Leray in Oflag XVIIA: The origins of sheaf theory, sheaf cohomology, and spectral sequences

Haynes Miller*

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Jean Leray (November 7, 1906–November 10, 1998) was confined to an officers' prison camp ("Oflag") in Austria for the whole of World War II. There he took up algebraic topology, and the result was a spectacular flowering of highly original ideas, ideas which have, through the usual metamorphism of history, shaped the course of mathematics in the sixty years since then. Today we would divide his discoveries into three parts: sheaves, sheaf cohomology, and spectral sequences. For the most part these ideas became known only after the war ended, and fully five more years passed before they became widely understood. They now stand at the very heart of much of modern mathematics. I will try to describe them, how Leray may have come to them, and the reception they received.

1 Prewar work

Leray's first published work, in 1931, was in fluid dynamics; he proved the basic existence and uniqueness results for the Navier-Stokes equations. Roger Temam [74] has expressed the view that no further significant rigorous work on Navier-Stokes equations was done until that of E. Hopf in 1951.

The use of Picard's method for proving existence of solutions of differential equations led Leray to his work in topology with the Polish mathematician Juliusz Schauder. Schauder had recently proven versions valid in Banach spaces of two theorems proven for finite complexes by L. E. J. Brouwer: the fixed point theorem and the theorem of invariance of domain. Schauder employed a novel method, approximating his infinite dimensional problem by finite dimensional ones where Brouwer's work could be applied. He and Leray provided definitions of degree and index valid in a Banach space, by the same approximation methods. These topological invariants were used in a Lefschetz number argument to prove existence of fixed points, and Leray and others used them to establish existence results for solutions of partial differential equations.

I can do no better than quote Armand Borel [4] for the next chapter of Leray's life: "The Second World War broke out in 1939 and J. Leray [then Professor at the Sorbonne

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and an officer in the French army] was made prisoner by the Germans in 1940. He spent the next five years in captivity in an officers' camp, Oflag XVIIA[1] in Austria [not far from Salzburg]. With the help of some colleagues, he founded a university there, of which he became the Director ("recteur"). His major mathematical interests had been so far in analysis, on a variety of problems which, though theoretical, had their origins in, and potential applications to, technical problems in mechanics or fluid dynamics. Algebraic topology had been only a minor interest, geared to applications to analysis. Leray feared that if his competence as a "mechanic" ("mécanicien," his word) were known to the German authorities in the camp, he might be compelled to work for the German war machine, so he converted his minor interest to his major one, in fact to his essentially unique one, presented himself as a pure mathematician, and devoted himself mainly to algebraic topology."

2 "Un cours de topologie algébrique professé en captivité"

When he found himself in enemy hands, Leray set himself the goal of discovering methods which could be applied to a very general class of topological spaces to prove directly the kinds of theorems he and Schauder had proven more indirectly earlier. The desire to avoid, or at least disguise, the use of simplicial approximation became one of his basic motivations. A formative influence was the redaction of course notes for Élie Cartan [8], on differential (or "Pfaffian") forms on Lie groups, and Leray repeatedly said that he wished to bring the power of Élie Cartan's theory of differential forms to bear on general topological spaces. The theorems of de Rham [61] were central to Leray's thinking as well.[2]

Leray's research was announced in Comptes rendus notes "presented" to the Academie des Sciences on May 4, 1942 [31]–[34], and a full writeup of this prison course was sent to Leray's thesis advisor Henri Villat as editor of the Journal de Mathématiques Pures et Appliquées. (This journal had been founded by Liouville in 1836, and Leray himself was to serve as an editor from 1963 to 1995, first as a co-editor with Villat then, until 1991, as secretary of an editorial committee.) Heinz Hopf was asked to comment, he gave his approval, and the paper, [35]–[37], was duly published, in 1945, with the subtitle "cours de topologie algébrique professé en captivité." Villat wrote a foreward to the work in January, 1944. Leray nicknamed the paper TA, "Topologie algébrique." As Eilenberg points out in his review, many of these results were obtained independently

[1] Oflag was the Nazi shortening of *Offizierslager*, Officer's camp.

[2] It is curious and surprising that it was André Weil [76] rather than Leray himself who found the modern proof of the de Rham theorem, since this proof was a vindication of the local methods espoused by Leray. While Weil does not acknowledge Leray's influence, he had had conversations with Leray in 1945 during which Leray had described his notion of a coefficient system varying from point to point—a sheaf. Weil described his proof to Cartan in a famous letter [75] from Sao Paolo dated 18 January, 1947, in which he expressed the hope that this discovery would reinvigorate Cartan's research in algebraic topology. It did, and it may have provided the light which led Cartan to the modern formulation of sheaf theory.

using standard Čech methods by Lefschetz in [29], a work of which it is very likely Leray remained unaware until after the war ended. In any case, it seems that Leray's specific approach oriented him towards the issue of localization, and opened the way to his really new discoveries in a way the Čech theory would not have.

The cohomology theory described in TA received further development in the hands of Henri Cartan (in his very first Seminars [12][3]) and [13] and in his Harvard University course [72][4] and of Armand Borel [3] and István Fáry (who was a student of Leray's).

Leray sought to axiomatize the theory of differential forms, as he and Schauder had earlier axiomatized the degree. He started with a cochain complex $C^\bullet$, which he called an "abstract complex," over a ground ring A. The cochain complex became "concrete" by means of a "support" function, assigning to each form $\omega \in C^n$ a closed subset $|\omega|$ of a topological space X, subject to some evident axioms. (In his original work Leray assumed each C^n was a finitely generated free A-module, and assigned a (not necessarily closed) support to basis elements only.) Next he wished to axiomatize the Poincaré Lemma. In order to do this he first observed that a closed subset F of X determined a subgroup

$$\{\omega \in C^n : |\omega| \cap F = \emptyset\}$$

for each n, and that these subgroups together formed a subcomplex. Dividing by this subcomplex of forms which are so to speak zero on F led to a chain complex $C^\bullet(F)$ associated to each closed subset, together with "restriction maps" associated to each inclusion of closed subsets. This was the first example of the structure Leray would later, in 1946, call a *faisceau*.

The Poincaré Lemma was now axiomatized by requiring that for every point $p \in X$ the cochain complex $C^\bullet(\{p\})$ had homology isomorphic to the ground ring A. These hypotheses defined a *couverture*. The homology of the chain complex was to be an approximation of the cohomology of the space. Leray thus completely circumvented what had seemed to be the major part of the construction of cohomology, namely, the combinatorial construction of an explicit chain complex. Various existing constructions gave rise to couvertures, and thus allow themselves to be related with each other.

Leray next wished to compare different couvertures, and for this he defined the "intersection" of two of them, written $C^\bullet \bigcirc D^\bullet$. The cochain complex of $C^\bullet \bigcirc D^\bullet$ was the quotient of the tensor product[5] cochain complex $C^\bullet \otimes D^\bullet$ by the subcomplex of forms with empty support. It was in order to verify that the result is again a couverture that the "fundamental argument" alluded to below was first used.

Leray then in effect defined the "homology of X" as the direct limit of the homology modules of all couvertures of his space, though in later presentations he preferred to use

[3]In the currently available versions of this first seminar the treatment of sheaf theory has been suppressed, being replaced by his subsequent treatment in the next seminar.

[4]This mimeographed book fills out lectures given by Cartan in the Spring of 1948. It was "edited"—written, actually—by George Springer and Henry Pollak after Cartan had returned to France, and so it is hard to know exactly how to interpret the fact that Leray's name is not mentioned in them at all. It is equally surprising that Weil's name goes unspoken as well, given that the intent of the course was to develop a version of Leray's theory far enough to give Weil's proof of the de Rham theorem. Cartan uses the term "grating," following a usage by Alexander for a related concept, as a translation of "carapace," as he called his approach to the concept of a couverture.

[5]Tensor products had been introduced only very recently, in 1938, by H. Whitney [79].

the existence of a “fine” couverture, as defined independently by him [42] and H. Cartan [11, 12]. During the period of his contributions to topology, Leray resisted Whitney's term[6] “cohomology,” insisting in his prison course ([35], p 98): “. . . partageant l'opinion de M. Alexander, déjà cité, je crois superflu, donc nuisible, d'introduire les groupes de Betti d'un espace topologique . . . .”

Leray explained how to define products in this cohomology theory. In his final account of this material, [47], Leray, following, he said, a proposal made by Cartan [11], considered rather only cochain complexes equipped with a (not necessarily graded commutative) product, and arrived more directly at the cup-product.

Leray had already worked out the details of what we now know as the Leray spectral sequence when he wrote this prison paper. In a footnote (p 201) we read: “Dans un travail ultérieur, intitulé <<Les modules d'homologie d'une repreésentation>>, nous étudierons la topologie des représentations [applications] par des méthodes étroitement apparentées à celles que ce Cours de topologie applique à l'étude de la topologie des espaces.” (No paper of this title ever appeared, but the Comptes rendus notes from 1946 approximate it closely.) Thus he had the idea of a spectral sequence in 1943 but probably not in 1942, since he did not mention this in his Comptes rendus announcements from that year.

Leray gave a hint of how he came to the notion of a spectral sequence in a later paper [47], p. 9: “Le raisonnement fondamental que répètent avec diverses variantes les n$^{\text{os}}$ **4**, **17**, **27** et **32** de mon article [TA] équivaut à l'emploi de la proposition 10.4 (ci-dessous), c'est-à-dire à la considération d'un anneau spectral indépendant de son indice r; c'est l'analyse de ce raisonnement fondamental qui me conduisit à envisager des anneaux spectraux, puis filtrée.”

The results of this argument were essential to the development of his cohomology theory—for example, to the proof of the Künneth theorem. Koszul [26] wrote “Vers 1955 je me souviens lui avoir demandé ce qui l'avait mis sur la voie de ce qu'il a appelé “l'anneau d'homologie d'une représentation” dans ses Notes aux C.R. de 1946. Sa réponse a été: “le Théorème de Künneth”; je n'ai pas pu en savoir plus.”

This “fundamental argument” is the argument still at the root of our understanding of spectral sequences. Its simplest expression (not Leray's original expression of course) is in terms of a double complex C such that for each n, $C_{p,n-p} = 0$ for sufficiently small p. Then the statement is that if C is acyclic with respect to vertical homology then its total complex is also acyclic. A cycle z in the total complex has trivial components in all large horizontal degrees, say larger than p. Let z_p be its component in $C_{p,q}$ (with $p+q$ equal to the degree of z). It must be a cycle with respect to the vertical differential, since $d_v z_p$ cannot cancel with any other component of dz, and hence a boundary: there is $\bar{z}_p$ such that $d_v \bar{z}_p = z_p$. Then $z - d\bar{z}_p$ has smaller filtration but is homologous to z, and eventually z becomes homologous to an element in a zero group.

[6] from [79]. Whitney is also responsible for the notation $\cup$, dual to the intersection product $\cap$, for the product in cohomology.

3 The 1946 announcements

After the war Leray returned to Paris, and soon he contributed a pair of notes [38] and [39] to Comptes rendus. The first introduced sheaves and the second spectral sequences. They carried the indication that he presented this work to a meeting of the Académie des Sciences on May 27, 1946. This work must have seemed incredibly obscure at the time—a sheaf, a new concept certainly containing a lot of information, was immediately fed into an unstudied cohomology theory, and then used to define an invariant of a map on which there appeared without any motivation a highly complex structure, all expressed in a terse and contrarian style. One imagines that the audience was nonplussed. However this is merely a fantasy; no such talk was ever given; the word "presented" simply meant that the indicated Academy member or "correspondant" (Leray himself, in this instance) submitted the paper to be published. Anyway, in these notes he claimed to be applying ideas of TA to a map. The word *faisceau*[7] was introduced in the first of these announcements; so sheaf theory was born at the same moment as spectral sequences. For Leray a sheaf assigned a module or ring to a *closed* subspace; it was not until 1950 that Cartan refounded the theory using *open* subspaces. Leray's first example was the sheaf assigning to $F \subseteq X$ its pth cohomology group.

He observed that the cohomology theory in TA naturally accepted a sheaf as coefficients, and referred to Steenrod's cohomology with local coefficients [73] as a precedent. The fact that Leray's construction was so amenable to the use of a sheaf of coefficients was a direct reflection of his consistent focus on local properties, a focus coming from his original motivation and experience, namely the local index or degree that he began by trying to study.

Leray now took a closed map of normal spaces, $\pi : E \to E^*$ and defined the sheaf $\pi\mathcal{H}^p(E)$ on E^* which assigned to $F \subset E^*$ the module $H^p(\pi^{-1}F)$. The qth cohomology group of E^* with this sheaf as coefficients was the "(p,q)th homology group of the map." It is also interesting to observe here the impulse to relativize, long predating Grothendieck, which again one may trace to Leray's interest in fixed points.

In the second note he described the "structure" of the homology ring of a map. This structure consisted in the following data: (1) two filtrations (to use Cartan's word from two years later)

$$0 = \mathcal{Q}_0^{p,q} \subseteq \mathcal{Q}_1^{p,q} \subseteq \cdots \subseteq \mathcal{Q}_{q-1}^{p,q} = \mathcal{Q}_q^{p,q} = \cdots \subseteq H^q(E^*; \pi(\mathcal{H}^p(E)))$$

and

$$H^q(E^*; \pi(\mathcal{H}^p(E))) = \mathcal{P}_1^{p,q} \supseteq \mathcal{P}_2^{p,q} \supseteq \cdots \supseteq \mathcal{P}_{p+1}^{p,q} = \mathcal{P}_{p+2}^{p,q} = \cdots$$

such that $\mathcal{Q}_{q-1}^{p,q} \subseteq \mathcal{P}_{p+1}^{p,q}$; (2) isomorphisms

$$\Delta_r : \frac{\mathcal{P}_r^{p,q}}{\mathcal{P}_{r+1}^{p,q}} \to \frac{\mathcal{Q}_r^{p-r,q+r+1}}{\mathcal{Q}_{r-1}^{p-r,q+r+1}};$$

(3) a filtration

$$0 = \mathcal{E}^{-1,p+1} \subseteq \mathcal{E}^{0,p} \subseteq \cdots \subseteq \mathcal{E}^{p,0} = H^p(E; A);$$

[7] As for the English translation, John Moore recalls [60] sitting in Norman Steenrod's kitchen, in 1951, and fixing on "sheaf" as the English equivalent of "faisceau."

and (4) isomorphisms

$$\Gamma : \frac{\mathcal{P}^{p,q}_{p+1}}{\mathcal{Q}^{p,q}_{q-1}} \to \frac{\mathcal{E}^{p,q}}{\mathcal{E}^{p-1,q+1}}.$$

Leray also fully described the multiplicative structure in this setting. There is not much that looks familiar to modern eyes in this description, beyond the use of p, q, and r. We will return to an examination of this structure later. It must have appeared quite formidable and unpromising, to judge for example by Eilenberg's review, which reads in its entirety: "The second paper enters into more detail into the structure of this new group and states without proofs a number of applications."

The applications Leray offered were the following. Let E^* be compact Hausdorff and $\pi : E \to E^*$ a map.
(1) If each point inverse image has the cohomology of a point then π induces an isomorphism in cohomology.
(2) If each point inverse image is connected then $H^1(\pi)$ is a monomorphism.
(3) If E and E^* are manifolds with E^* simply connected, and π is a smooth fiber bundle, then $H^q(E^*; \pi(\mathcal{H}^p(E))) = H^q(E^*; H^q(F))$, the usual cohomology of the base with coefficients in the cohomology of the fiber. Furthermore the Poincaré polynomials $p(t; -)$ (with respect to a field of coefficients) enjoy the following relationship. There is a polynomial $b(t)$ with nonnegative integer coefficients such that $p(t; E) = p(t; F)p(t; E^*) - (1+t)b(t)$.
(4) Let E be a simply connected compact Hausdorff group, F a closed connected one-parameter subgroup, and $\pi : E \to E^*$ the projection to the corresponding homogeneous space. Then with rational coefficients, the cohomology of E^* is generated by a two-dimensional class z together with odd classes, subject only to a relation $z^{n+1} = 0$; $\pi^* z = 0$; $H^*(E)$ is generated by odd primitives ("hypermaximal classes") all but one of which are in the image of π^*.
(5) The Gysin sequence of a sphere bundle can be derived from this structure. This is of course quite evident from today's formulation, but Borel [6] recalls spending weeks, in 1949 or 1950, trying to understand how Leray did this. Leray also asserted that the following theorem of Samelson[8] came out: if a compact Lie group G acts transitively on a sphere S^m then its cohomology is isomorphic to the cohomology of a product of odd spheres, among which is S^m if m is odd and S^{2m-1} if m is even.

May, 1946, was also the month in which Roger Lyndon submitted his thesis at Harvard University, written under the direction of Saunders Mac Lane. As published in [54] (which is marked "Received June 16, 1947"), it contains an "ineffective" form of a spectral sequence for a group extension. He observed that if B is a normal subgroup of G, with quotient A, and G acts on a ring K, then A acts naturally on $H^*(B; K)$, and $H^*(G; K)$ has a filtration whose quotients, in sequence, are subgroups of quotients of $H^k(A; H^{n-k}(B; K))$.[9] At the end of the paper he alludes to examples amounting to

[8]Samelson's paper [62] dealt with Hopf's theory of the rational cohomology of Lie groups. The term Pontryagin ring is introduced here. Samelson considered the algebraic structure on homology consisting of Pontryagin product together with the intersection pairing.

[9]Two years later, Serre [64] was to observe that Cartan's work [10] led to a spectral sequence of the form $H^k(A, H^{n-k}(B; K)) \Rightarrow H^n(G; K)$. G. Hochschild read Serre's announcement and found a "more complicated but more explicit" (in Serre's words [70]) construction of the spectral sequence, and the two published their work together [22].

nontrivial differentials in the spectral sequence, and to the improbability of the filtration splitting. He ends with the following pessimistic comment: "But the groups $H^*(G,K)$ can hardly be determined from a knowledge of A and B, and the operators involved, without regard for the factor set W. Thus analysis of the present type, although it has proved useful in particular instances, can hardly be expected to yield any stronger general theorem." Mac Lane wrote (in a letter [55] to McCleary): "I visited Paris in late 1947. I talked to Leray (about sheaves and about spectral sequences) but did not see the connection with Lyndon's work. Leray was obscure!"

Leray contributed two more notes to Comptes rendus in 1946, [40] and [41], at the session of August 26. In the first he described the behavior of Poincaré duality in the spectral sequence of a smooth fibration. In the second, he computed the cohomology of the classical flag manifolds—that is, a classical simple Lie group modulo a maximal torus. His method was to study the spectral sequence of the fibration $G \to G/T$; the cohomology of the fiber and of the total space are known, and the problem was to deduce the cohomology of the remaining term. He explained that one deduced the first differential easily, and that the collapse of the spectral sequence followed from this purely algebraically. He obtained not only the Poincaré series—and so discovered that at least in these cases the cohomology of G/T was concentrated in even degrees and the Euler characteristic was the order of the Weyl group—but also a great deal of information about the ring structure. This is a challenging exercise, even today![10]

These results drew immediate attention but not immediate belief. George Whitehead commented mildly [77] "Most people (including myself) found Leray's papers obscure." Bill Massey was somewhat more blunt, in a letter [57] to John McCleary: "In the late 1940's and early 1950's all of us were studying Leray's papers to try to understand how he got the marvelous results he claimed. To be perfectly frank, I never got to 1st base in this enterprise, it was very frustrating. Leray was a horrible expositor."[11]

Leray was distressed at the slow acceptance of his ideas: "Ces notions furent mal accueillies en Amérique au moment de leur publication. C'était trop difficile. Les *Mathematical Reviews* demandèrent "À quoi ça peut servir?" Henri Cartan et Jean-Pierre Serre ont montré à quoi cela sert!" ([63], p 166)

Be that as it may, in early 1947 Leray was elected to the Chair of the theory of differential and functional equations at the Collège de France. This honor was followed

[10]Leray never published the details of this calculation, since they were superseded by the method described in his Brussels conference paper [49], which applied uniformly to any compact connected Lie group. This is essentially the modern approach to these questions, using "…un raisonnement par récurrence dû à A. Borel et …un théorème d'algèbre dû à C. Chevalley." A well-known consequence, stated in [49], is that the representation of the Weyl group W on $H^*(G/T)$ is none other than the translation representation on the group algebra.

[11]Of course Massey's own theory of exact couples [56], 1952, has made learning about spectral sequences much less frustrating for subsequent generations. In this letter Massey explains that his discovery of exact couples was not in fact a response to the French work at all, and at first was unrelated to spectral sequences: "Somehow it occurred to me that the procedure J.H.C.W.[hitehead] used [in [78]] to get his ["certain"] exact sequence, and the procedure Chern-Spanier used [to obtain the Gysin sequence, in [16]] can be extended (generalized?) to get an exact couple. About this time, Serre's thesis came out, which enabled me to understand spectral sequences, and make the connection with exact couples. If it hadn't been for Borel, Cartan, and Serre, I don't think I would have ever understood Leray's stuff."

in 1949 by the Prix Petit d'Ormoy, "pour l'ensemble de ses travaux de topologie et de la mécanique des fluides" [17]. This was a major prize—FF 50,000; the next largest prize announced at this time, in the sciences in general, was for FF 15,000.

4 Koszul and Cartan

Perhaps the first person to understand what Leray was up to was Henri Cartan. He had risen through the ranks in the 1930's at the University of Strasbourg. When the war began in 1939 that University evacuated and shared facilities with the University of Clermont-Ferrand. During that year Cartan was named Professor at the University of Paris and head of mathematics instruction at the Ecole Normale Supérieure. He was obliged to seek repatriation to occupied France in order to return to Paris. Before he left he promised the rector of the University of Strasbourg that he would return to Strasbourg after the war, and he did indeed spend the years 1945–47 there on leave ([63], p 32).

Cartan had a student there named Jean-Louis Koszul. Koszul was interested in the cohomology of homogeneous spaces, and Cartan encouraged him to look at Leray's work. The result was a pair of Comptes rendus notes, [24, 25] dated July 21 and September 8, 1947. As presented by Leray, a spectral sequence was a highly specialized and complex structure attached to an obscure topological object (the "homology of a map") which was itself expressed in terms of an unfamiliar homology theory (using "couvertures") with coefficients in yet another brand new object (a "sheaf"). Koszul liberated the notion of spectral sequence from this topological confinement, and brought the theory into its present-day form. His achievements were remarkable.

(1) He isolated the notion of a filtered differential ring as a convenient object determining a spectral sequence.[12] (Koszul calls this "un anneau à dérivation supérieure"; the word "filtration" only appears in Cartan's note [9] from 5 January 1948.) Once this was understood, a much wider range of applications opened up. Along with this enlargement of scope came the need for a name for the structure: Koszul called it the "sequence of homologies" ("*suite d'homologies*") of the filtered differential ring.

(2) He wrote down the structure of a spectral sequence as we now do, denoting the rth term by $\mathcal{E}_r$.

(3) This formulation makes the ring structure much easier to describe than it was for Leray; as Koszul observed, one has a sequence of differential rings.

(4) He pointed out that in defining a spectral sequence an internal grading is irrelevant to the structure (but thanks Cartan for this observation, and later [26] called Leray's attribution ([47], p 9) of it to him "bizarre"). Koszul adopted this convention in the first note, but in the second, [25], he used p for the filtration degree and q for the complementary degree, establishing the modern convention.

Looking back at Leray's definition, we see that $H^q(E^*; \pi(\mathcal{H}^p(E))) = E_2^{q,p}$, $\mathcal{Q}_r^{p,q}$ is the sum of the images of $d_2, \ldots, d_{r+1}$ in $E_2^{q,p}$, and $\mathcal{P}_r^{p,q}$ is the intersection of the kernels of $d_2, \ldots, d_r$ in $E_2^{q,p}$. Thus

$$E_r^{q,p} = \frac{\mathcal{P}_{r-1}^{p,q}}{\mathcal{Q}_{r-2}^{p,q}}.$$

[12] Dieudonné [18] and Leray [42] suggest that this idea goes back to Cartan.

The isomorphism Δ_r is essentially the differential d_{r+1}. The last differential hitting $E_2^{q,p}$ is d_q, and the last differential emanating from it is d_{p+1}. This accounts for the indexes at which the sequences of $\mathcal{P}$'s and $\mathcal{Q}$'s stabilize, and shows that $E_\infty^{q,p} = \mathcal{P}_{p+1}^{p,q}/\mathcal{Q}_{q-1}^{p,q}$. The map Γ is the identification

$$E_\infty^{q,p} \cong E_0^q H^{p+q}(E;A).$$

All the essential elements were thus present and complete in Leray's original formulation.

That summer, from June 26 to July 2, 1947, the Centre National de la Recherche Scientifique, or CNRS, hosted an international conference in Paris, taking as its title "*Topologie algébrique.*" This was the first international airing of Leray's wartime ideas. Koszul recalls [26]: "Je vois encore Leray posant sa craie à la fin de son exposé en disant (modestement?) qu'il ne comprenait décidément rien à la Topologie algébrique. Whitney, de son côté, avait commencé en affirmant qu'on avait maintenant fait le tour de l'homologie et que le moment était venu de faire porter ses efforts sur un autre terrain."— a familiar refrain, repeated almost without variation by many others in the subsquent half century.

We learn from Cartan [9] that Leray spoke at the 1947 CNRS conference about the spectral sequence of a finite Galois covering. Cartan used the exercise of generalizing Leray's theorem to an infinite cover as an occasion to work out the theory to his own satisfaction. The resulting pair of announcements are masterpieces of clarity. In the first, [9], Cartan firmly distinguished between graded and filtered rings. This paper contains the first use of the term "filtration." He recalled Koszul's association of a "Leray-Koszul sequence" to a filtered differential ring (and wrote E_r rather than $\mathcal{E}_r$). He considered as an example the two filtrations associated to a bigraded ring.

In the second part, [10], Cartan gave himself a group G acting on a differential graded ring A (in which the derivation has degree $+1$), and defined the cochains and hence the cohomology $H^*(G;A)$. The cochain complex is the total complex of a double complex, and hence there are two spectral sequences, one of which has $E_2 = H^*(G;H^*(A))$. He applied this with A given by the cochain complex of a locally compact space X with an action of G on it, and arrived at a spectral sequence which he called (echoing Leray) an invariant of the action. If further the action is totally discontinuous and the space is a countable union of compacts, then the spectral sequence converges to the cohomology of the orbit space. These announcements have a strikingly modern feel to them.

5 "Spectral"

Leray's writeup [42] for the 1947 conference represents part of a new cycle of publications by Leray on algebraic topology. These papers clearly show, and acknowledge, the influence of Henri Cartan, who had moved to Paris in October, 1947, immediately after the CNRS Colloquium. The style was fresher, with many fewer idiosyncrasies of notation and convention. Leray adopted the improvements proposed by Cartan (in his talk at the Colloquium, we are told) and Koszul, going so far as to use one of Cartan's words in his title: "L'homologie filtrée." This 23 page paper, represented as the contents of his 1947–48 course at the Collège de France, was Leray's first full-length work-up of spectral sequences. (The later Journal de Mathématiques Pures et Appliquées papers [47], representing this course together with the contents of a 1949–50 course, form his last and definitive statement.) In it he abandoned his initial formulation, recalled above; this was never to be published in full. Leray preferred to write $\mathcal{H}_r$ for Koszul's $\mathcal{E}_r$. (It was Cartan [9] who started to use the roman font.)

And in this conference writeup we find the first published use of the word "spectral," in the combination "anneau spectral." He certainly did not use it in the talk itself, and he studiously avoided any terminology for this structure in the series of Comptes rendus notes from 1949 [43]–[45], which deal with applications to Lie groups, homogeneous spaces, and group actions. I quote from a letter from Borel [5]: "I do not remember him telling me why he chose spectral, I can only speculate. At the time, "suite de Leray-Koszul" was emerging. Serre and I used it in our 1950 C.R. Note [7], which, in retrospect, surprises me a bit. What I know is that Leray was against it, he wanted a terminology without proper name, and shorter than "une suite [d'algèbres différentielles graduées]." I presume he came to spectral in analogy with spectral analysis or spectral decomposition of a Hilbert space with respect to a self-adjoint operator. Note that in his definition of filtration, in [42], he allows a filtration to be parametrized by the real numbers. To have such a filtration, maybe with some semi-continuity conditions, is formally more reminiscent of things labeled spectral in analysis (his field after all)."

6 Borel and Serre

During the academic year 1949–50 Armand Borel was studying in Paris under a CNRS subvention. He attended Leray's course at the Collège de France, and decided to follow it closely. A French student named Jean-Pierre Serre followed the lectures at first but Leray may have been a less than lucid lecturer and Serre soon dropped the course. So Borel would digest the material and then present it to Serre. The following summer, in June, 1950, Borel attended the Colloque de Topologie in Brussels (but Serre did not). There Beno Eckmann (who had been the one to suggest to Borel that he contact Cartan and Leray) mentioned some progress on a problem of Montgomery and Samelson (see [19]): if Euclidean space is fibered by compact fibers, must these fibers be points? When Borel returned to Paris he and Serre decided ("un certain dimanche de juin 1950," [63], p 222) to try to apply Leray's methods to this problem, and to their astonishment they succeeded in solving it completely, by "a simple application" of Leray's ideas [7]. This paper contains what may be the first example of what we now call a corner argument in what they termed a Leray-Koszul sequence. It represents the first time anyone other than Leray had been able to apply this machinery,[13] four years after its initial announcement and some eight years after its discovery. This success was a great impetus to both the authors: both went on to write theses [2, 68] in which spectral sequences were of central importance and which made decisive advances in this theory. Serre's thesis, in particular, was the entry point into spectral sequences for many topologists of the time and ever

[13]Interestingly, Arnold Shapiro [71] applied Leray's ideas, in the form described by Cartan in his 1949 papers and in his Harvard University course [72] to prove the same theorem, and so shares some part of this honor with Borel and Serre.

since.

In a letter [69] to John McCleary, Serre gave an account of the development of the ideas in his thesis. Soon after proving the theorem with Borel about fibering Euclidean spaces, he notice a paper which observed that the Eilenberg-Mac Lane space $K(\mathbb{Z},2)$ was none other than $\mathbb{C}P^\infty$, and hence its cohomology, at least, was known. Serre was used to thinking of $\mathbb{C}P^\infty$ as the base space for a principal S^1-bundle with contractible total space, and since S^1 is a $K(\mathbb{Z},1)$ this made him wonder whether two successive Eilenberg-Mac Lane spaces were always connected by a fibration of this form; or, more generally, whether for any given space X there was a fibration having contractible total space and X as base space. In a flash of insight he realized that the path-space construction had to be the answer, and that the definition of fibration could be generalized accordingly. Using the homotopy lifting property as a definition seems so natural to us today that it is hard to appreciate the originality of this step. It may help to recall the following words from S.-T. Hu [23] in 1950: "According to [Ralph Fox's 1943 review [20] of the theory of fiber spaces], the object of introducing the definition of fiber spaces is to state a *minimum* set of readily verifiable conditions under which the covering homotopy theorem holds." Hu goes on to give an improvement of Fox's definition, involving slicing functions.

There was still the difficulty that none of these spaces were locally compact, so Leray's machinery as currently formulated could not be applied. It is an irony typical of the history of science that despite Leray's declared intention to discover methods which worked for "general" topological spaces, his work was unusable by Serre precisely because it did not apply generally enough.

I now quote Serre [69]: "...in October 50, I took part in a Bourbaki meeting north of Paris [at Royaumont ([63], p 222)], and one day Cartan and Koszul asked me what I was doing with Eilenberg-MacLane cohomology, homotopy groups, etc. I told them that I had plenty of new things, but they all depended on a would-be extension of Leray theory to the singular context [and to his extended notion of fibration]. Then, I think it was Koszul who told me that he had already toyed with the idea of filtering the singular complex of the fibered space, and that it looked encouraging." In his subsequent announcement Serre specifically thanked Cartan and Koszul for help in finding the correct filtration to use.[14]

There were many nontrivial details to work out—he wound up using cubical theory, for example—but by December he had the results and announced them in Comptes rendus notes [65]—[67]. In this work Serre used *homology*, and so was forced to abandon the term "anneau spectral," which he had used in his work ([64], October 2, 1950) on the cohomology of group extensions. Koszul and Cartan were both using "suite" ("d'homologies" and "de Leray-Koszul" respectively), and "La suite spectrale" came out naturally in the title of Serre's first note on the subject, dated December 18, 1950.

Here then is a little table of the various terms used for this structure:

> **Leray** (27 May 1946) "une structure particulière de l'anneau d'homologie d'une représentation"

[14] This was not a matter of filtering by preimages of skeleta in a CW structure on the base. That idea is due to T. Kudô [27],[28].

Koszul (21 July 1947) “suite d’homologies (d’un anneau à dérivation supérieure)”

Cartan (5 January 1948) “suite de Leray-Koszul”

Leray (1949) “anneau spectral”

Borel and Serre (26 June 1950) “suite de Leray-Koszul”

Serre (2 October 1950) “anneau spectral”

Serre (18 December 1950) “suite spectrale”

With the publication of Serre’s thesis we reach the modern era of the subject, and Leray’s contribution to it ends (though he returned briefly to clean up some loose ends in [51] and [52]). Despite the profound impact he had on the subject, Leray’s total output in algebraic topology represents barely one sixth of his bibliography.

And what of sheaf theory? It was reborn in modern form in an *exposé* of the 1950–51 Cartan Seminar [13], written by Cartan and dated April 8, 1951. Following Michel Lazard, Cartan defined a sheaf as an *espace étalé* with group structure, and he realized that the natural form of localization was to *open* sets rather than closed. The notation $\Gamma(F, U)$ was used there for the group of sections of a sheaf F over an open set U; the order of the arguments was only reversed in later work. Cartan axiomatized the notion of supports; Leray had used “compact supports.” Cartan defined sheaf cohomology axiomatically, and proved existence by means of a resolution by fine sheaves. In his 1953 Brussels Colloquium paper [14] Cartan viewed a sheaf as a presheaf satisfying the gluing conditions, though the word presheaf had to await Grothendieck. The derived functor definition of sheaf cohomology first occurred in Grothendieck’s Kansas lectures from 1955, exposed in 1957 in “Tôhoku,” [21].

> La science ne s’apprend pas: elle se comprend. Elle n’est pas lettre morte et les livres n’assurent pas sa pérennité: elle est une pensée vivante. Pour s’intéresser à elle, puis la maîtriser, notre esprit doit, habilement guidé, la redécouvrir, de même que notre corps a dû revivre, dans le sein maternel, l’évolution qui créa notre espèce; non point tous ses détails, mais son schéma. Aussi n’y a-t-il qu’une façon efficace de faire acquérir par nos enfants les principes scientifiques qui sont stables, et les procédés techniques qui évoluent rapidement: c’est donner à nos enfants l’esprit de recherche.
>
> —Jean Leray [63], p 1.

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