Commentary on Algebra

B. GROSS AND J. TATE [1]

The problem of Artin concerning the equality of the right and left dimensionality of a division ring extension was settled negatively by P.M. Cohn and has recently been completely killed by A.H. Schofield, who gave examples in which the two dimensions are arbitrary integers > 1.

Brauer's results on modular characters, and blocks in the decomposition matrix, were useful in the classification of the finite simple groups — one of the great mathematical achievements of our time. Brauer's proof of the Artin conjecture on induced characters has remained a central result in finite group theory; as a technical tool it has also proved extremely useful in reducing various problems on Galois modules to the case where the Galois group is elementary or even cyclic. It was also an important step in the generalization of class-field theory to the non-Abelian case, though not perhaps a "decisive one." Artin's belief that "whatever can be said about non-Abelian class field theory follows from what we know now," and that "our difficulty is not in the proofs, but in learning what to prove," seems overly optimistic. In the late 1960s, Langlands recognized the connection between non-Abelian Galois representations and automorphic forms on reductive algebraic groups, and told us essentially what to prove. But now, twenty years later, the proofs have been carried out in only a few special cases. Using techniques of base change for the group GL(2), Langlands himself showed how to establish the holomorphy of Artin's L-series attached to 2-dimensional representations of solvable Galois groups, and thereby prove that such representations come from automorphic forms. A converse (over $\mathbf{Q}$) was established by Deligne and Serre for holomorphic modular forms of weight one: every such form gives rise to a 2-dimensional Galois representation with odd determinant.

One important aspect of our present view is that the theory of ordinary complex representations of finite Galois groups, i.e., the theory of "Artin

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motives", cannot be separated from the theory of general motives, i.e., from the theory of systems of ℓ-adic representations of Galois groups coming from Grothendieck's étale cohomology of algebraic varieties. This theory was not even dreamed of at the time of the Princeton Bicentennial Conference in 1946.

Commentary on Algebraic Geometry

HERBERT CLEMENS[1]

Of the four problems listed at the beginning of "Algebraic Geometry," only one which explicitly appears seems in retrospect to have been fundamental to the development of the discipline in the ensuing forty years, namely the first — the extension of the Riemann–Roch theorem to higher dimensional varieties. In fact, with the advent of index theory in the fifties and the relative formulation of the theorem by Grothendieck somewhat later, the Riemann–Roch theorem has been generalized and applied far beyond the wildest dreams of the participants at the 1946 conference! The profound nature of those subsequent successes make the other three problems, as stated, seem much more naive and superficial by comparison. Problem (2) is curious — there are indeed easy topological obstructions. More interestingly perhaps, there are "differential-geometric" obstructions to mapping complex lines into higher dimensional varieties; this is studied via a global geometric version of classical Nevanlinna theory [G]. One other problem hinted at in (3), that of determining when a variety is rational, is quite significant, and is beginning to be attacked successfully by Iskovskih and his school. A class of varieties which give many interesting and "controllable" examples is that of "conic bundles," that is, algebraic fiber spaces whose generic fiber is a rational curve (rational curves have a distinguished divisor class of degree two). This class has provided the way to an example of a "stably" rational variety which is not rational. An important related problem which remains outstanding is that of determining whether a smooth deformation of a smooth rational manifold need be rational. The prime candidate for a counterexample, the cubic fourfold, has resisted several assaults.

Further along in the problem list, the suggestion of looking at Halphen's work on classification of curves in P^3 by genus and degree was indeed taken up and straightened out in characteristic zero by Gruson–Peskine [GP]. (It turns out that even in characteristic zero Halphen's analysis was not complete.) Several techniques related to this problem, perhaps most notably the

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vast generalization of the notion of a Koszul resolution, have given algebraic geometers much deeper insight into the resolution of the ideal sheaves of curves in projective space and in more general settings.

With their discussion of Hodge's famous conjecture, the authors of the problem list were on target, though again in unexpected ways. This conjecture or problem (depending on whether one is a "believer" or not), remains a very important outstanding problem in complex geometry. Perhaps even more importantly, work in Hodge theory motivated by the conjecture has led to some of the deepest mathematics in complex geometry and related fields. For example, the theory of intersection cohomology of Goresky and MacPherson combines with Hodge theory to give the decomposition theorem of Beilinson–Bernstein–Deligne–Gabber for the Hodge theory of projective morphisms between complex varieties and Saito's theorem giving pure Hodge structures on intersection cohomology. See [S] for an overview of these results.

Finally, the citation of the problem of minimal models, central to birational classification theory, was again the correct insight. It is only in the last ten years, with the advent of Mori's theory [K] of classifying varieties by finding rational curves on them, that the centrality of the minimal model question has again been appreciated. The great technical stumbling block, that of handling exceptional sets of codimension greater than one, is finally being dealt with via the theory of "extremal rays" and "flips," thus allowing the construction of minimal models in dimension three.

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Commentary on Differential Geometry

ROBERT OSSERMAN [1]

The discussion of differential geometry, at least as reported by Carl Allendoerfer, is clearly more notable for what (and who) is left out than for what is included. To start with "who," the names Cartan, Chern, Weyl, and Myers are nowhere mentioned. Their absence is even more striking in the context of the discussion. J. H. C. Whitehead is reported to have said of a recent Bochner theorem, that "This is the first time that anyone has got a topological result out of the Ricci tensor, without using the full curvature tensor." However, in 1941 Sumner Myers had published his (now) well-known result that a complete Riemannian manifold with a positive lower bound on Ricci curvature must be compact and have a finite fundamental group.

The most glaring omission of all is Chern's intrinsic proof of the general Gauss–Bonnet theorem. The work was done in 1943 in Princeton's own backyard at the Institute for Advanced Study, where Chern was spending two years at the invitation of Veblen. It was published in two papers that appeared in 1944 and 1945 in Princeton's own house journal, the *Annals of Mathematics.* Its direct antecedents are the Gauss–Bonnet theorem of Heinz Hopf for hypersurfaces, the tube theorems of Herman Weyl which gave the form of the higher-dimensional Gauss–Bonnet integrand, and the theorems of Fenchel, Allendoerfer, and Weil. Yet with Veblen, Hopf, and Allendoerfer all present, and the subject of generalized Gauss–Bonnet theorems under discussion, there is no mention of Chern's work. (Again the *caveat:* there is no mention in the written summary, prepared by Allendoerfer.)

It would appear that the session opened with presentations by each of the discussion leaders, V. Hlavaty and T.Y. Thomas, of their own recent work. (In the case of Thomas, the subject was not differential geometry, but dynamical systems.) Bochner then described his own recent results, that have since acquired the name of "the Bochner technique." The responses indicate a clear appreciation of the value of Bochner's contribution, which subsequent

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developments have amply confirmed. In particular, the Bochner "vanishing theorem" to which Whitehead alludes in the comment cited above shows that certain geometric constraints imply the vanishing of harmonic forms which in turn leads to the vanishing of homology (or cohomology) classes via Hodge theory. This was the precursor of the renowned Kodaira vanishing theorems of the fifties, in addition to a steady stream of related results. As an indication of the breadth of that stream, two recent surveys — one by Pierre Bérard: "From vanishing theorems to estimating theorems: the Bochner technique revisited," in the *Bulletin of the American Mathematical Society*, Vol. 19, No. 2, October 1988, and the other a monograph by Hung-Hsi Wu: "The Bochner Technique in Differential Geometry," appearing in *Mathematical Reports,* Vol. 3, Part 2, January 1988 — have extensive bibliographies with almost no overlap.

The Bochner results lead naturally to the final circle of ideas discussed: the relations between curvature and topology. Specific references are to the theorems of Preissmann, Cohn–Vossen, and Gauss–Bonnet. All the results have in common with Bochner's that they relate curvature to topology, but whereas Bochner's conclusions regard homology, Preissmann looks at the fundamental group. Since the work of Gromoll–Wolf and Lawson–Yau in the early 70s, the subject of curvature and the fundamental group has been more and more intensively studied. Surveys may be found in recent articles by Eberlein, and the 1985 monograph by Ballmann, Gromov, and Schroeder.

As for the Cohn–Vossen theorem, subsequent results have been most interesting in the two-dimensional case. The original theorem is the bound

$$\int_M K dA \leq 2\pi\chi$$

for the total curvature of a complete 2-dimensional Riemannian manifold M in terms of the Euler characteristic χ of M. Refinements by Robert Finn and Alfred Huber include the result of Huber that if $\chi = -\infty$, then also $\iint K dA = -\infty$. In particular, finite total curvature implies finite topology. A simpler proof of that result was recently obtained by Brian White. Another consequence of Huber's Theorem is that nonnegative curvature outside a compact set implies finite topology.

For complete minimal surfaces, a stronger version of the Cohn–Vossen inequality holds, and the strengthened version plays an important role in the recently elaborated theory (and examples) of complete *embedded* minimal surfaces.

In higher dimensions, only scattered results are known: for dimension 4, under certain curvature restrictions (Poor, Walter, Greene/Wu), and in arbitrary dimension, for a restricted class of manifolds (Harder). Recent work of Cheeger and Gromov has shed light on the possible values of Cohn–Vossen type integrals and other integrals of characteristic classes for complete

Riemannian manifolds of finite volume, with a bound on sectional curvature. Also, a number of recent papers have been devoted to studying the topology of manifolds whose sectional curvature is nonnegative (or even identically zero) outside a compact set.

The Gauss–Bonnet Theorem states that for a compact surface M,

$$\int_M K dA = 2\pi\chi .$$

Among its most important consequences is that if the curvature K is of one sign everywhere on M, then the Euler characteristic χ must have the same sign. In the higher-dimensional Gauss–Bonnet theorem, the integrand K is replaced by a fairly complicated expression in the components of the curvature tensor. An obvious question is the relation between the sign of the sectional curvature and the sign of χ. In four dimensions, it follows from Myers' theorem cited above that if the sectional curvature is positive, then the first Betti number $b_1(M) = 0$, and then by Poincaré duality $b_3(M) = 0$, so that $\chi(M) = 2 + b_2(M) > 0$. A different argument due to Milnor shows that in four dimensions, if the sectional curvature is either everywhere positive *or* everywhere negative, then $\chi(M) > 0$.

In the case of positive sectional curvature, there was the possibility that the Gauss–Bonnet integrand would be pointwise positive. However, Geroch showed in 1976 that for dimensions six and higher the integrand could be negative even if the sectional curvature is positive.

More important than these particular results were the broad developments leading from Chern's intrinsic treatment of the Gauss–Bonnet theorem to the general theory of vector bundles and characteristic classes, and to the Atiyah–Singer index theorem. Although the first steps in that direction had already been taken, it would have required enormous prescience to foresee where they might lead. On the other hand, the prediction that links between curvature and topology were the wave of the future has been fully borne out in the theorems of Rauch–Berger–Klingenberg, Gromoll–Meyer, Gromoll–Cheeger, Lawson–Gromov, Schoen–Yau, the recent results of Ballmann–Brin–Eberlein–Burns–Spatzier, and others far too numerous to mention.

Commentary on Mathematical Logic

YIANNIS N. MOSCHOVAKIS [1]

According to McKinsey's account of the session in mathematical logic, "the discussion revolved about a single broad topic — decision problems." A decision problem calls for an "effective method" (an "algorithm") which will decide whether an arbitrary object from some set D has or does not have a given property P. It can be solved "positively" by producing an algorithm which does the required job. To solve it "negatively" and prove that the given decision problem is *unsolvable*, you must establish that the function which needs to be computed *is not computable by any algorithm*, and for this you need a precise, mathematical notion of computability. The pioneering work of Church, Gödel, Kleene and Turing in the thirties had produced just such a notion — *recursiveness* — and by 1946 essentially everyone in the field had accepted *Church's Thesis*, the claim that (formal) recursiveness and (intuitive) computability are identical.

In the natural course of events, the first unsolvability results were inside logic, including Church's fundamental result that *the problem of validity in predicate logic is unsolvable.* Just before the Princeton meeting Post had proved that *the word problem for semigroups is unsolvable*, and Church now suggested three more candidates for unsolvability results "of a more standard mathematical character": "the word problem for groups and the problems of giving a complete set of topological invariants for knots and for closed simplicial manifolds of dimension n."

The first of these questions has been answered by Boone and Novikov (independently) who proved in the late fifties that *there exist finitely presented groups with unsolvable word problem.* This was a very active research area in the fifties and sixties, one of its best results (in the experts' opinion) being Higman's (1961) Embedding Theorem: *a finitely generated group can be recursively presented if and only if it is isomorphic to a subgroup of a finitely*

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presented group. Markov proved in 1958 that *the decision problem for homeomorphy of closed, simplicial manifolds in any dimension $n \geq 4$ is unsolvable,* and this is generally assumed to rule out any acceptable solution to the classification problem for these dimensions. In dimension 2 manifolds have been classified by classical methods, and nothing much is known now about the most difficult dimension 3. The problem of classifying knots is apparently still open — no matter how you make it precise.

What is quite astonishing about this extensive discussion of decision problems is the absence of any reference to Hilbert's 10th, the only decision problem in Hilbert's famous list: *to determine whether an arbitrary polynomial with integer coefficients has any integer roots.* Hilbert's 10th was proved unsolvable in 1970 by Matijacevič, building on a great deal of groundwork laid by Davis, Putnam, and Julia Robinson. It is generally regarded as the most impressive among "applications" of recursive function theory to fields outside logic.

McKinsey next makes a passing reference to the problem "whether or not particular questions are decidable in a given theory," accompanied by the suggestion that the Riemann Hypothesis may be a good candidate for such an example. Actually this is a bad example, since the Riemann Hypothesis has the property that its consistency with any minimally strong axiomatic theory implies its truth. Much better candidates for such examples are simple statements of set theory, like the axiom of choice or the continuum hypothesis which had already been proved consistent with Zermelo–Fraenkel set theory by Gödel and were generally believed to be independent of ZF. (Their independence was proved in 1963 by Paul Cohen, who introduced in the proofs the seminal new method of *forcing* and earned with his results the only Fields Medal ever given for work in logic.) Why were these not mentioned — particularly with Gödel in the group? Even now, however, when the question of independence of "natural, mathematical" statements comes up, logicians look for "logically simple" examples, as close to number theory as possible. The first simple and elegant example of this kind was a Ramsey-type partition theorem which can be expressed in the language of number theory, and was proved true and independent of the classical Peano axioms by Paris and Harrington in 1977, building on earlier work of Paris. A lot has happened in this area since then, particularly by Friedman and his school.

McKinsey reports a brief reference by Quine to "restricted arithmetic" and a long passage on "many valued logics," not among the most studied areas of logic today. Other than that, McKinsey's account mentions just three other topics which were touched upon, and which are more relevant to current work in logic.

"The analogy between recursive sets of integers and Borel sets of real numbers was stressed by Tarski, who pointed out the possibilities of proving analogous theorems and of developing a single theory to include both as special cases." Most likely Tarski was referring to the work of Mostowski, who was pushing then the stronger analogy between *recursively enumerable* sets of integers and *analytic* sets of reals, and I wish the text recorded whether Kleene responded and how. Kleene certainly learned of Mostowski's proposal — if not in 1946, shortly afterwards — and he published in 1950 (at Mostowski's suggestion) his result that *there exist disjoint, recursively enumerable, recursively inseparable sets*, which disproved the proposed strong analogy and suggested that the weaker analogy suggested by Tarski was "defective." It took about eight more years before the *hyperarithmetic sets* of integers were introduced (independently) by Davis, Kleene, and Mostowski and the "correct" analogy between *them* and the Borel sets was proposed by Addison and firmly established by Kleene's proof of what we now call *the Suslin–Kleene Theorem. Effective descriptive set theory* was born of these results, it grew enormously in the seventies under the influence of determinacy and large cardinal hypotheses and has certainly provided some version of Tarski's "single theory" of definability in the integers and the reals. It is one of the main research areas of logic today.

There is a tantalizingly brief remark that "Kleene discussed the limitations which general recursiveness may place on quantitative proof." Although somewhat cryptic, the reference must be to *realizability theory* which Kleene was developing just then. The gist of the idea is that a *constructive assertion* of some statement A can be understood (at least partially) as a *classical assertion* that a recursive function exists which *realizes* some constructive (or computable) aspect of A. In a seminal paper published in 1945, Kleene gave the first precise definition of *number realizability* and announced Nelson's result, that *every theorem of intuitionistic number theory is realizable.* The theory took off after that, different realizability interpretations of many intuitionistic theories were introduced, and they have provided one of the main tools for the metamathematical study of constructive mathematics. More recently, notions from realizability theory have invaded theoretical computer science, primarily in the theory of programming languages.

Finally, McKinsey devotes two paragraphs to the "spirited discussion" on Gödel's proposal of "a particular tremendous enlargement of a formal system — which would allow uncountably many primitive notions and allow the notion of an axiom to depend on the notion of truth." This threw me for a while, until I looked up Gödel's *Remarks before the Princeton Bicentennial* in Davis' most useful source book, *The Undecidable*, and recalled what it was about: Gödel talked about set theory, as one might expect, even though the word "set" never occurs in McKinsey's account. He introduced the notion of *ordinal definability* and announced that *the class of hereditarily ordinal definable sets is a model of ZF with the axiom of choice*, and he also talked about *axioms of infinity*. Intuitively, an axiom of infinity is an assertion that very large sets exist, in some specific way of measuring "size." Quoting now from the account of his remarks in *The Undecidable,*

> There might exist, e.g. a characterization [of axioms of infinity] of the following sort: An axiom of infinity is a proposition which has a certain (decidable) formal structure and which in addition is true . . . the following could be true: Any proof for a set theoretic theorem . . . is replaceable by a proof from such an axiom of infinity.

In his account, McKinsey focused on the *logical* aspect of Gödel's proposal — about nonconstructive axiom systems — which has not yet led to anything substantial. For set theory, however, the idea that we should look to plausible axioms of infinity in order to settle the classical open problems about sets has been one of the dominant themes of the field and has motivated some of its most substantial successes in the last forty years.

It is quite a sobering experience to read this account of McKinsey's and try to relate it to what is happening in mathematical logic today. Ours is a very young field, indeed: there was no *model theory* in 1946; Gödel was practically the only person in the west interested in *set theory*; *recursion theory* was viewed almost exclusively as a tool for applications outside logic and to proof theory; and even in the parts of the discussion on *proof theory*, there is no mention of the work of Herbrand and Gentzen, which has influenced the theory of proofs since then much more than, say, many-valued logics. After the subsequent work of some who attended the Princeton meeting and many others later, we can find today work in all these research areas which is as deep and as mathematically sophisticated as any in pure mathematics. In fact, this increasing "mathematization" of the field has prompted recent accusations that "we have lost our roots" — but this is not our topic here.

The first AMS Summer Research in Symbolic Logic was held in Cornell in 1957. Its list of participants is an incredible who-will-be-who of young logicians and its proceedings are full of exciting new beginnings. A lot had happened in those ten years. Ward Henson has commented that in contrast to the 1946 meeting, "the Cornell meeting we can recognize as one of ours."

Commentary On Topology

WILLIAM BROWDER [1]

The year 1946 and the Princeton Bicentennial Conference discussion of topology could be said to celebrate the birth of one field (compact transformation groups) and the puberty of another (homotopy theory, and algebraic topology).

The Fifth Problem of Hilbert commanded considerable attention in 1946, but a "central question" at the time is still unsolved: if a compact group G acts effectively on a manifold is G necessarily a Lie group? Many other related problems were soon solved such as the theorem of Montgomery–Zippin and Gleason: a topological group which is a manifold is a Lie group.

On the other hand, the focus of attention in this area soon moved toward problems of classifying actions, a typical question being: how much does an arbitrary compact group action on a familiar space (e.g., disk, sphere, euclidean space) resemble (e.g., homeomorphic, diffeomorphic, homotopy equivalent, or have similar fixed set) a familiar action (e.g., linear action), the point of view pioneered by Smith in the 30s. The specific question asked of this type: "does a cyclic group acting on a euclidean space fix some point?" was soon answered in the negative (Conner–Floyd). But this class of questions became the central focus in this area, and compact transformation groups became a testing ground and application area for every new development in topology, with great success.

The field of algebraic topology, on the other hand, was already in a golden age, with the development of homology and cohomology theory, homotopy groups, obstruction theory, and the emerging perception of a general context for algebraic topology, the functorial approach, and homological algebra. Steenrod had just begun to discover cohomology operations in calculating $\pi_n(S^{n-1})$, but the "mass production" methods made possible by the spectral sequence of Leray were not yet visible. A decade or so later the spectral sequence would have made itself felt, most notably in topology with

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the Leray–Serre and the Adams spectral sequences, but also in applications to other areas.

In fact, the introduction of topological and, more specifically, *homological* methods into so many diverse areas of mathematics is a striking development hinted at in this conference in many areas, but not then realized. The introduction of sheaf theory into complex analysis, homological algebra, and cohomology of groups into number theory and algebraic geometry, and later the rise of K-theory in its multifarious guises has changed the shape of numerous areas of mathematics.

As algebraic topology developed after 1946 with its flourishing development using spectral sequences, cohomology operations and fiber bundle theory, a new theme began to be sounded strongly in the middle 1950s and 1960s: differential and geometric topology. With Thom's work on cobordism theory, a new approach to studying manifolds using homotopy methods emerged. With Milnor's discovery of the exotic differentiable structures on S^7 a whole new field opened up, the classification of smooth structures on a given topological manifold. And with Smale's discovery of the h-cobordism theorem and the Poincaré conjecture in dimensions greater than 4, the possibility opened of attacking many of the old geometric problems using the well-developed and powerful machinery of algebraic topology. These methods of homotopy theory — algebraic topology, characteristic classes, fiber bundle theory — became the everyday tools of geometric topologists in "surgery theory," the systematic, *algebraically controlled* method of cutting and pasting manifolds (initially smooth manifolds).

Pioneered by Milnor and Kervaire in studying smooth structures on $S^n, n \geq 5$, surgery theory soon was applied to studying how to characterize the homotopy type of smooth manifolds and classify (up to diffeomorphism) the ones in a given homotopy type (Browder and Novikov) always with dimension ≥ 5. Initially developed in the context of the trivial fundamental group, the generalization of the theory to the nonsimply connected case by Wall opened up a whole new connection of topology with number theory, through K-theory of hermitian forms related to surgery obstruction groups. Among the outstanding accomplishments of the theory were Novikov's proof of topological invariance of rational Pontryagin classes and Sullivan's analysis of the *Hauptvermutung* for manifolds of dimension ≥ 5 (surgery theory having been adapted to "piecewise linear" manifolds).

There was a parallel development of piecewise linear (PL) manifold theory in the 1950s and 60s, which was hinted at in 1946 in Whitehead's thinking, as was the development of algebraic K-theory which had its birth in Whitehead's simple homotopy theory. But nobody in 1946 could have anticipated that all these diverse methods would come together 25 years later to give Kirby and Siebenmann's comprehensive theory of PL triangulation of manifolds of dimension ≥ 5, and another decade later, with the addition of the decomposition space methods pioneered by Bing, lead to Freedman's topological classification of 1-connected 4-dimensional manifolds.

The reference of the conference to "the need of studying not only the [homology] groups involved, but also the homomorphisms connecting them" leads quickly (in retrospect) to exact seqeunces, homological algebra, the Eilenberg–Steenrod axioms for homology, and the general abstract approach, which became so popular. The Eilenberg–Mac Lane spaces, $K(\pi, n)$'s, the universal models for cohomology led to the Postnikov tower methods, calculations of Serre and Cartan, and to more general cohomology operations (of higher order) and similar methods in homotopy theory (Toda brackets). The crowning achievement of this approach was the Adams spectral sequence, a powerful tool for calculating stable homotopy from cohomology, which led to Adams' solution of the Hopf invariant 1 problem (e.g., there are no non-singular pairings $\mathbf{R}^n \times \mathbf{R}^n \to \mathbf{R}^n$ if $n \neq 1, 3$, or 7).

Into this increasingly formalized algebraic study came another dramatic development of the 50s (totally unanticipated in 1946), the Periodicity Theorem of Bott, the development of topological K-theory and interesting *extraordinary* cohomology theories, whose existence was suggested by the Eilenberg–Steenrod axioms. Bott's bizarre result, proved initially with Morse Theory, that the homotopy groups of the unitary groups were stably periodic, made possible the K-theory of stable complex vector bundles. Combined with analytic ideas, the first results on indices of elliptic operators from the Russian school, and no doubt inspired by topological methods in algebraic geometry, as in Hirzebruch's book, Atiyah and Singer created their Index Theory for elliptic operators, and another chapter in the reunification of topology with other fields of mathematics opened.

At the same time K-theory quickly found its uses in topology both in the geometric side (e.g., nonparallelizability of $S^n, n \neq 1, 3, 7$ and in surgery theory), but also as a powerful new tool in homotopy theory, spearheaded by Adams' solution of the vector fields on spheres problem, and Atiyah's K-theory proof of Adams' Hopf invariant one theorem. Bordism theory, another new, geometrically defined, extraordinary homology theory soon was put to use by homotopy theorists and these two theories and derivative theories are now central tools in the subject.

All the new influence of differential geometry, smooth manifolds, and analysis in topology and homotopy theory was something not envisioned in the 1946 conference, but if Pontryagin had been there, perhaps some hint of his theory of framed manifolds would have emerged.

The three-dimensional Poincaré conjecture was, in 1946, and remains today, an outstanding problem. Even though enormous progress has been made in low dimensional topology since 1946, it is still difficult to foresee a solution in the near future.

On the other hand, beginning with the classical theorems of Papakyriakopoulos in 1956, and most recently the work of Thurston and his school in the 1980s, many deep problems in 3-manifolds have been solved. Again, in Thurston's approach, we see new geometric input into topology, by showing many 3-manifolds have "geometric" structures (e.g., metrics of constant curvature) so that geometric methods can be employed. The positive solution of the Smith conjecture (any periodic transformation of S^3 fixing a point is homeomorphic to a linear one) is the product of this "geometrization" method together with differential geometric input (Meeks–Yau) and algebraic input (Bass). When there is no fixed point this problem is still open for most groups.

But the biggest surprise since 1946, and probably of this century, is the enormous qualitative difference between topology in dimension 4 and all other dimensions, and the methods of physics on which these results are based. Donaldson's remarkable first result in 1982, proved using Gauge Field Theory Methods, that the only positive definite intersection forms on closed smooth 1-connected 4-manifolds are diagonal, caused universal astonishment. The corollary which followed from this and Freedman's topological classification theorem mentioned above (proved at almost the same time) showed that the smooth structure on $\mathbf{R}^4$ is not unique, a statement false in every other dimension. Since then, it has been shown $\mathbf{R}^4$ admits infinitely many different smooth structures, as do many closed 4-manifolds. The theory of smooth 4-manifolds is of an entirely different nature from higher or lower dimensions. In particular the kind of algebraic topological invariants which describe the whole story, by surgery theory, in higher dimensions, are irrelevant to the rich phenomenology of dimension 4.

This new influence of physics has also made itself felt strongly in algebraic topology with many new points of view from quantum mechanics contributed by Witten.

One might look at the history of topology since the 1946 conference in two complementary aspects:

(1) The enormous development of all the themes represented there, homology and homotopy theories, fiber bundles, group actions, and combinatorial topology, and the spreading of the central parts of these methods (in particular homology and fiber bundle theory) into other areas, until there is hardly an area of mathematics today in which topology does not play a prominent role, as well as in parts of theoretical physics and molecular biology.

(2) An influx of new ideas into topology, from differential geometry and calculus of variations, differential equations, algebraic geometry and number

theory, to the most recent striking input of ideas from physics. Coupled with the internal development of the subject, it has led to enormous progress.

One might say that topology has developed from being a field of mathematics in 1946, somewhat isolated in its subject matter and approach, into a meeting ground for many different areas of mathematics, an area in which interaction and cooperation with other fields has led to enormous progress for all sides.

In this, topology mirrors, on a smaller scale, the change in position of mathematics with respect to other sciences.

Herman Weyl's worries "...maybe it [mathematics] has become too hard for us unless we are given some outside help..." were prophetic. In topology, with respect to other fields of mathematics and physics, we have received abundant outside help. But topology has richly repaid its debt with an enormous contribution to other areas, including physics.

Commentary on Probability

J. L. DOOB [1]

The basic difference between the roles of mathematical probability in 1946 and 1988 is that the subject is now accepted as mathematics whereas in 1946 to most mathematicians mathematical probability was to mathematics as black marketing to marketing; that is, probability was a source of interesting mathematics but examination of the background context was undesirable. And the fact that probability was intrinsically related to statistics did not improve either subject's standing in the eyes of pure mathematicians. In fact the relationship between the two subjects inspired heated fruitless discussions of "What is probability?," and thereby encouraged the confusion between probability and the phenomena to which it is applied. (This confusion still plagues the subject.) Furthermore in 1946 there was no advanced probability text leading from definitions to basic theorems, although at the research level there were specialized articles and books written under the assumption that the readers would not worry about the lack of a rigorous axiomatic basis underlying the work. Nevertheless Kolmogorov had published the basis for mathematical probability in his (German) 1933 monograph, including the identification of random variables as measurable functions and expectations as integrals. Furthermore he had there introduced conditional expectations as functions, and had defined measures on coordinate spaces of arbitrary dimensionality, thereby providing models (the coordinate functions) of families of random variables. Unfortunately the significance of his work was not appreciated for years, and some mathematicians sneered that probability should not bury its spice in the bland soup of measure theory, that perhaps probability needed rigor, but surely not *rigor mortis.*

The carrying out of the program inspired by Kolmogorov's additions was delayed by a factor usually ignored by historians of mathematics, the surprisingly long time it takes for new mathematical ideas to be absorbed, for new points of view to be taken seriously. Although Lebesgue measure dates back to 1902 it was only noted (by Fréchet) in 1915 that for measures on Borel

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fields of subsets of abstract spaces the corresponding measurable functions and integrals can be defined and manipulated in precisely the same way as in the context of Lebesgue measure on a linear interval. And even thereafter, for a long period the elementary manipulations of measurable functions and their integrals, now an early part of the graduate education of every student of analysis, were avoided, or rejustified as needed when the measure was not Lebesgue measure. "Measure" meant "Lebesgue measure" as against the more general "positive additive set function"; this convention, together with the use of (Riemann) Stieltjes integrals provided a terminological delaying action against the acceptance of general measure theory. Probabilists were perhaps more inhibited in applying measure theory than other analysts because of their private measure terminology and because their probability measures were usually not Lebesgue measure. It is not much of an exaggeration to assert that probabilists are the only mathematicians who ever evaluate an integral by other than the antiderivative method of elementary calculus. The ironic fact is that the role of measure theory in probability has embarrassed many probabilists and still embarrasses some who like to think that mathematical probability is not a part of analysis. It is noteworthy that Kolmogorov himself, in his 1933 monograph that made mathematical probability rigorous by basing it on measure theory, hindered the acceptance of his own ideas by being chary in his use of measure theory terminology. For example, instead of defining a real random variable as a real measurable function he avoided the use of the adjective, going back to the defining property that the inverse image of an interval under the function should be a set in his specified Borel field. Although in an introductory paragraph he stated that his random variables were described elsewhere as measurable functions he did not refer to the customary measure theory terminology in discussing almost everywhere convergence or convergence in measure, and went to the extreme of proving that the limit of a convergent sequence of random variables is a random variable. He obviously felt, and he was right, that the jump from Lebesgue to general measures was psychologically difficult.

With this background it becomes understandable that in 1946 only a few mathematicians were taking mathematical probability seriously as mathematics, that probabilists who were carrying on deep research, for example Paul Lévy, wrote in a mysterious style that discouraged outsiders from entering the field. Yet with the infallible help of hindsight it has become obvious that by the 1940s the level of analysis, in particular that of mathematical probability, was such that there was bound to be a flowering of the latter and its application to all areas involving averaging, some quite unexpected. It would be pointless to list those areas here. Suffice it to write that probability theory has on the one hand penetrated many parts of analysis and on the other hand still has its own special character. Every analyst must now be familiar with some aspects of mathematical probability although only specialists can be expected to follow specific probability research.

Commentary on Fourier Analysis

E. M. STEIN [1]

When the progress of Fourier series is viewed from the perspective of 1946 to the present — a considerable time span in the history of any branch of mathematics — one can better appreciate the paradoxical fact that the rates at which various parts of the subject developed were not at all in proportion to the interest they initially held. This point is brought out when we consider the advances made in the last 40 years in three areas alluded to in Zygmund's remarks, namely (1) the uniqueness of trigonometric series, (2) the problem of convergence of Fourier series, and (3) extensions of the theory to n-dimensions. 1. In the theory of uniqueness of trigonometric series there has been one major result: the theorem of Salem and Zygmund (ca. 1950) that a Cantorlike set of constant ratio of dissection r is a set of uniqueness if and only if 1/r is a Pisot–Varadarajhan number, namely an algebraic integer whose conjugates all lie strictly in the unit disc. Except for this striking result the field of uniqueness of trigonometric series has lain mostly dormant.

2. As for Lusin's problem of the a.e. convergence of Fourier series, the crowning result is Carleson's positive solution in 1966. For higher dimensions (except for Fefferman's observation that the analogue for rectangular convergence does not hold), much still remains to be done. A major problem resisting solution is that of the spherical convergence of Fourier series in dimension greater than 2, as well as the corresponding problem for the eigenfunction expansions for the Laplacian on a compact manifold. Connected with this is the sobering fact that, as subtle and as ingenious as Carleson's theorem is, the ideas in it have as yet not been significantly exploited in other parts of harmonic analysis.

3. It is, however, in the direction of extending other parts of the theory to higher dimensions that there has been in the last 40 years the greatest activity and success. Here we have witnessed a bountiful harvest of results. I have in mind the theory of singular integrals, the related extensions of the Littlewood–Paley theory, and the n-dimensional theory of Hardy spaces; in

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all of these important new points of view were developed, and significant relations within other parts of mathematics have been achieved.

What might the next 40 years hold in store for us? It is impossible not to be fascinated by this question. However, experience teaches us that our present interests may not be much help in wrestling with this puzzle.

Commentary on "Analysis in the Large"

KAREN UHLENBECK [1]

In the opening paragraph on the discussion of analysis in the large, it is commented that the discussion centered on choice of method. Morse is quoted as emphasizing that the categorization into subdisciplines is regarded as "all for convenience, and has nothing to do with essential differences." The emphasis seems to be on existence and uniqueness of solutions. Choice of methods is apparently between calculus of variations and fixed point theorems, although mention is made of the relationship of differential and topological invariants, perturbation in a parameter and generalized curves. Today the major change would be an emphasis on content. The slightly puzzling division into differential geometry, applied mathematics, and something called equilibrium analysis has exploded into dynamical systems, differential topology, minimal surface theory, global differential geometry, analysis on Riemannian manifolds, complex geometry and several complex variables, nonlinear elliptic and parabolic equations, nonlinear hyperbolic equations and a whole realm of various subjects in mathematical physics and applied mathematics. Most mathematicians would not work seriously in very many of these subjects, but there are, of course, more mathematicians. The "choice of method" is less central, although various techniques, such as implicit function theorems (strangely absent from the 1948 article), Fredholm index theory, fixed point theorems, variational methods, complex dynamics, geometric measure theory, analysis of singularities, topological techniques, computer graphics and numerical methods are exchanged and shared by these disciplines. It is convenient in discussing this field to point to the halfway mark (1968). In this year, there was a grand American Mathematical Society Summer Mathematics Institute held in Berkeley on the subject of what had come to be called global analysis. A perusal of the three-volume proceedings (Proceedings of Symposia in Pure Mathematics, Volumes 14–16), or even just their tables of contents, gives one a very good idea of how far this optimistic

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belief that all of nonlinear mathematics could be put in a proper framework by a universal method could be carried. These volumes are divided informally and less mysteriously into what is in fact dynamical systems (vol. 14), differential geometry and infinite dimensional differential topology (vol. 15), and partial differential equations (vol. 16).

In dynamical systems, various current fundamental concepts of structural stability, stable manifolds, entropy, zeta functions and attractors are well-developed, but little contact with physical, geometric, or computer-simulated problems is present. The theory of infinite dimensional manifolds and the study of functions, flows, and topology for them is almost too well-developed, and the techniques for the study of partial differential equations are already very numerous. I noted monotone operators, fixed point theorems, critical point theory, kernels and pseudodifferential operators, basic ideas of characteristics, and the notion of genericity.

Superficially those first twenty years of spectacular abstract technical development bear out the concern of Hermann Weyl, quoted in the final session of the 1948 report. Especially note the quote dating even further back from 1931, "I am afraid the mathematical substance in the formulation of which we have exercised our powers in the last two decades shows signs of exhaustion." In hindsight, it appears to me that global analysis in 1968 was an abstract theoretical technique in search of applications. Much intuition of a more specific mathematical substance did lie behind most of the abstraction, but it was certainly not the fashion to make this transparent.

Analysis in the large became global analysis, went out of favor, and became in its present state something the National Science Foundation calls geometric analysis. The most striking feature of these last twenty years has been a revolution from the general to the particular of which I feel sure Hermann Weyl would approve. Ideas have poured into "analysis in the large" from other parts of mathematics and related sciences. Computers of all sizes from personal to super have revolutionized dynamical systems, not only justifying its theoretical and topological bias, but making contact with complex analysis as well as more applied subjects such as fluid mechanics. Strange attractors and universal bifurcation phenomena are of central interest in many sciences. A book on chaos has been on the bestseller list of the *New York Times.*

Global differential geometry has been enriched by the successful study of a list of specific problems which interact with other areas of mathematics and theoretical physics. I hope I offend no one by citing as an example the Calabi problem, which solves Einstein's equation in general relativity on a complex geometric manifold, has important applications in algebraic geometry, and now provides vacua for string theories in high energy physics. Yau's solution is technically that of the unsophisticated "continuity method," or estimates and the implicit function theorem. Similar interconnections are observed for the Yamabe problem, harmonic maps, minimal surfaces, and Yang–Mills theory.

The calculus of variations plays a central role in the 1948 discussion. By 1968 it had been elegantly abstracted to calculus on Banach manifolds by Palais and Smale. In 1988, every variational problem in geometry or applications is first measured in difficulty by judging how far it deviates from the Palais–Smale criteria for Morse theory, and then studied for specific details. Estimates, hard and soft implicit function theorems, and constructive fixed point theorems are the tools of the trade, while the purely topological fixed point theorems have grown out of favor as being "nonconstructive."

I really cannot be fair here to the new advances which have occurred in all of nonlinear partial differential equations. Elliptic and parabolic equations are in such good shape that often one can ask what solutions actually look like! The last decade has brought a revolution in the understanding of hyperbolic problems, especially that of many physically relevant systems. It might be emphasized that there is in these developments a lot of contact with geometric ideas, computational algorithms, and specific applications intertwined with the fundamental functional analysis.

Finally, in returning to the 1948 discussion, we should note that some of the problems are still only partly solved. Allendorfer's comments connecting differential invariants with topological invariants refer to the theory of characteristic classes then being developed, although I first thought of them as looking forward to the Atiyah–Singer index theorem. From one point of view it was the "Dark Ages." From another point of view, the insight of theoretical physicists such as Witten into global topological invariants via quantum field theory reminds us of the rich possibilities of an unknown mathematical future.

In responding to the 1948 discussion, I have not had much space to point toward the future and the central open questions. However, although geometric analysis does not appear to me unified, the rest of the final paragraph concerning its remarkable size and tendency to absorb the rest of mathematics still seems true.