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(*b*. Königsberg, Germany [now Kaliningrad, R.S.F.S.R.], 23 January 1862; *d*. Göttingen, Germany, 14 February 1943)

mathematics.

Hilbert was descended from a Protestant middleclass family that had settled in the seventeenth century near Freiberg, Saxony. His great-grandfather, Christian David, a surgeon, moved to Künigsberg, East Prussia. David's grandfather and father were judges in Königsberg. His father's Christian name was Otto; his mother's maiden name was Erdtmann. Hilbert's inclination to mathematics is said to have been inherited from his mother. From 1870 he attended the Friedrichskolleg in Königsberg; his last year of high school was spent at the Wilhelms-Gymnasium. In 1880 he took the examination for university admission. He studied at the University of Königsberg from 1880 to 1884, except for his second semester, when he went to Heidelberg. After his doctoral examination in 1884 and receipt of his Ph.D. in 1885, he traveled to Leipzig and Paris. In June 1886 he qualified as *Privatdozent* at Königsberg University. In 1892 Hilbert was appointed professor extraordinary to replace Adolf Hurwitz at Königsberg, and in the same year he married Käthe Jerosch. In 1893 he was appointed ordinary professor, succeeding F. Lindemann. He was appointed to a chair at Gölttingon University in 1895, remaining there until his official retirement in 1930. In 1925 he fell ill with pernicious anemia, which at that time was considered incurable. New methods of treatment enabled him to recover, although he did not resume his full scientific activity. He died in 1943.

Königsberg, the university where Immanuel Kant had studied and taught, became a center of mathematical learning through Jacobi's activity (1827–1842). When Hilbert began his studies there, the algebrist Heinrich Weber, Dedekind's collaborator on the theory of algebraic functions, was a professor at Königsberg. In 1883 Weber left. His successor was Lindemann, a famous but muddle-headed mathematician who the year before had had the good luck to prove the transcendence of π Lindemann displayed an astonishing seminar activity. (The notes of the Lindemann seminar are at present in the possession of Otto Volk.) Under his influence Hilbert became interested in the theory of invariants, his first area of research. At that time Königsberg boasted a brilliant student, Hermann Minkowski, two years younger than Hilbert but one semester ahead of him, who in 1883 received the Grand Prize of the Paris Academy. In 1884 Hurwitz, three years older than Hilbert and a mature mathematician at that time, was appointed professor extraordinary at Königsberg. For eight years he was Hilbert's guide in all of mathematics. In his obituaries of Minkowski and Hurwitz, Hilbert acknowledged the great influence of these two friends on his mathematical development. In 1892 Hurwitz left for Zurich and was soon followed by Minkowski. In 1902 Hilbert was reunited with Minkowski at Göttingen, where a new mathematics chair had been created for Minkowski at Hilbert's instigation.

The mathematician whose work most profoundly influenced Hilbert was the number theoretician Leopold Kronecker, although Hilbert took exception to Kronecker's seemingly whimsical dogmatism on methodological purity and hailed Georg Cantor's work in set theory, which had been criticized by Kronecker.

Hilbert's scientific activity can be roughly divided into six periods, according to the years of publication of the results: up to 1893 (at Königsberg), algebraic forms; 1894–1899, algebraic number theory; 1899–1903, foundations of geometry; 1904–1909, analysis (Dirichlet's principle, calculus of variations, integral equations, Waring's problem); 1912–1914, theoretical physics; after 1918, foundations of mathematics.

One should further mention his famous choice of mathematical problems which he propounded to the Second International Congress of Mathematicians at Paris in 1900.

At the end of a paper read at the International Mathematical Congress at Chicago in 1893, Hilbert said:

In the history of a mathematical theory three periods can easily and clearly be distinguished: the naïve, the formal, and the critical ones. As to the theory of algebraic invariants, its founders Cayley and Sylvester are also representatives of the naïve period; when establishing the simplest invariant constructions and applying them to solving the equations of the first four degrees, they enjoyed their prime discovery. The discoverers and perfectioners of the symbolic calculus Clebsch and Gordan are the representatives of the second period, whereas the critical period has found its expression in the above mentioned theorems 6–13.

Whatever this historical tripartition means, it is obvious that Hilbert would have characterized his own numerous contributions to the theory of invariants from 1885 to 1888 as still belonging to the first two periods. Yet when he delivered his Chicago address, the theory of invariants was no longer what it had been five years before. Hilbert had perplexed his contemporaries by a revolutionary approach, his contemporaries by a revolutionary approach, nicknamed "theology" by Gorand, the "King of Invariants," What Hilbert had called Clebsch's and Gordan's formal period was the invention and skillful handling of an apparatus, the symbolic method, which still can elicit the ejobyment of the historian who is faced with it. Hilbert's new approach was quite different: a direct, nonalgorithmioc method, foreshadowing and preparing what would be called abstract algebra in the twentieth centur. It has often been consdered a mustery why, after his Chicago address, Hilber left the field of invariants, never to return to it. But it should be pointed out that Hilbert was not the only mathematician to do so. It was said that Hilbert had solved all problems of the theory of invariants. This, of course, is not true. Never has a blooming mathematical theory withered away so suddenly. The theory of invariants died as a separate discipline. Hilbert had not finished the theory of invariants by solving all of its problems but, rather, by viewing invariants under a broader aspect. This often happens in mathematics. From a higher standpoint, paramount ideas can become futilities, profound facts trivialities, and sophisticated methods obsolete. Nevertheless, it is striking that the fortune of the theory of invariants changed so abruptly, that its fall was so great, and that it was caused by a single man.

In more modern terms, the theory of invariants dealt with linear groups G acting on N-space R and the polynomials on R, invariant under G. The groups actually studies at that time were mainly the linear representations of the special linear group of n-space by m-fold symmetric tensor products—in the terminology of the time, the invariants of an n-ary form of degree m. Up to that time much skill had been applied to finding and characterizing full systems of invariants. The invariants formed a ring with a flnite basis, as far as one could tell from the examples available. Generally these basic invariants $I_{1,}....,I_k$ are not algebraically independent; the polynomial relators, called syzygies, form an ideal, which again, according to the examples, has a finite ideals basis, $F_1..., F_i$ The $F_{1,}...F_1$ need not be ideal-independent; there can be relations $R_1F_1+...+R_1F_1=0$ among them, so that one obtains an ideal of relators $R_1...., R_1$ or of "second-order syzygies," and so on.

When Hilbert started his work, the finiteness of a ring basis for invariants had been tackled by algorithmic methods which apply to very special cases only. Hilbert did not solve the total problem, and it still has not been solved. He also restricted himself to very special groups; explaining general methods through examples became one of the outstanding features of Hilbert's work. It is one of the reasons why he could build such a strong school.

It may be guessed that Hilbert started with the finiteness of the ideal basis of syzygies. In fact he proved the finiteness of the basis for any ideal in any polynomial ring. It was mainly this bold generalization and its straightforward proof which perplexed his contemporaries. The present formulation of Hilbert's basic theorem is as follows: The property of a ring R with one element of letting every ideal have a finite basis shared by it polynomial ring R[x]. It has proved fundamental far outside the theory of invariants. of course, it applied to the ideals of syzygies of any order as well. Moreover, Hilbert showed that the cascade of syzygies stops at last after *m* steps. This latter result looks like a nicety, and so it seems to have been considered for half a century, since no textbook used to mention it. Its revival in today's homological algebra is a new proof of Hilbert's prophetic vision.

Applied to the ring of invariants itself, Hilbert's basic theorem says that any invariant *I* can be presented in the form $A_1I_1+...+ A_kI_k$ where $A_{1,}..., A_1$ are polynomials which may be supposed of lower degree than I. If *G* is finite or compact, they can be changed into invariants by averaging over *G*. The new $A_{1,}... A_1$ can be expressed in the $I\ I_1,..., I_k$ in the same way as I has been; this process is continued until the degree of the coefficeints have reached zero. This more modern averaging idea stems for Hurwitz. Hilbert himself used a differential operation, Cayley's Ω process, to reach the goal.

Further of Hilbert's results connected the invariants to fields of algebraic functions and algebraic varieties, in particular the *Nullstellensatz:* If a polynomial *f* vahishes in all zeros of a polyomial idea, *M*, then some power of *f* belongs to that ideal.

Other work from the same period dealt with the representation of definite polynomials or rational functions as terms of squares, a problem to which Artin made the definitive contribution thrity years later. There is also Hilbert's irreducibility theorem, which says that, in generaly, irreducibility is preserved if, in a polynomial of several variable with integral coefficients, some of the variables with integral coefficients, some of the variable are replaced by integers. An isolated algebraic subject of later years is his investigation of the ninth-degree equation, solved by algebraic functions of four variables only and suggesting the still open problem of the most economic solving of algebraic equations.

There is no field of mathematics which by its beauty has attracted the elite of mathematicians with such an irresistible force as number theory—the "Queen of Mathematics," according to Gauss—has done. So from the theory of invariants Hillbert turned to algebraic number theory. At the 1893 meeting at Munich the Deutsche Mathematiker-Vereinigung, which Hilbert had presented with new proofs of the splitting of the prime ideal, charged Hilbert and Minkowski with preparing a report on number theory within two years. Minkowski soon withdrew, although he did read the proofs of what would be known as *Der Zahlbericht*, dated by Hilbert 10 April 1897. The *Zahlbericht* is infinitely more than a report; it is one of the classics, a masterpiece of mathematical literature. For half a century it was the bible of all who learned algebraic number theory, and perhaps it is still. In it Hilbert collected all relevant knowledge on algebraic number theory, reorganized it under striking new unifying viewpoints, reshaped formulations and proofs, and laid the groundwork for the still growing edifice of class field theory. Few mathematical treatises can rival the *Zahlbericht* in lucidity and didactic care. Starting with the quadratic field, Hilbert step by step increases the generality, with a view to a complete theory of relative Abelian fields; but from the beginning he chooses those methods which foreshadow the general principles.

At the end of the preface of the *Zahlbericht*, Hilbert said:

The theory of number fields is an edifice of rare beauty and harmony. The most richly executed part of this building as it appears to me, is the theory of Abelian fields which Kummer by his work on the higher laws of reciprocity, and Kronecker by his investigations on the complex multiplication of elliptic functions, have opened up to us. The deep glimpses into the theory which the work of these two mathematicians affords, reveals at the same time that there still lies an abundance of priceless treasures hidden in this domain, beckoning as a rich reward to the explorer who knows the value of such treasures and with love pursues the art to win them.

It is hard, if not unfeasible, in a short account to evoke a faint idea of what Hilbert wrought in algebraic number theory. Even in a much broader context it would not be easy. Hilbert's own contributions to algebraic number theory are so overwhelming that in spite of the achievements of his predecessors, one gets the impression that algebraic number theory started with Hilbert—other than the theory of invariants, which he completed. So much has happened since Hilbert that one feels uneasy when trying to describe his work in algebraic number theory with his own terms, although it should be said that many modernizations of the theory are implicitly contained or foreshadowed in Hilbert's work.

Hilbert's work centers on the reciprocity law and culminates in the idea of the class field, where the ideals of the original field become principal ideals. The reciprocity law, as it now stands, has gradually developed from Gauss's law for quadratic residues. Hilbert interpreted quadratic residues as norms in a quadratic field and the Gauss residue symbol as a norm residue symbol. In this interpretation it can be generalized so as to be useful in the study of power residues in the most efficient way. The odd behavior of the even prime $p = 2$, which in general does not admit extending solutions of $x^2 = a \bmod p^k$ to higher values, of k, is corrected by seeking solutions not in ordinary integers but in *p-adic* numbers, could not occur explicitly in Hilbert's exposition. Likewise, the totality of prime spots, although not explicitly mentioned, is Hilbert's invention. In fact, to save the reciprocity law, he introduced the infinite prime spots. His formulation of the reciprocity law as $\mathrm{II}_p\ (\alpha/p) = 1$ foreshadowed *idèles*, and his intuition of the class field has proved an accurate guide for those who later fried to reach the goals he set.

Algebraic number theory was the climax of Hilbert's activity. He abandoned the field when almost everything had yet to be done. He left it to his students and successors to undertake the completion.

Hilbert turned to foundations of geometry. Traditional geometry was much easier than the highly sophisticated mathematics he had engaged in hitherto. The impact of his work in foundations to geometry cannot be compared with that of his work in the theory of invariants, in algebraic number theory, and in analysis. There is hardly one result of his *Grundlagen der Geometrie* which would not have been discovered in the course of time if Hilbert had not written this book. But what matters is that one man alone wrote this book, and that it is a fine book. *Grundlagen der Geometrie*, published in 1899, reached its ninth edition in 1962. This means that it is still being read, and obviously by more people than read Hilbert's other work. It has gradually been modernized, but few readers realize that foundations of geometry as a field has developed more rapidly than *Grundlagen der Geometrie* as a sequence of reeditions and that Hilbert's book is now a historical document rather than a basis of modern research or teaching.

The revival of mathematics in the seventeenth century had not included geometry. Euclid's choice of subjects and his axiomatic approach were seldom questioned before the nineteenth century. Then projective and non-Euclidean geometries were discovered, and the foundations of geometry were scrutinized a new by a differential geometry (Riemann) and the group theory approach (Helmholtz). G.K.C. von Staudt (1847) tried an axiomatic of projective geometry but, unaware of the role of continuity axioms, he failed. The first logically closed axiomatic system of projective and Euclidean geometry was Pasch's (1882), modified and elaborated by the Itlian school. Hilbert is often quoted as having urged: "It must be possible to replace in all geometric statements the words *point, line, plane* by *table, chair, mug*." But Pasch had earlier said the same thing in other words. Moreover, this was not all that had to be done to understand geometry as a part of mathematics, independent of spatial reality; one needs to understand the relations between those points, lines, and planes in the same abstract way. The insight into the implicitly defining character of an axiomatic system had been reached in the *Grundlagen der Geometrie*, but at the end of the nineteenth century it was in the air; at least G. Fano had formulated it, even more explicitly, before Hilbert. It is true that this idea has become popular thanks to Hilbert, although quite slowly, against vehement resistance.

What Hilbert meant to do in his book, and actually did, is better characterized by the following statement at the end of the *Grundlagen:*

The present treatise is a critical inquiry into the principles of geometry; we have been guided by the maxim to discuss every problem in such a way as to examine whether it could not be solved in some prescribed manner and by some restricted aids. In my opinion this maxim contains a general and natural prescription; indeed, whenever in our mathematical considerations we meet a problem or guess a theorem, our desire for knowledge would not be satisfied as long as we have not secured the complete solution and the exact proof or clearly understood the reason for the impossibility and the necessity of our failure.

Indeed, the present geometrical inquiry tries to answer the question which axioms, suppositions or sids are necessary for the proof of an elementary geometric truth; afterwards it will depend on the standpoint which method of proof one prefers.

Hilbert's goals in axiomatics were consistency and independence. Both problems had been tackled before him. Non-Euclidean geometry was invented to show the independence of the axiom of parallel lines, and models of non-Euclidean geometry within Euclidean geometry proved its relative consistency. Hilbert's approach was at least partially different; his skillfully used tool was algebraization. Algebraic models and countermodels were invoked to prove consistency and independence.

Algebraization as a tool in foundations of geometry was not new at that time. It goes as far back as Staudt's "calculus of throws," although before Hilbert it seems not to have been interpreted as a relative consistency proof. For independence proofs, algebraization had been tried, just before Hilbert, in the Italian school; but Hilbert surpassed all his predecessors. In Hilbert's work and long afterward, algebraization of geometries has proved an important force in creating new algebraic structures. Isolation and interplay of incidence axioms and continuity axions are reflected by analogous phenomena in the algebraic models. In Hilbert's work they led to structures which foreshadow the ideas of field and skew field, on the one hand, and topological space, on the other, as well as various mixtures of both. Indeed, Hilbert taught the mathematicians how to axiomatize and what to do with an axiomatic system.

In 1904 Hilbert perplexed the mathematical world by salvaging the Dirichlet principle, which had been brought into discredit by Weierstrass' criticism. Before Weierstrass it had been taken for granted in the theory of variations that the lower bound of a functional F is assumed and hence provides a minimum. If some integral along the curves joining two points was bounded from below, a minimum curve must exist. The boundary value problem for the potential equation was solved according to the Dirichlet principle by minimizing $F(u) = \int |\text{grad } u|^2 d\omega$ under the given boundary conditions. After Weierstrass had shown that this argument was unjustified, the Dirichlet principle was avoided or circumvented.

Hilbert proved the Dirichlet principle by brute force, as straightforwardly as he had solved the finiteness problem of the theory of invariants. A sequence u_n is chosen such that $\lim_n F(u_n) = \inf_u F(u)$; the $|\text{grad } u_n|$ may be supposed bounded. Then a now-classic diagonal process yields a subsequence which converges first in a countable dense subset, and consequently everywhere and uniformly. Its limit solves the minimum problem. The method seems trivial today because it has become one of the most widely used tools of abstract analysis.

Hilbert also enriched the classical theory of variations, but his most important contribution to analysis is integral equations, dealt with in a series of papers from 1904 to 1910. In the course of the nineteenth century it had been learned that in integral equations the type $f - Af = g$ (where A is the integral operator and f the unknown function) is much more accessible than the type $Af = g$. Liouville (1837) once encountered such an equation and solved it by iteration. So did August Beer (1865), when trying to solve the boundary problem of potential theory by means of a double layer on the boundary; Carl Neumann mastered it (1877) by formal in version of $1 - A$. The same method proved useful in Volterra's equations (1896). When Poincare (1894) investigated the boundary problem $\Delta f + \lambda f = h$, turned into an integral equation $f - \lambda Af = g$ by means of Green's function, the parameter λ was analytically involved in the solution. This allowed analytic continuation through the λ plane except, of course, for certain polar singularities. To solve this kind of equation Fredholm (1900, 1902) devised a determinant method, but his greatest merit is to have more clearly understood the λ singularities as eigenvalues of the homogeneous problems.

At this point Hilbert came in. He deliberately turned from the inhomogeneous to the homogeneous equations, from the noneigen-values to the eigen-values—or, rather, he turned form the linerar equation to the quadratic form, that is, to its transformation on principal axes. Fredholm's method told him how this transformation had to be approached from the finite-dimension case. It was a clumsy procedure and was soon superseded by Erhard Schmidt's much more elegant one (1905). With a fresh start Hilbert then coordinatized function space by means of an orthonormal basis of continuous functions and entered the space of number sequence with convergent square sums, of Hilbert space, as it has been called since. Here the transformation on principal axes was undertaken anew, first on the quadratic forms called "completely continuous" ("compact," in modern terminology) and then on bounded forms, where Hilbert discovered and skillfully handled the continuous spectrum by means of Stieltjes' integrals. The term "spectrum" was coined by Hilbert, who, indeed, must be credited with the invention of many suggestive terms. "Spectrum" was even a prophetic term; twenty years later physicists called upon spectra of operators, as studied by Hilbert, to explain optical spectra.

Hilbert's turn to the space of number sequences seems odd today, but at that time it was badly needed; Hilbert space in a modern sense was not thinkable before the Fischer-Riesz theorem (1907), and its abstract formulation dates from the late 1920's. Hilbert's approach to spectral resolution, utterly clumsy and suffering from the historical preponderance of the resolvent, was greatly simplified later, essentially by F. Riesz (1913); the theory was extended to unbounded self-adjoint operators by J. von Neumann and M. H. Stone about 1930.

Today the least studied and the most obsolete among Hilbert's papers are probably those on integral equations. Their value is now purely historical, as the most important landmark ever set out in mathematics: the linear space method in analysis, with its geometrical language and its numerous applications, quite a few of which go back to Hilbert himself.

From Hilbert's analytic period one rather isolated work, and the most beautiful of all he did, should not be overlooked: his proof of Waring's hypothesis that every positive integer can be represented as a sum of, at most, m l^{th} powers, m depending on l only.

From about 1909 Hilbert showed an ever increasing interest in physics, which, he asserted, was too difficult to be left to physicists. The results of this activity have only partially been published (kinetic gas theory, axiomatics of radiation, relativity). It is generally acknowledged that Hilbert's achievements in this field lack the profundity and the inventiveness of his mathematical work proper. The same is true of his highly praised work in the foundations of mathematics. (It is still a sacrilege to say so, but somebody has to be the first to commit this crime.) In this field even lesser merits have made people famous but, according to the standards set by Hilbert himself, his ideas in foundations of mathematics look poor and shallow. This has become clear with the passing of time. His contemporaries and disciples were much impressed, and even now it is difficult not to be impressed, by his introduction of the "transfinite" functor τ, which for every predicate A choose an object τA such that $A(\tau A) \rightarrow A(x)$— the so-called Aristides of corruptibility, who, if shown to be corruptible, would prove the corruptibility of all Athenians. Indeed, it is a clever idea to incorporate all transfinite tools of a formal system, such as the universal and the existential quantifier, and the choice axiom into this one symbol τ and afterward to restore the finitistic point of view by systematically eliminating it. For many years the delusive profundity of that artifice led investigators the wrong way. But how of all people could Hilbert, whose intuitions used to come true like prophecies, ever believe that this tool would work? Asking this question means considering the tremendous problem of Hilbert's psychological makeup.

One desire of Hilbert's first axiomatic period was still unfulilled: after the relative consistency of geometry he wanted to prove the consistency of mathematics itself—or, as he put it, the consistency of number theory. This desire, long suppressed, finally became an obsession. As long as mathematics is no more than counting beans, its consistency is hardly a problem. It becomes one when mathematicians start to treat infinities as though they were bags of beans. Cantor had done so in set theory, and the first to reap glory by the same kind of boldness in everyday mathematics was Hilbert. Is it to be wondered that he was haunted by the need to justify these successes?

He conceived the idea of formalism: to reduce mathematics to a finite game with an infinite but finitely defined treasure of formulas. This game must be consistent; it is the burden of metamathematics to prove that while playing this game, one can never hit on the formula $0 \neq 0$. But if a vicious circle is to be avoided, metamathematics must restrict itself to counting beans. If some chain of the game delivered $0 \neq 0$, one should try to eliminate all links involving the transfinite Ä and to reduce the chain to one in which simple beans were counted—this was Hilbert's idea of a consistency proof.

From the outset there were those who did not believe this idea was feasible. Others rejected it as irrelevant. The most intransigent adversary was L. E. J. Brouwer, who from 1907 held that it is truth rather than consistency that matters in mathematics. He gradually built up a new mathematics, called intuitionism, in which many notions of classic mathematics became meaningless and many classic theorems were disproved. In the early 1920's Hermann Weyl, one of Hilbert's most famous students, took Brouwer's side. Both Hilbert and Brouwer were absolutists; for both of them mathematics was no joking matter. There must have been tension between them from their first meeting; although disguised, it can be felt in the discussions of the 1920's between a crusading Brouwer and a nervous Hilbert.

The mathematical world did not have to decide whether formalism was relevant. The catastrophe came in 1931, when Kurt Gödel proved that Hilbert's approach was not feasible. It was a profound iscovery, although there had been intimations, such as the Löwenheim-Skolem paradox. Had Hilbert never doubted the soundness of his approach? All he published in this field is so naïve that one would answer "yes." But how was it possible?

Hilbert, as open-minded as a mathematician could be, had started thinking about foundations of mathematics with a preconceived idea which from the outset narrowed his attitude. He thought that something he wished to be true was true indeed. This is not so strange as it seems. It is quite a different thing to know whether mathematics is consistent, or whether some special mathematical hypothesis is true or not. There seems to be so much more at stake in the first case that it is difficult to deal with it as impartially as with the second.

At closer look, 1931 is not the turning point but the starting point of foundations of mathematics as it has developed since. But then Hilbert can hardly be counted among the predecessors, as could Löwenheim and Skolem. This is a sad statement, but it would be a sadder thing if those who know nothing more about Hilbert than his work in foundations of mathematics judged his genius on this evidence.

In 1900 Hilbert addressed the International Congress of Mathematicians on mathematical problems, saying: "This conviction of the solvability of any mathematical problem is a strong incentive in our work; it beckons us: *this is the problem, find its solutions. You can find pure thinking since in mathematics there is no Ignorabimus!* [*Gesammelte Abhandlungen*, III, 298]". With these words Hilbert introduced twenty-three problems which have since stimulated mathematical investigations:

1. *The cardinality of the continuum*. After a great many unsuccessful attempts the problem was solved in 1963 by Paul J. Cohen, although in another sense than Hilbert thought: it has been proved unsolvable. In the same connection Hilbert mentions well-ordering, which was accomplished by Zermelo.

2. *The consistency of the arithmetic axioms*. The history of this problem has already been dealt with.

3. *The existence of tetrahedrons with equal bases and heights that are not equal in the sense of division and completion*. The question was answered affirmatively shortly afterward by Max Dehn.

4. *The straight line as the shortest connection* The problem is too vague.

5. *The analyticity of continuous groups*. The analyticity has been proved by small steps, with the final result in 1952.

6. *The axioms of physics*. Even today axiomatics of physics is hardly satisfactory. The best example is R. Giles's *Mathematical Foundations of Thermodynamics* (1964), but in general it is not yet clear what axiomatizing physics really means.

7. *Irrationality and transcendence of certain numbers* From C. L. Siegel (1921) and A. O. Gelfond (1929) to A. Baker (1966–1969), problems of this kind have been tackled successfully.

8. *Prime number problems*. Riemann's hypothesis is still open, despite tremendous work. In algebraic fields it has been answered by E. Hecke (1917). Goldbach's hypothesis has successfully been tackled by L. Schnirelmann (1930), I. M. Vinogradov (1937), and others.

9. *Proof of the most general reciprocity law in arbitrary number fields*. The problem has been successfully tackled from Hilbert himself to Artin (1928) and I. R. Šafarevič (1950).

10. *Decision on the solvability of a Diophantine equation*. A rather broad problem, this has often been dealt with—for instance, by Thus (1908) and by C. L. Siegel (1929). The general problem was answered negatively by J. V. Matijasevič in 1969.

11. *Quadratic forms with algebraic coefficients*. Important results were obtained by Helmuth Hasse (1929) and by C. L. Sigel (1936, 1951). Connections to *idèles* and algebraic groups were shown by A. Weil and T. Ono (1964–1965).

12. *Kronecker's theorem on Abelian fields for arbitrary algebraic fields*. This relates to finding the functions which for an arbitrary field play the same role as the exponential functions for the rational field and the elliptic modular functions for imaginary quadratic fields. Much has been done on this problem, but it is still far from being solved.

13. *Impossibility of solving the general seventh-degree equation by functions of two variables*. Solved by V. I. Arnold (1957), who admits continuous functions, this is still unsolved if analyticity is required.

14. *Finiteness of systems of relative integral functions*. This was answered in the negative by Masayoshi Nagata (1959).

15. *Exact founding of Schubert's enumerative calculus*. Although enumerative geometry has been founded in several ways, the justification of Schubert's calculus as such is still an open problem.

16. *Topology of real algebraic curves and surfaces*. The results are still sporadic.

17. *Representation of definite forms by squares*. This was solved by Artin (1926).

18. *Building space from congruent polyhedrons*. The finiteness of the number of groups with fundamental domain was proved by Ludwig Bieberbach (1910). A Minkowski hypothesis on the covering of space with cubes was proved by Georg Hajos (1941).

19. *The analytic character of solutions of variation problems*. A few special results have been obtained.

20. *General boundary value problems*. Hilbert's own salvage of the Dirichlet problem and many other investigations have been conducted in this area.

21. *Differential equations with a given monodromy group*. This was solved by Hilbert himself (1905).

22. *Uniformization*. For curves, this was solved by Koebe and others.

23. *Extension of the methods of variations calculus*. Hilbert himself and many others dealt with this.

If I were a painter, I could draw Hilbert's portrait, so strongly have his features engraved themselves into my mind, forty years ago when he stood on the summit of his life. I still see the high forehead, the shining eyes looking firmly through the spectacles, the strong chin accentuated by a short beard, even the bold Panama hat, and his sharp East Prussian voice still sounds in my ears [F. W. Levi, *Forscher und Wissenschaftler im heutigen Europa*, p. 337].

This description by Levi is confirmed by many others. People who met Hilbert later were gravely disappointed.

Hilbert was a strong personality, and an independent thinker in fields other than mathematics. As an East Prussian he was inclined to political conservatism, but he abhorred all kinds of nationalist emotions. During World War I he refused to sign the famous Declaration to the Cultural World, a series of "it-is-not-true-that" statements; and when the French mathematician Darboux died during the war, he dared to publish an obituary.

Biographical sketches written during Hilbert's lifetime are more or less conventional but never Byzantine. The oral tradition is more characteristic; it has been collected by Constance Reid, who in her biography of Hilbert gives a truthful and understanding image of the man and his world. Her biography also contains a reprint of Weyl's obituary, which is the most expert analysis of his work and reflects Hilbert's personal influence on his students and collaborators: "the sweet flute of the Pied Piper that Hilbert was, seducing so many rats to follow him into the deep river of mathematics." There are more witnesses concerning Hilbert: Hilbert himself, telling about his friend Minkowski; and the list of sixty-nine theses written under his guidance, many of them by students who became famous mathematicians.

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I. Original Works. Hilbert's *Gesammelte Abhandlungen*, 3 vols. (Berlin, 1932–1935; 2nd ed., 1970), includes analyses of his work and a biography by Otto Blumenthal. Not included are his *Grundlagen der Geometrie* (Leipzig, 1899; 9th ed., Stuttgart, 1962) and *Grundzuge einer allgemeinen Theorie der Integralgeichungen* (Leipzig 1912; 2nd ed. 1924).

II. Secondary Literature. The best analysis of Hilbert's work as a whole is in Hermann Weyl, "David Hilbert and His Work," in *Bulletin of the American Mathematical Society*. **50** (1944), 612–654. See also F. W. Levi, *Forscher und Wissenschaftler im heutigen Europa*, *Weltall und Erde* (Oldenburg, 1955), pp. 337–347.

An analysis of his work in foundations of geometry is Hans Freudenthal, "Zur Geschichte der Grundlagen der Geometrie," in *Nieuw archief voor wiskunde*, 4th ser., **5** (1957), 105–142. The history of Hilbert's problems is discussed in P. Alexandrov, ed., *Problemy Gilberta* (Moscow, 1969); and Ludwig Bieberbach, "über den Einfluss von Hilbert's Pariser Vortrag über 'Mathematische Probleme' auf die Entwicklung der Mathematik in den letzten dreissig Jahren," in *Naturwissenschaften*, **18** (1930), 1101–1111.

Biographical writings are Paul Bernays, "David Hilbert," in *Encyclopedia of Philosophy*, III (New York, 1967), 496–504; Otto Blumenthal, O. Toeplitz, Max Dehn, Richard Courant, Max Born, and Paul Bernays, in *Naturwissen-schaften*, **10** (1922), 67–99; Constantin Caratheodory, "Hilbert," in *Sitzungsberichte der Bayerischen Akademie der Wissenchaften zu München*, Math.-nat. Abt. (1943), 350–354; Constantin Carathéodory and Arnold Sommerfeld, "Hilbert," in *Naturwissenschaften*, **31** (1943), 213–214; G. Polya, "Some Mathematicians I Have Known," in *American Mathematical Monthly*, **76** (1969), 746–753; and Constance Reid, *Hilbert* (Berlin-Heidelberg-New York, 1970).

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