszul, Jean-Pierre Serre, and Armand Borel.

It is a mild understatement to say that Leray's ideas, reworked and refined by Cartan, Koszul, Serre, Borel, and others revolutionized large parts of pure mathematics after World War II. In the process of algebraization of mathematics, which is one of the main features of twentieth-century mathematics, sheafs and spectral sequences were the jewel in the crown.

Leray became a professor in the Collège de France in 1948, and a member of the Académie des sciences in 1953. He was a member of the Institute for Advanced Study in Princeton every fall semester from 1951 to 1961, belonged to twelve foreign academies, and received the Feltrinelli and Wolf prizes and the Lomonosov medal.

In the early fifties, Leray switched fields, once again—not by obligation this time. Starting from the study of wave propagation, he became interested in complex analysis, proving among other things a deep generalization of the Cauchy integral formula in 1959—the so-called Cauchy-Leray formula.

Leray worked and even published until the end of his life. In the later part of his life, he became interested in propagation problems with singularities. In 1981, at the age of seventy-five, he published a long book at MIT Press (*Lagrangian Analysis and Quantum Mechanics*); in the early nineties he published a few papers, and he was working on a draft of a research announcement in 1997. The publication of his collected works, which had been under consideration by Springer for many years, had to wait until Leray was ready to say that his work was ready to be collected. It was only in 1995 that he finally consented, at the age of eighty-nine: he entrusted the task of coordinating the publication of his collected works to Professor Paul Malliavin. The three volumes appeared in 1998, shortly before his death.

Leray's Scientific Contributions

It is a daunting task to give even a glimpse of Leray's mathematics, particularly in a short space. We will try to highlight three aspects.

[11] G. Whitehead, letter to John McCleary, 1997, in [J].

Partial differential equations and fluid mechanics.[12] As we have seen above, Leray was a student of Henri Villat, a specialist in fluid mechanics who was particularly interested in wakes—the mathematical study of fluid flows around a curved obstacle. Of course, the mathematical approach relies on studying a *partial differential equation*, i.e., an equation relating some partial (time and space) derivatives of the solution, or a system of such equations. Physically, one expects that once some boundary conditions are given, the equation should have *one, and only one* solution (when you bake a cake, its temperature at any point in the cake at time t should be completely determined by the initial temperatures of the dough and the temperature of the oven).

Several questions immediately arise:

- Can one prove mathematically this existence and uniqueness of solutions (under appropriate conditions on the data)?
- If indeed a solution exists, will it exist only for a short period of time, or forever (concretely, the physical object might explode after a certain amount of time)?
- What exactly does one mean by a solution? Is it a smooth function (and then how smooth)? If one simply looks at the flow of water in a very tame river, or at the wake of a boat on that river, it becomes obvious that the solutions ought to be very irregular (turbulent)—even with some discontinuities. But then how can one speak of the partial differential equation in the first place?

In his famous 1934 *Acta Mathematica* paper "Sur le mouvement d'un fluide visqueux emplissant l'espace," Leray addresses these questions (and others) in a highly innovative way. Viscous incompressible fluids are governed by the Navier-Stokes equations. He proves that regular solutions exist up to a time T and characterizes T. He introduces the notion of a *weak* solution (and in order to do this he defines what is now called a *Sobolev space*), giving a precise meaning to an irregular solution of the equation, and proves that there exist such weak solutions. A "strong-weak" uniqueness theorem then shows that, when there is a solution in the usual sense and a weak solution, the two should coincide.

Some progress has been made on the problem since 1934. But, as Chemin points out in [K], the paper has not lost its interest and currency today: progress has not been dramatic, much of the work that has been done since follows his approach, and the basic open questions that Leray mentions in his paper are still open. Also, the paper opens

[12] The few lines here give a very imprecise and somewhat misleading account of the results in the paper. See J.-Y. Chemin's article in [K] or the paper itself for precise statements.

new ground leading to Sobolev's work and the theory of distributions of L. Schwartz. A masterpiece indeed!

In the thirties, Leray also studied the "Helmholtz conformal representation problem"[13] in the theory of cavitating wakes. Helmholtz had studied the case of a flat obstacle; but curved obstacles remained intractable. Leray's idea was to "deform" the curved obstacle to a flat one, and use the fact that one had solutions in the flat case.

Several equations in analysis are solved using a "fixed point theorem"—one proves that solving the equation is actually equivalent to finding a fixed point for some abstract transformation from the set of all possible states of the physical system to itself. The problem here consisted of proving the appropriate fixed point theorem, and keeping track of what happens during the deformation procedure. The fixed point theorem is of course the Leray-Schauder theorem, and the application to the Helmholtz problem Leray's own work.

Sheaves and spectral sequences.[14] As we have seen, in Oflag XVIIA Leray had to be a topologist rather than a mechanician. In making this act of pure moral and intellectual will he could rely on two previous instances when he had thought about topology: his work with Schauder on the one hand, and an occasion when, still a student, he had been asked by Elie Cartan to write up his course on differential forms on Lie groups. His first task was to define a new cohomology theory.

Starting with Poincaré, attaching some algebraic objects to geometrical/topological spaces had become one of the most powerful tools of geometry/topology. For instance, the Poincaré group of a space describes the number of "holes" it contains. In cohomology theory one associates vector spaces (or, more generally, modules) to spaces. The simplest (but not simple) example is the de Rham cohomology of a manifold, the cohomology of differential forms: for instance, the so-called "first de Rham cohomology group" gives a precise description of the fact that a vector field whose curl is 0 may or may not derive from a potential. In his Oflag paper Leray extracted from the (concrete) example of de Rham cohomology (well understood at the time) axioms of what a cohomology theory ought to be, and then constructed such a theory. Of course, actually *computing* the cohomology groups is another matter!

Leray's cohomology was an important step for him, but other attempts by other mathematicians, around the same time, had a more lasting influence. Where Leray broke completely new ground was in

[13] See J. Serrin's article in [K] for details.

[14] See Borel's foreword in the volume 1 of [L], Houzel and Miller's papers in [K], McLeary's paper in [J].

the 1946 papers (although there are some indications that he had this development in mind in the 1942–45 papers). It consisted in attaching a cohomology theory to any map from a space E to another E'. This posed some deep problems, which he solved by defining two completely new concepts:

- the notion of a sheaf (*faisceau*), which permits the consideration of the cohomology of variable subspaces of E;
- the notion of a *spectral sequence*, which allows one to keep track of the cohomology for well-chosen subspaces and use the knowledge to compute the cohomology of E.

It took another few years for the theory to mature, stabilize its presentation, show its amazing power, and be fully accepted. For instance, the account by S. Eilenberg of the 1946 papers in the *Mathematical Reviews* was neutral, speaking of "interesting new methods." By the early fifties, the work of Cartan, Borel, and Serre had made it clear how powerful indeed these notions were, and not only in the context of topology for which they had been defined, but in many areas of mathematics. Contrary to Leray, Serre, who received the Fields medal in 1954, was—and is—a remarkable expositor. This was a significant factor in the acceptance of the new methods—manifested since the late fifties by the wide use of an ugly neologism: the English "sheafify," the French "faisceautiser."

Hyperbolic equations and complex analysis.[15] One of the first major applications of sheaf theory and spectral sequences was in complex analysis—the celebrated theorems A and B of Henri Cartan. But Leray, when he came back to analysis after ten years of topology, became interested in complex analysis in its own right. His motivation was again in partial differential equations, and more specifically hyperbolic partial differential equations (for instance wave propagation).

He wrote a series of papers "Problème de Cauchy I, II, . . . , VI" (actually number V was never published) where he studied these hyperbolic PDE, obtaining some deep results in the first papers, in particular an expression of the elementary solution of the corresponding Cauchy problem (one corresponding to a delta function input) as an integral. But since the integral (a kind of Laplace transform) was over a complex projective space, it became clear that he would need a calculus of residues that was not available in n variables complex analysis—just as computations in the classical theory of (one-dimensional) Laplace transform rely on the Cauchy integral formula and the Cauchy calculus of residues.

[15] See Henkin's introduction in vol. 3 of [L].

This led him in 1956 to prove the so-called "Cauchy-Fantappiè-Leray formula," which seems to be the ultimate generalization of Cauchy's integral formula for holomorphic functions. Then in 1959, in "Problème de Cauchy III," he proved his residue formula.

Again, Leray's work in this direction had a tremendous influence in analysis and in complex analysis. Let us quote Guennadi Henkin (in [L], volume 3): "Without exaggeration one can say that during the fifties-sixties the ideas of Leray twice radically changed the direction of the development of contemporary complex analysis. The Leray sheaf theory was the main tool for the great breakthrough in complex analysis in the early fifties. . . . In the sixties, thanks to the highly general Cauchy-Leray formula, the constructive methods of residue theory and of integral representations occupied once again a first rank position."

If World War II had not happened, Leray would perhaps be remembered "only" for his contributions in analysis and complex analysis. And Peter Lax would be fully justified in explaining ([L], vol. 2): "Like Poincaré, Leray chose to work mostly on problems that came from physics. In marked contrast, the founding members of the Bourbaki movement, most of them Leray's contemporaries, sought inspiration not in nature but in mathematics itself. That Leray remained faithful to nature had a profound effect on postwar French mathematics. . . . He was the intellectual guide of the present distinguished French school of applied mathematics. More than that, he provided that balance between the concrete and the abstract that is so essential for the health of mathematics."

But there was the "other" Leray, the one who, in the Oflag, constrained himself to be a pure mathematician. That this Leray should have had such an influence on the "Bourbaki" type of mathematics is perhaps one of the most exhilarating paradoxes in the history of contemporary mathematics.

Elected 1959

MARTIN ANDLER
Professor of Mathematics
Université de Versailles-Saint-Quentin

BIBLIOGRAPHY

[A] M. Andler, Les mathématiques à l'Ecole normale supérieure au XXème siecle: une esquisse, in *Ecole normale supérieure, le livre du bicentenaire*, ed. J. Sirinelli (Paris: PUF, 1994).

[J] I. M. James, *History of Topology* (Elsevier, 1999).

[K] J.-M. Kantor (ed.), *Jean Leray*, special issue, *La Gazette des mathématiciens* (Paris, 2000).
[L] J. Leray, *Œuvres scientifiques*, with an introduction by P. Malliavin and forewords by A. Borel, P. Lax, and H. Henkin (Heidelberg-Paris: Springer & Société mathématique de France, 1998).
[S] M. Schmidt, *Hommes de science: 28 portraits* (Paris: Hermann, 1990).