Raoul Bott was born in Budapest. He received his bachelor's and master's degrees in electrical engineering from McGill University, and in 1949 a doctorate in applied mathematics from Carnegie Institute of Technology, where he worked in network theory under the guidance of R. J. Duffin. His conversion to topology was then consummated during a two-year stay at the Institute for Advanced Study. After working for the next eight years at the University of Michigan and again at IAS, he accepted a professorship at Harvard University in 1959. Among his many honors is an AMS Veblen Prize in geometry. He is a member of the National Academy of Sciences and a recipient of the National Medal of Science. This article is the text of an AMS–MAA Invited Address at the Providence Meetings on August 9, 1988.

The Topological Constraints on Analysis

RAOUL BOTT[1]

The other day I mentioned to Erna Alhfors that I was reading some autobiographical remarks of Jung. "How can you read an autobiography, Raoul," she reproached me. "Don't you know they are just a pack of lies?"

I start with this anecdote to give you fair warning. For, of course, what I have prepared for the occasion are precisely some reminiscences pertaining to my general topic. On balance, I decided that at sentimental occasions such as birthdays, personal lies are still to be preferred to quite impersonal verities.

It has been my honor—involving more work than you might think — to speak in this forum once before and then I talked about the years 1949–1951 at the Institute for Advanced Study in Princeton. This time I would like to talk about my second visit there in 1955–1957. For if I had learned some topology during my first stay, then this second visit was my initiation to a little analysis—granted in a rather algebraic form. But above all I chose this topic because that visit gave me a personal glimpse into a momentous period in the world of mathematics altogether and an especially significant one in the mathematical life of this country.

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For the start of my story I will take two famous mathematical paradigms of this century: (1) The Maximum Principle and (2) The Brouwer Fixed Point Theorem. Recall here that the Maximum Principle asserts that on a compact space a continuous function must *attain its maximum.* It is from this bedrock that the Birkhoff Minimax and the subsequent Morse, and Lusternick–Schmirelman theories spring, and even when the hypothesis of compactness fails, as it does in all the "harder" calculus variation problems, e.g., Minimal Surface Theory, Yang–Mills Theory, it is still this extension that points the way.

It was this first branch that captivated me during my first stay at Princeton and I cannot let it go by even now without comment. In a sense the "Morse Theory" *quantifies* the maximum principle. If X is a space, the measure of its *topological complexity* was taken by Morse to be its *Poincaré polynomial,* $P_t(X)$. This polynomial is defined by

$$P_t(X) = \sum b_i(X) \cdot t^i,$$

where the $b_i(X)$ are the Betti numbers of the space X. (They — roughly — count the number of holes in X, so that, for instance, $b_0(X)$ measures the number of connected pieces into which X falls.)

Now Morse replaces "maximum" by "extremum" and attaches a measure of complexity $\mu_t^f(Y)$ to every extremum Y of a function f on X. The total complexity of f is then measured by

$$\mathscr{M}_t(f) = \sum_Y \mu_t^f(Y),$$

where Y ranges over the extrema of f. He then shows that under suitable conditions $P_t(X)$ *serves as a lower bound* for $\mathscr{M}_t(f)$. More precisely, Morse writes

$$\mathscr{M}_t(f) - P_t(X) = (1+t)E(t),$$

and then shows that the "error term" $E(t)$ must be a "positive" polynomial in the sense that its coefficients are ≥ 0. In short, Morse's maxim is that "topological complexity of a space goes hand in hand with the extremal complexity of functions on the space." Or to put it most simply, "topological complexity forces extrema."

Let me show you how all this works in an example from analysis which, however, is usually *not* thought of this way in strictly analytical circles. Namely, consider the eigenvalue problem:

$$L(y) \equiv \frac{d^2y}{dx^2} + q(x)y = \lambda y$$

on the unit interval $0 \leq x \leq 1$ with boundary conditions $y(0) = y(1) = 0$. Now associated to L is its quadratic form:

$$Q(y) = \int_0^1 Ly\, y\, dx = \int (-y'^2 + q(x)y^2)\, dt$$

and you should think of it as a function on the unit sphere S^∞:

$$\int y^2\,dt = 1,$$

in an appropriate Hilbert space of functions y. For then, as is well known, the extrema of Q on S^∞ correspond to the solutions of (4). Indeed, the Lagrange procedure tells us to look for the extrema of $Q(y) + \lambda \int y^2\,dt$, and these immediately lead to (4).

But note now that both $Q(y)$ and the constraint $\int y^2\,dt = 1$ have the symmetry $y \to -y$, so that *properly speaking* the above argument should be applied to the projective space rather than S^∞, and this innocent remark is crucial to us because the topology of $\mathbf{R}P_\infty$ is much, much more complicated than that of the sphere S^∞ at least if we work mod 2. In fact, $\mathbf{R}P_\infty$ then has a "hole" in every dimension so that

$$P_t(\mathbf{R}P_\infty) = 1 + t + t^2 + \cdots,$$

while the *sphere* in an infinite-dimensional *Hilbert space* has no holes whatsoever:

$$P_t(S^\infty) = 1.$$

Hence, if we know *Morse theory* we would immediately expect this problem to have an infinite number of eigenvalues — as of course it does.

But even more is true. Regardless of the degeneracy of the eigenvalues of Q, they are such as to precisely account for the holes in $\mathbf{R}P_\infty$, in short the error term $E(t)$ vanishes *on the nose* for this example, and one speaks of a "perfect Morse function." Indeed if Y is an extremum of Q on $\mathbf{R}P_\infty$, corresponding to an eigenvalue λ of (4) with multiplicity k, then $Y = \mathbf{R}P_k$ and in Morse's way of counting Y is seen to contribute by

$$\begin{aligned}\mu_t^Q(Y) &= P_t(\mathbf{R}P_k)\cdot t^{\lambda_Y}\\ &= (1 + t + \cdots + t^k)t^{\lambda_Y}\end{aligned}$$

with λ_Y equal to the number of eigenvalues less than λ. Hence the μ_ts clearly add up to $P_t(\mathbf{R}P_\infty)$. Q.E.D.

Two comments are now certainly in order:

(1) In some sense the Morse theory or one of its variants is the only way to treat the nonlinear extensions of our problem, although this is often hidden in the actual accounts by analysts.

(2) Secondly, note that the interesting topology in this example was forced by the symmetry, $y \to -y$ of the problem, which was treated here in the most straightforward way imaginable — that is, by simply *dividing by the action of* $\mathbf{Z}_2$. In the more difficult cases, i.e., in the Yang–Mills theory, the extrema are again seen to be created by what the physicists call the group of "gauge transformations," i.e., the symmetries of the problem. However, these now have to be treated in a much more subtle manner.

But so much for my first love in topology. To get on to my topic proper I have to pass on now to my second love, and as you will see, it has, quite characteristically, many similarities to my first one. This second love of mine deals with the fixed point phenomena, and the line I would like to draw here starts with Brouwer, goes on to Lefschetz, and then reappears again, after the deep insight of the 1950s due to the advent of sheaf theory, in the realm of elliptic differential equations. For it is in this guise that Atiyah and I recognized the Lefschetz phenomena again in 1964.

The magnificently simple statement of Brouwer's theorem is of course the following one: "*Any continuous map of the unit disc* $D^n \subset \mathbf{R}^n$ *into itself has a fixed point.*" I expect everyone here is familiar with this theorem and has used it in some form or another, if not for profound reasons, then at least to amuse some layman with one of its very unlikely consequences in everyday life.

The Lefschetz extension of this phenomenon again uses the same topological invariants of a space as the Morse theory does — but now understood in a more sophisticated sense. Indeed the Betti numbers $b_i(X)$ are really the dimensions of certain vector spaces, $H^i(X)$, "the cohomology spaces" of a space X, and these behave "functorially" with respect to continuous functions.

Precisely, this means that the vector space $H^i(X)$ is not only well defined in terms of X, but *moves* with X in the sense that a continuous function from X to Y,

$$\varphi\colon X \to Y,$$

induces a homomorphism denoted by φ^* — or by $H^i(\varphi)$ by the purists — going in the opposite direction:

$$H^i(Y) \xleftarrow{\varphi^*} H^i(X),$$

and subject to the very simple axiom that $(\varphi \circ \psi)^* = \psi^* \circ \varphi^*$ and that the homomorphism induced by the identity is the identity.

This "simple" and by now quite natural functorial concept due to Eilenberg and Mac Lane is of course also of the 1950s vintage, and its clarifying force on all our mathematical thinking cannot be overestimated. In any case in terms of such a "cohomology functor" $X \rightsquigarrow H^i(X)$ with $H^i(X)$ *finite dimensional* one can then define a Lefschetz number for

$$\varphi\colon X \to X,$$

by the formula

$$\mathscr{L}(\varphi) = \sum (-1)^i \text{trace } H^i(\varphi),$$

which is then some sort of a numerical measure of the extent to which the topology of the space has been perturbed by φ. The Lefschetz theorem now asserts that if $\varphi\colon X \to X$ is a map of the compact polyhedron X into itself, and if $L(\varphi) \neq 0$ *then* φ *has a fixed point.*

Let me start with a few quite elementary observations concerning this theorem.

(1) When X is the disc, $H^0(X) = \mathbf{R}$ and $H^i(X) = 0$ for $i > 0$. Furthermore, $\varphi^* = 1$. Hence the Lefschetz theorem does extend Brouwer's theorem!

(2) When φ is the identity, then the Lefschetz number is clearly given by

$$\mathscr{L}(I) = \sum(-1)^i \dim H^i(X) = \sum(-1)^i b_i(X).$$

This alternating sum turns out to be much simpler to compute than the Betti numbers individually, and indeed is given by purely counting the number of cells one needs to decompose X! In fact, *this* Lefschetz number turns out to agree with the *oldest* of topological invariants, the Euler number of a polyhedron. That is,

$$\text{Euler}(X) = \sum(-1)^k(\#\text{ of } k - \text{cells in } X).$$

Thirdly, let me remark that the implication $L(\varphi) \neq 0 \Rightarrow \varphi$ has a fixed point, suggests a refinement of the Lefschetz theorem of the sort:

$$\mathscr{L}(\varphi) = \sum_p \mu(p),$$

where p ranges over the fixed point set of φ, and $\mu(p)$ is now some sort of numerical measure of the *local* behavior of φ near p. For instance, if φ is a smooth map of a compact manifold with isolated fixed points $\{p\}$ then the "*Hopf formula*" does just that. That is, under these circumstances,

$$\mathscr{L}(\varphi) = \sum \mu(p)$$

is valid with $\mu(p)$ an integer which measures how often a small sphere about the fixed point p is "wrapped about itself" by φ.

Finally, note that the Lefschetz theorem is not unrelated to the Morse inequalities. Indeed, if φ pushes M a little in the direction of the gradient of a function f on M, then the *fixed points* of φ are precisely the *critical points* of f, and φ now being deformable into the identity, we have $L(\varphi) = L(I)$, so that the Lefschetz number of φ is given by

$$\mathscr{L}(\varphi) = \text{Euler number}(M) = \sum_{df_p=0} \mu(p),$$

while the Morse inequalities yield (after setting $t = -1$):

$$\sum(-1)^i b_i(X) = \sum(-1)^{\lambda_p}.$$

And indeed one checks here that in this case the Hopf formula gives $\mu(p) = (-1)^{\lambda_p}$ so that these two formulas are identical. In short, the Morse inequalities refine Lefschetz's theorem for maps φ near the identity which are given by small gradient pushes of functions, while the Lefschetz formula isolates that part of the Morse inequalities which is valid for all maps.

Now what I learned at the Institute in these years, 1955–1957, and what had really only just burst upon the mathematical landscape a few years earlier, was that all these "old verities" could be fitted into a much larger framework, and it was my very good fortune that my principal "instructors" in this new lore were two young upstarts called J.-P. Serre and F. Hirzebruch.

The Institute had changed dramatically since 1949. Einstein was no more, von Neumann was gravely ill, C. L. Siegel had left, and Hermann Weyl was commuting to Switzerland. Oppenheimer was strangely altered after the adventures of the McCarthy period, and when I came to pay my respects his whole manner had somehow taken on an "Einsteinian" aura. Of course, my older friends were still quite unchanged: Marston Morse, A. Selberg, and Deane Montgomery — but if my previous stay was dominated by a feeling of awe at the brilliance of the older generation, during this second one, my dominant feeling was one of awe, alas, mixed with envy, at the brilliance of a generation younger or contemporary to my own!

And no wonder. During that time, and largely at Princeton, I met Serre, Thom, Hirzebruch, Atiyah, Singer, Milnor, Moore, Borel, Kostant, Harish-Chandra, James, Adams,..., and I could go on and on. But these people — together with Kodaira and Spencer — and my more or less "personal remedial tutor," Arnold Shapiro, were the ones I had most mathematical contact with.

It was Serre who tried to teach me sheaf theory. "Poor Bott," he would say, whenever he was on the verge of giving up. I had come to Princeton all excited to meet the great topologist, Serre, and, lo and behold, here he was — suddenly an algebraic geometer! But of course the other side of it was that in some sense Serre had brought topology and its techniques to algebraic and analytic geometry and so he could explain it to someone with my interests like nobody else.

Serre is a prime example of what I call a "smart mathematician" — as opposed to a "dumb one." What he knows is so crystal clear in his mind that he can give us lesser mortals the feeling that it is indeed all child's play. He also had, and still has, the infuriating habit of never seeming to work. In public one sees him playing ping pong, chess, or reading the paper — never in the sort of mathematical fog so many of us inhabit most of the time. If one asks him a question, he either knows the answer immediately, and then inside out, or he declines comment. I would ask him: "Well, have you *really thought* about this?" He would say, "How can I *think about it* when I don't know the *answer*!" Added in Proof: Serre disputes this interchange and asserts that it is just one of the many untruths in my "pack of lies." Mrs. Serre comments that from her view "Serre works all the time." And indeed he claims that all his true work is done in his sleep! What can one say? It is an unfair world — as we all know.

But I should return to my subject and tell you a little of what it was that I was being taught. Well, next to what Serre was explaining to me, it was the goings on at the University, namely the activities of the team of Kodaira, Spencer and Hirzebruch, which fascinated me most, and in particular, it was Hirzebruch's "Riemann–Roch" formula which quite took my breath away. So let me try to explain here a little the completely new view that "sheaf theory" brought to our understanding of cohomology, and let me illustrate this with the de Rham theory — which is in any case the most "immediate" concept with deep ramifications in physics, geometry, and topology.

Now "homology" is of course the most intuitive concept in topology. Thus $H_0(X)$ measures the number of connected components into which a space falls, $H_1(X)$ measures how many interesting loops — or 1-dimensional holes — X has and so on. Cohomology was, therefore, at first just "the dual" object to homology. For example, the 1-form

$$\omega = \frac{1}{2\pi}\frac{xdy - ydx}{x^2 + y^2}$$

defined in $\mathbf{R}^2 - 0$, is dual to the hole in $\mathbf{R}^2$ created by removing the origin, in the sense that the line integral

$$\int_\gamma \omega$$

over any oriented curve in $\mathbf{R}^2 - 0$ measures its winding number around 0.

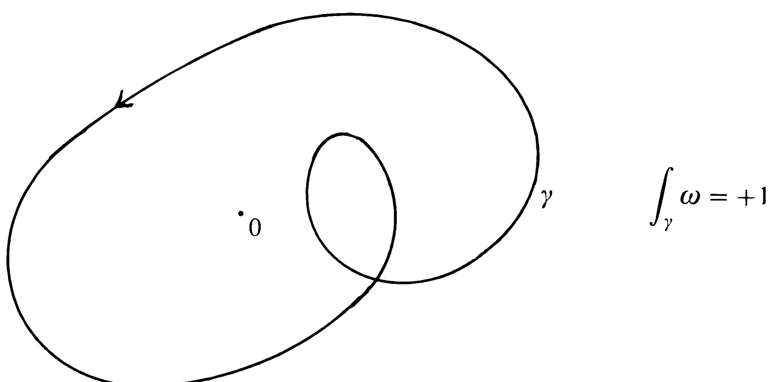


FIGURE 1

In "sheaf theory" (Leray, Cartan, Weil,...) which fashioned such a striking path between topology, analysis, and algebraic geometry, the relation of 1-form to winding numbers is quite *disregarded.* Instead one starts directly from a dual definition of connected component, and then follows this definition to its "logical" or, should I say its "homological" end. Namely, one defines the number of components of a space, as the dimension of the vector space $H^0(X)$ of "locally constant functions" on X. This concept is hopefully quite clear: f is locally constant on X if every point $p \in X$ has a neighborhood U with $f(p) = f(q)$ for all $q = U$.

Now if we are dealing, say, with an open set U in R^n, then the *locally constant* C^∞-functions can already serve to define $H^0(U)$, but these are now clearly characterized by a simple differential equation:

$$f \text{ locally constant } \iff df \equiv 0$$

where

$$df = \sum \frac{\partial f}{\partial x^i} dx^i.$$

In short, $H^0(U)$ can now be interpreted as the *vector space of solutions* on U of the differential equation

$$df = 0,$$

and of course one now finds that this equation makes sense in an intrinsic manner on *all C^∞-manifolds.* Furthermore the so-called higher de Rham groups $H^i(X)$ are now seen to be determined through a "*universal principle,*" really, of homological algebra, from H^0, of which the classical de Rham complex procedure is just one special example!

Recall that this so-called de Rham complex of a manifold X, usually notated

$$\Omega^*(X)\colon\ \Omega^0(X) \xrightarrow{d} \Omega^1(X) \cdots \Omega^n(X)$$

consists of the collection of q-alternating covariant tensors on X, notated Ω^q, so that

$$\Omega^0(X) \equiv C^\infty(X),$$

and to which our d has a canonical "God-given" extension satisfying $d^2 = 0$. In terms of this extended d, the "de Rham" cohomology is given by

$$H^*(X) = \text{Ker } d/\text{Image } d.$$

That is, by

$$H^q(X) = \begin{cases} \text{solutions of } du = 0 \text{ in } \Omega^q \\ \text{modulo the trivial} \\ \text{solutions } u = dv,\ v \text{ in } \Omega^{q-1}. \end{cases}$$

Thus $H^0(X)$ is precisely the space of solutions of $du = 0$, while the others are more mysterious. Now what sheaf theory taught us was that actually all these "higher" cohomology groups could all already be determined from the behavior of just H^0, *but now considered as a function or, rather, functor over all open sets U in X*! Put otherwise, the sheaf

$$U \rightsquigarrow \underline{\mathbf{R}}(U) \equiv \text{locally constant functions on } U,$$

determines everything, and from this vantage point a whole new vista of inquiry appeared, a vista which we have been exploring for the past forty years and which still shows no signs of being exhausted. One new question was of course this: What has this universal cohomological construction to teach us when applied to the sheaf determined by the solutions of general differential equations $Df = 0$?

Now generalization for generalization's sake is always suspect in mathematics. But when the generalization sheds new light on old questions and provides new tools to tackle these old questions — then we have first-rate mathematics. And that was certainly the case in those early 50s, when all the ramifications of the homological point of view were falling into place, primarily in the realm of "complex analytic" and "algebraic" manifolds. The step here is to pass from the operator $df = \frac{\partial f}{\partial x^i} dx^i$ in $\mathbf{R}^n$ to the operator

$$\overline{\partial} f = \frac{\partial f}{\partial \overline{z}_i} d\overline{z}^i \text{ in } C^q.$$

In short, to replace the sheaf of locally constant *functions* $\underline{\mathbf{R}}(U)$ on $U \subset M$, by the sheaf of locally *holomorphic functions* $O(U)$ on a complex manifold M. The resulting cohomology groups, usually denoted by $H^q(M; O)$, now turned out to be familiar in the classical literature for $q = 0, 1, 2$, and at the same time amenable to quite *new* manipulations inspired by their similarity with the old de Rham theory. It was into these mysteries that my friends were initiating us in Princeton at that time.

For those unfamiliar with the notion of an algebraic and complex manifold, let me recall here the most *fundamental* example of such an animal. That is the "Complex Projective Space": CP_n. It is obtained from $C^n - 0$, by identifying $z = (z_0, \dots, z_n)$ with $(\lambda z_0, \dots, \lambda z_n)$, $\lambda \in C^*$, z in $C^n - 0$. This space is compact because we can rescale any $z \in C^n - 0$ to have length $|z|^2 = \sum |z|_i^2$ equal to 1, where CP_n is a quotient space of the $(2n-1)$-sphere, that is, we have a map

$$\pi\colon S^{2n+1} \to CP_n,$$

with fibers, the circles swept out by λz; $|\lambda| = 1$. (This is the famous Hopf fibering, which by now is well known and fundamental in so many branches of mathematics.)

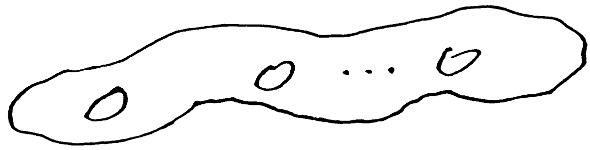
Classical algebraic geometry really starts with the study of CP_n and all the "algebraic varieties" cut out by a system of polynomials in CP_n, in short, by the spaces $X \subseteq CP_n$ given by

$$z \in X \Leftrightarrow \{f_i(z) = 0\},$$

where $f_i(z)$ is a system of *homogeneous* polynomials in z. For example, the equation

$$z_0^n + z_1^n + z_2^n = 0$$

cuts out in CP_2, the Riemann surface



with $(n-1)(n-2)$ one-dimensional *holes*.

Now the first theorem about the structure sheaf O on such a variety X is really that the new cohomology group $H^i(X;O)$ are "finite-dimensional," and that amongst them one could discover all the "old" invariants of algebraic geometry. In fact, the new *holomorphic* Euler number:

$$\chi(X;O) = \sum(-1)^i \dim H^i(X;O)$$

now turned out to be the so-called "algebraic genus" of X, so that an old and famous inequality of Riemann and Roch could now be re-interpreted as a computation of $\chi(X;O)$ and more generally of $\chi(X;\mathscr{F})$ where $\mathscr{F}$ were certain *twisted* forms of the sheaf O.

This "twisting concept" which replaced ill-defined objects with poles — such as meromorphic functions — by smooth holomorphic "sections of a line bundle L over M" was another insight of this era — originally due to A. Weil, I believe, which was most welcome to topologists. After all, we cut our teeth on the arch *line bundle* of all — the infinite Möbius strip.

Recall here that this line bundle L arises from the strip $-1 \leq x \leq 1$ in $\mathbf{R}_2$ by identifying $(-1, y)$ with $(1, -y)$. This space clearly admits a projection onto the circle S^1, in the guise of the interval $-1 \leq x \leq 1$ where $+1$ has been identified with -1, and this projection π clearly has inverse images $\pi^{-1}(x)$ which are isomorphic to $\mathbf{R}$. (See Figure 2.)

Hence a section s of L, that is, any map

$$s\colon\ S^1 \to L, \text{ with } \pi s(x) = x,$$

behaves locally *just* like an $\mathbf{R}$-valued function, but is of course globally very different! Indeed notice that L has an obvious 0-section, s, but it also has a section s_1 which intersects s_0 in one point! But if these were ordinary functions from S^1 to $\mathbf{R}$, they would have to intersect in two points!

Well, in 1954 Kodaira was teaching a course where I started to learn all these things — but in the complex analytic domain and intertwined with complex analysis. And what wonderful lectures those were! Kodaira at the board, a benevolent presence of keen and silent intelligence, printing magnificent symbols and short sentences with his impeccable Japanese hand. Once in a while he would have a look at us — in order to punctuate an argument, — and, after a slight struggle, produce a word or two.

The only problem for me was that these lectures went too fast. Although the immediate impression was one of great calm and of a measured pace, without all that chatter, he could really cover ground!

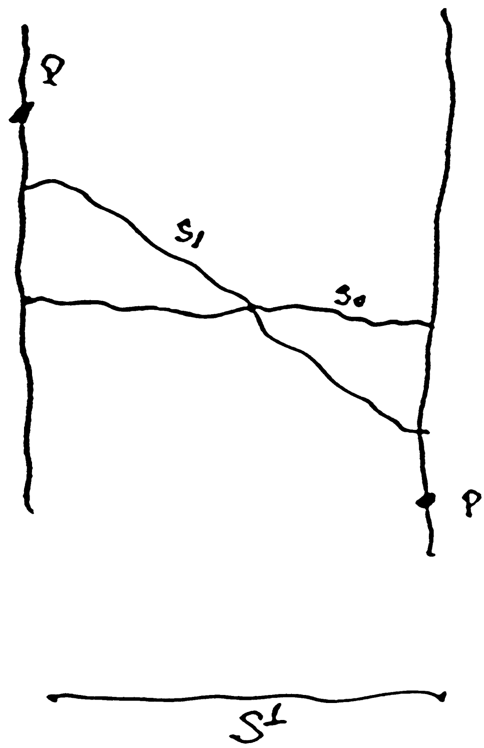


FIGURE 2

But in any case, I did learn the broad outlines of this new view of algebraic geometry. Quite briefly, it turns out that to study the space of meromorphic functions whose poles do not exceed a certain "magnitude" is the same thing as to first build a holomorphic line bundle L over M, and then to study the space of "holomorphic sections $\Gamma_h(L)$" of M. Because L is holomorphic, the operator $\overline{\partial}$ is seen to make sense in this context. Thus the sheaf $O(L)$ of sections $s \in \Gamma(L)$ with $\overline{\partial} s = 0$, makes sense and $\Gamma_h(L)$ is thus re-interpreted as:

$$\Gamma_h(L) \cong H^0(X; O(L)).$$

Also one sees that the notion of "magnitude" or "degree of the total number of poles" is now recoded into the more topological notion of measuring the twist of a line bundle L over X.

For a Riemann surface these reformulations are usually quite transparent. For instance, there the possible twists of a line bundle are measured by an integer $c_1(L)$, and $\Gamma_h(L)$ describes precisely those meromorphic functions whose poles counted with multiplicity add up to $c_1(L)$. Indeed, any such meromorphic function is then the ratio s_1/s_2 of two *holomorphic sections* in $\Gamma(L)$!

In this context the classical Riemann–Roch formula now reads

$$\begin{aligned}\dim H^0(X; O(L)) - \dim H^1(X; O(L)) \\ = c_1(L) + 1 - g\end{aligned}$$

with $2g$ the number of "1-holes" in X. Note that this is really an existence theorem in disguise. For if $c_1(L) > (1-g) + 1$ then we must have $H^0(X, O(L)) \geq 2$. In short, there must be a nontrivial meromorphic function s_1/s_2, with total pole "strength" $c_1(L)$! For instance, Riemann–Roch implies that there exists at least one meromorphic function f_P on X with a pole of degree $1+g$ at a given point P on X! This is a far from obvious fact.

It is this formula which F. Hirzebruch saw how to generalize to arbitrary dimensions, and which — as I said earlier — filled me, and I think all of us, with wonder. Fritz told me once that Hermann Weyl just shook his head in disbelief when Fritz first explained his ambitious conjectures to him.

Let me here at least explain our *wonder* at the nature of his generalization. Indeed, to get anywhere one must realize that the proper measure of the twist of a general line bundle L over M is really an element $c_1(L)$ in $H^2(M; R)$, the *ordinary* second cohomology of M. This granted, let us assume next (only for the sake of brevity) that the tangent bundle of M splits into a direct sum of line bundles

$$T = E_1 + \cdots + E_d.$$

Thus our data X and L furnish us with d elements $x_i = c_1(E_i)$ in $H^2(M; R)$ as well as with additional class $c_1(L)$, describing the twist of L.

Now "Presto" form the expression

$$e^{c_1(L)} \cdot \prod_{i=1}^{d} \frac{x_i}{1 - e^{-x_i}}$$

in $H^*(M)$, then collect the terms of dimension $2m$ and integrate over M to get Hirzebruch's generalization:

$$\sum (-1)^i \dim H^i(M, O(L)) = \int_M e^{c_1(L)} \prod \frac{x_i}{1 - e^{-x_i}}.$$

Thus the innocent $c_1(L)$ and $(1-g) = \frac{1}{2}$ (Euler number of X) of the original formula had grown into this — at first sight — quite unlikely expression. No wonder Hermann Weyl shook his head.

But Fritz went on undaunted, impetuously using *everything* he had so precociously learned. I like to draw the picture of this proof as in Figure 3.

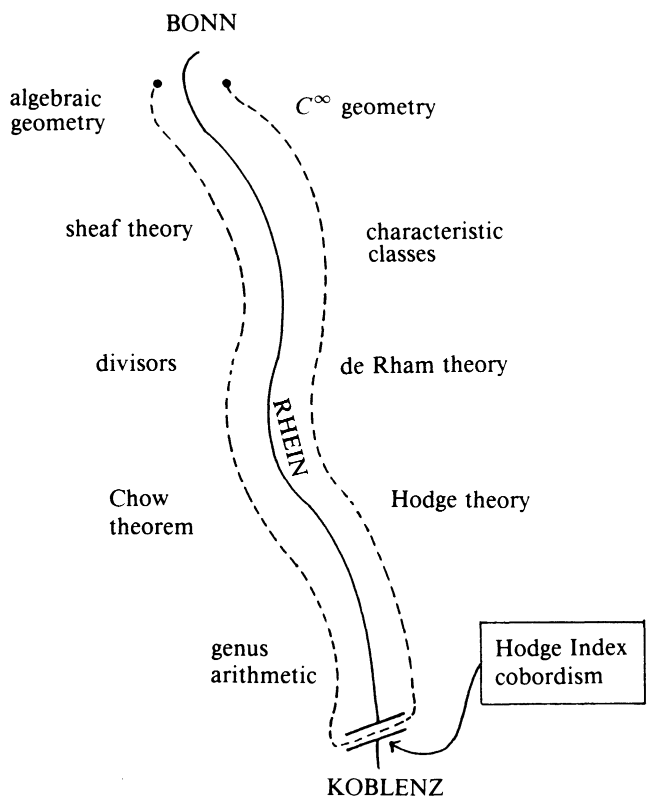


FIGURE 3

As you see, he left nothing out.

As my time is limited, I can really say no more about his proof — rather, let me emphasize that this theorem again fits into my general topic of the constraints of topology on analysis, and also establish the theorem more firmly in the line of a fixed point formula. The point is that on the left, the expression

$$\sum(-1)^i \dim H^i(M\colon\ O(L))$$

is clearly an integer which depends on the complex analytic structure of M. In some sense you should think of it as the dimension of the space of "*virtual solutions of the analytic system of differential equations* $\bar\partial f = 0$ *on* M." On the other hand, on the right-hand side, we find a purely C^∞-invariant of M. It is not purely *topological* in the sense that it doesn't depend only on "purely continuous" concepts. The calculus has entered, but only to the first order, so to speak. Once we know how "twisted" the space of tangents over M is and how twisted L was — all C^∞ concepts — then the right-hand side is determined.

This beautiful theorem refuses to die. We had hardly gotten used to it when Grothendieck produced a magnificently original proof purely in the context of algebraic geometry. Then ten years later or so Atiyah and Singer gave us their general index theorem which generalizes the theorem to all elliptic complexes over M, and again using a new set of ideas. And quite recently we have come to realize that the anomaly calculations of supersymmetric field theory throw new light on this old question. I cannot do justice to all these developments here, but would like to end with some remarks on one of these developments in which I had a personal hand.

Michael Atiyah and I had become very good friends and collaborators by the early sixties and the first proof of his and Iz Singer's index theorem was in fact unveiled in a joint seminar all three of us had started at Harvard during Atiyah's visit in 1963. But the idea of developing a "fixed point formula," to go with the new sheaf theory — and in the context of elliptic complexes — only occurred to us when we met at the Woods Hole Conference of 1964. I think it was a conjecture of Shimura that put us on the right track, as well as some very obscure parts of Eichler's work in number theory.

Recall that as we already saw in the classical case, the Lefschetz number, $L(\varphi)$, was computed in terms of the fixed points by the Hopf formula:

$$\mathscr{L}(\varphi) = \sum_p \mu(p),$$

where the local multiplicities were integers given by the local degree of φ at p. We found the analogous expression in all elliptic situations, but here let me explain it to you only in the complex domain. In this context one of course starts with a *holomorphic* map

$$\varphi\colon M \to M$$

and assumes also that in the general case of a twisting line bundle L, that φ has a lifting, $\hat{\varphi}$, for L over M. Thus we have the diagrams:

$$\begin{array}{ccc} L & \xrightarrow{\hat{\varphi}} & L \\ \downarrow & & \downarrow \\ M & \xrightarrow{\varphi} & M \end{array}$$

Such a lifting induces a homomorphism

$$H^i(\hat{\varphi})\colon H^i(M; O(L)) \to H^i(M; O(L)).$$

And so one also has a well-defined holomorphic Lefschetz number

$$\mathscr{L}(\hat{\varphi}) = \sum (-1)^i \operatorname{Trace} H^i(\hat{\varphi}).$$

Now what we did to find the appropriate measure μ_p to go with $\mathscr{L}(\hat{\varphi})$ under the assumption that the fixed points of φ were isolated and nondegenerate. Indeed, under these conditions we found that

$$\mu_p = \frac{\hat{\varphi}(p)}{\det(1 - d\varphi_p)}$$

were

$$d\varphi_p \colon T'_p \to T'_p$$

is the holomorphic differential of φ at p. In short, our version of the Lefschetz formula therefore reads

$$\mathscr{L}(\hat{\varphi}) = \sum \frac{\hat{\varphi}(p)}{\det(1 - d\varphi_p)}, \ |\varphi(p) = p,$$

so that for the sheaf O itself, for example, when no lifting is needed one has

$$\mathscr{L}(\varphi) = \sum \frac{1}{\det(1 - d\varphi_p)}.$$

We were amazed at first that in this context the "measure" μ_p was actually a complex number, even though in this untwisted case the $\mathscr{L}(\varphi)$ had to be an integer. And Michael recounts in his recollection of that summer that our first attempts to check this formula for elliptic curves with the assembled experts on that subject was a failure! But luckily we didn't believe the experts and pressed on.

I like to think of these formulas as yet another example of the constraints that topology places on analysis. For if we consider the eigenvalues of $d\varphi_p$, say $\lambda_1 \cdots \lambda_d$, then the fixed point formula — read the other way around — puts a remarkable numerical restraint on these eigenvalues:

$$\sum_p \frac{1}{\prod_i (1 - \lambda_i)} = \mathscr{L}(\varphi).$$

Ah! There are so many facets of these formulas I would like to divulge here — but I am afraid my time is up. Still I cannot let you go without at least hinting to you that we are again in an era of new insights which — for instance — have brought about the first refinements of these restrictions on these eigenvalues in the last 25 years. The impetus has this time been from physics, a wonderful byproduct of the newly created vigorous exchange of ideas between our cousins and us.

Thus the much debated physical dreams of string theory have at least had one quite concrete mathematical consequence. The unexpected novelty here was to consider the Lefschetz number in our sense but applied to an *infinite dimensional twisting* bundle L. Thus stimulated by discussions with the topologists P. Landweber, R. Stong, and S. Ochanine, E. Witten predicted that if one twisted $\bar{\partial}$ by a q-dependent bundle which has an "infinite product expansion" of the form:

$$L_q = \bigotimes_{n=1}^{\infty} \Lambda_{q^n} T^* \otimes \Lambda_{q^n} T \otimes S_{q^n}(T^*) \otimes S_{q^n}(T).$$

Then $\chi(M; L_q)$ will have certain quite new "rigidity properties."

I will not even *try* to explain this formula to the uninitiated. Let me simply describe it as the 20th century view of the following formula going back to Gauss and Jacobi:

$$\varphi(\lambda) = \frac{(1+\lambda)}{(1-\lambda)} \prod_{n=1}^{\infty} \frac{(1+\lambda q^n)(1+\lambda^{-1}q^n)}{(1-\lambda q^n)(1-\lambda^{-1}q^n)}$$

for an elliptic function on the torus

$$T_q = C^*/\{q^n\}.$$

I have just returned from a conference on elliptic cohomology where C. Taubes presented his proof of these new conjectures — with a little old man's help from me — which exploits this old Lefschetz formula to the hilt! It is also clear that these physics-inspired ideas are producing a new brand of "elliptic topology" — which someone recently referred to as the beginnings of 21st century K-theory.

In short, I can report to you that once again a quite new point of view has rejuvenated old mathematics — and some old mathematicians — and on this 100th birthday celebration of our Society, who can wish for better news!