it would have been so easy to reproduce an entire theory in detail on the pretext of presenting it as necessary for understanding the papers, or even better, with less work, to interlard a branch of science with two or three new theorems, without pointing out which ones! It would also have been so easy to substitute successively all the letters of the alphabet into each equation, numbering them in order to be able to see which combinations of letters the later equations belong to; this could have multiplied the number of equations indefinitely, if we reflect that after the Latin alphabet there is still the Greek alphabet, and after that is used up, there are still German letters, and nothing prevents us from using Syriac letters, and if necessary Chinese letters! It would have been so easy to transform each sentence ten times, carefully preceding each version with the pompous word "Theorem"; or again, to obtain by OUR ANALYSIS results known since good old Euclid; or finally to precede and follow each proposition with a formidable train of special cases! And from among so many possibilities I have not been able to select even one!

Reference

É. Galois, Écrits et mémoires mathématiques, edited by R. Bourgne and J.-P. Azra, Gauthier-Villars, Paris, 1962, pp. 13–17, 5–7.

PROGRESS REPORTS

EDITED BY P. R. HALMOS

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It is easy to be too busy to pay attention to what anyone else is doing, but not good. All of us should know, and want to know, what has been discovered since our formal education ended, but new words, and relations between them, are growing too fast to keep up. It is possible for a person to learn of the title of a recent work and of the key words used in it and still not have the faintest idea of what the subject is.

Progress Reports is to be an almost periodic column intended to increase everyone's mathematical information about what others have been up to. Each column will report one step forward in the mathematics of our time. The purpose is to inform, more than to instruct: what is the name of the subject, what are some of the words it uses, what is a typical question, what is the answer, who found it. The emphasis will be on concrete questions and answers (theorems), and not on general contexts and techniques (theories). References will be kept minimal: usually they will include only one of the earliest papers in which the answer appears and a more recent exposition of the discovery, whenever one is easily available.

Everyone is invited to nominate subjects to be reported on and authors to prepare the reports. The ground rules are that the principal theorem should be old enough to have been published in the usual sense of that word (and not just circulated by word of mouth or in preprints); it should be of interest to more than just a few specialists; and it should be new enough to have an effect on the mathematical life of the present and near future. In practice most reports will probably be on progress achieved somewhere between 5 and 15 years ago.

HAUPTVERMUTUNG

H. SAMELSON

Sometimes long-standing and seemingly hopeless problems get solved. One that did is the "Hauptvermutung der kombinatorischen Topologie" (principal conjecture of combinatorial topology).

Put very briefly, the conjecture said that homeomorphism implies combinatorial equivalence. The answer as it is now known says that for manifolds the conjecture is nearly true; the deviation from the truth (the "nearly") is made quite explicit.

To explain what it all means, it is best to go back to the early days of topology. It is not surprising that in those days concepts such as curve, surface, polyhedron, manifold, and topological space were not very well defined. One used the engineering approach, where everybody is already supposed to know what is meant and where giving a name to an object is supposed to explain all its properties. Even Riemann's definition of a manifold in his famous lecture "Über die Hypothesen..." is of this type (although, of course, the necessary ideas are all present).

Poincaré's definitions were somewhat free and easy too. In fact, partly because of the very intuitive character of his definitions, he ran into trouble by overlooking "torsion" (that is, elements of finite order in the relevant groups). When this was pointed out to him (by Heegaard) he started over again with a more rigorous approach, considering his spaces as divided into "cells."

Even this needed sharpening; it led after a while to the concepts of "polyhedron" and "simplicial complex." [The latter is a finite set of simplices in $\mathbf{R}^k$ with the property that the intersection of any two of them is a face of both. A polyhedron is the union of the simplices in a simplicial complex, and is thus a (very special type of) subset of $\mathbf{R}^k$. A simplex of dimension r (or r-simplex) is the convex hull of $r+1$ independent points in $\mathbf{R}^k$, where "independent" means that the convex hull has dimension r, not less. Thus a 0-simplex is a point, a 1-simplex is a segment, a 2-simplex is a triangle, a 3-simplex is a tetrahedron, etc.] There is an associated notion of piecewise linear (PL) map: a map that (after dividing the polyhedra, if needed, rectilinearly into smaller simplices) maps each simplex linearly into some simplex.

Combinatorial (or PL) manifolds of dimension n appeared on the scene. They are polyhedra that are not only locally like $\mathbf{R}^n$ (every point has a neighborhood homeomorphic to $\mathbf{R}^n$), but have the subtler property of satisfying this condition in the PL sense (every point has a neighborhood PL homeomorphic to $\mathbf{R}^n$—where it is understood that $\mathbf{R}^n$ is triangulated in the usual rectilinear way). The simplicial objects are, so to speak, finite, and therefore easier to handle; all definitions and operations become clear and crisp.

Brouwer managed to fit ordinary continuous maps into this scheme, through his ideas of simplicial approximation. This led to, e.g., the topological invariance of the combinatorially defined homology groups. What one really was after, however, were topological manifolds (locally homeomorphic to $\mathbf{R}^n$, with no PL or differentiability proviso), and the step from simplicial homeomorphism to plain homeomorphism seemed infinite. It seemed best to give up and simply to state the step as the Hauptvermutung (HV): *if two polyhedra are homeomorphic, then they are* PL *homeomorphic* (Steinitz, 1907). There is also the restriction of this to PL manifolds, and there is the related triangulation conjecture (Tr): *every topological manifold can be triangulated into a* PL *manifold.*

For the lowest dimension ($n=1$), there is no problem. For surfaces ($n=2$), HV and Tr were proved by Kerékjártó and by Radó (1923–24), and for 3-manifolds by Moise (1952). There were some early wrong proofs for the general case, and then the problem slumbered for a long time; homeomorphisms seemed just too complicated to handle.

In the meantime topology grew. The many new concepts that arose included vector bundles and fiber bundles, classifying spaces, characteristic classes, Thom's new decisive approach to classifying differentiable manifolds (up to the relation "bounding") by a systematic use of transversality (the implicit function theorem), and the corresponding notions for PL manifolds, surgery (the n-dimensional version of the old-fashioned scissors-and-glue topology), handle bodies, Whitehead torsion, s-bordism, Kervaire's example of a manifold that cannot be made differentiable, Milnor's exotic differentiable structures on the 7-sphere, etc., etc. A startling note was Milnor's example (1961) of two polyhedra that are homeomorphic but not PL homeomorphic; that disproved HV and it might have seemed like the end of the line. But eventually all this work, together with new ideas, led to positive results for manifolds. Sullivan (1967) and Lashof–Rothenberg (1969) gave an answer that says roughly that HV holds for a manifold M if the cohomology group $H^3(M; \mathbf{Z}/2)$ is 0, and similarly Tr holds if $H^4(M; \mathbf{Z}/2)$ is 0. The conditions on H^3 and H^4 appearing here are surprising. The full story is now known; it is due to Kirby and Siebenmann (1969), and it explains the role of H^3 and H^4. A basic role is played by two (topological) groups called PL and TOP. Here PL_n means, roughly, the group of PL homeomorphisms, and TOP_n the group of ordinary homeomorphisms of $\mathbf{R}^n$, and PL and TOP are direct limits under the obvious maps $\mathrm{PL}_n \to \mathrm{PL}_{n+1}$ and $\mathrm{TOP}_n \to \mathrm{TOP}_{n+1}$. These groups yield TOP/PL, a suitably defined quotient group. That TOP and PL and TOP/PL enter is not surprising. The coordinate transformations in an atlas for a manifold come (at least locally) from TOP_n or PL_n, and one formulation of the whole problem is whether the coordinate transformations, which are in TOP_n to begin with, can be "improved" so as to become elements of PL_n.

The question would be easy to answer if the quotient TOP/PL were contractible; all "obstructions" would then be 0, and HV and Tr would be true without any restriction. It turns out that TOP/PL is not very far from being contractible. It fails by a small amount: its third homotopy group is $\mathbf{Z}/2$, and all others are 0. (Recall that the ith homotopy group of a space X consists of the homotopy classes of maps of the sphere S^i into X.) It is this $\mathbf{Z}/2$ that gives rise to the H^3 and H^4 above. There are non-trivial "obstructions" to Tr and HV in H^3 and H^4; if a manifold avoids them by having its H^3 or H^4 vanish, then Tr or HV hold. (In due course counterexamples to Tr and HV were found; they involved these groups, of course.)

It is difficult to give a brief description of the methods. They are complex, they involve most of the topics mentioned above, and they involve striking new constructions. They are much more sophisticated than the simple-minded early approaches, but they have the starting point in common with them: take the coordinate transformations that occur in the definition of a manifold and try to improve them.

It should be noted that in all this the dimension of M has to be $\geqslant 5$ (or $\geqslant 6$ if there is a boundary). There are still open questions in dimension 4, just as the Poincaré question in dimension 3 is still alive. (Is every simply connected, closed 3-manifold homeomorphic to S^3?) The most "difficult" dimensions are, in fact, 3 and 4; surely this is what the creator had in mind when he made the world 3 (or 4?) dimensional.

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Reference

1. R. C. Kirby and L. C. Siebenmann, Foundational essays on topological manifolds, smoothings, and triangulations, Annals of Mathematics Studies 88, Princeton University Press, Princeton, N.J., 1977.

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MISCELLANEA

11. Complication is one of the manias of our era. If you are saying something really complex, you should do your best to spell it out for the reader in so far as is possible. ... In fine, it is no use showing off. You will only, when the chips are down, have produced the effect of having done so.

Pamela Hansford Johnson,
Important to Me, 1974, p. 244.
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