Kurt Gödel. 28 April 1906-14 January 1978
Author(s): G. Kreisel
Source: *Biographical Memoirs of Fellows of the Royal Society*, Vol. 26 (Nov., 1980), pp. 148-224
Published by: The Royal Society
Stable URL: http://www.jstor.org/stable/769782
Accessed: 21-05-2015 09:18 UTC

KURT GÖDEL

28 April 1906—14 January 1978

Elected For. Mem. R.S. 1968

By G. Kreisel, F.R.S.

Kurt Gödel did not invent mathematical logic; his famous work in the thirties settled questions which had been clearly formulated in the preceding quarter of this century. Despite sensational presentations by crackpots, philosophers and journalists (or even in poems, for example, by H. M. Enzensberger, set to music by H. W. Henze), Gödel's results have not revolutionized the silent majority's conception of mathematics, let alone its practice; much less so than the internal development of the subject since then. Certainly, those results refuted most elegantly each of the grand foundational 'theories' current at the time, of which Hilbert's, on the place of *formal rules* in mathematical reasoning, and those associated with Frege and Russell, on its reduction to *universal* systems like set theory, were most popular. (Gödel's own and related results also deflate the particular 'anti-formalist' foundations of the time, Poincaré's and Brouwer's constructivist and Zermelo's infinitistic schemes being extreme examples; they are taken up in the last sections of parts II–IV.) For obvious reasons, in his original publications Gödel made a point of formulating his work in terms acceptable to the theories mentioned, and to stress its bearing on them. But it is fair to say that they were suspect anyway, and—less trivially—that they can be refuted more convincingly by simple *constatations* rather than by (his) mathematical theorems as explained in more detail in part II. Further, as so often with very grand schemes, the refutations put nothing comparable in the place of the discredited foundational views which are, quite properly, simply ignored in current practice.

The first principal aim of this memoir is to restate Gödel's main results in the light of present knowledge, and hence independently of those foundational views. This is done in parts II and III by reference to two classes of *axiomatic* definitions, first discovered about a century ago, and familiar to anybody with an up-to-date elementary background in mathematics. Peano's and Dedekind's set-theoretic axioms for the natural and real numbers are typical of the broad class, elementary algebra and, for that matter, computer programmes of the narrow class. The relation to the foundational theories is simple: each of the latter wildly overemphasizes the role of one of the two classes of definitions,

Kurt Gödel

and so completely misjudges both classes. Gödel's results establish the different potentialities of the two classes of definitions much more dramatically than had been done before. He did not go on to study just where those potentialities are actually useful. This was done by many others who, over the last fifty years, developed and, occasionally, applied the more successful branches of logic: model theory, recursion theory, and set theory (the latter not as a foundational system, but as a specialized part of mathematics).—Readers interested in the reaction of the logical community in the thirties to Gödel's results can find a most faithful description in Kleene (1976), and some of Gödel's comments on it in Kleene (1978).

The second principal aim of this memoir is to substantiate Gödel's own view of the essential ingredient in his early successes, which solved problems directly relevant to principal interests of some of the most eminent mathematicians of this century, including Poincaré, Hilbert, Brouwer and Hermann Weyl. His view differs sharply from the impressions of many mathematical logicians who, over more than forty years, have looked in Gödel's work for the germs of some exceptionally novel mathematical constructions or for previously unheard-of subtle distinctions, but not very convincingly. Without losing sight of the permanent interest of his work, Gödel repeatedly stressed—at least, during the second half of his life—how little novel mathematics was needed; only attention to some quite commonplace (philosophical) distinctions; in the case of his most famous result: between arithmetic truth on the one hand and derivability by (any given) formal rules on the other. Far from being uncomfortable about so to speak getting something for nothing, he saw his early successes as special cases of a fruitful general, but neglected scheme:

By attention to or, equivalently, analysis of suitable traditional philosophical notions and issues, adding possibly a touch of precision, one arrives painlessly at appropriate concepts, correct conjectures, and generally easy proofs—to be compared to the use of physical reasoning for developing mathematics or, on a smaller scale, to the use of geometry in algebra.

In terms used by Kant (A 713)—philosophy analyses and mathematics builds up concepts—Gödel looked for a combination (where Kant saw only a distinction): for a given problem one may have the choice *between* a solution by means of philosophical analysis and easy mathematics *and* one by elaborate or otherwise subtle constructions. The simplest example is a solution by new axioms, discovered and justified by means of philosophical analysis. (Part IV describes a specific proposal under the slogan: axioms of infinity.) Evidently, Gödel's scheme goes counter to the wide-spread ideal of *Methodenreinheit* (purity of methods) in mathematics, made famous by Hilbert's successful use of it in geometry. With great determination and much imagination Gödel looked for other areas of knowledge where this kind of analysis would be rewarding, including the natural sciences (where, after all, Einstein had used such analysis so successfully that it remained a kind of ideal of theoretical science for decades). Part V covers this material.

It is clearly beyond the scope of this memoir to assess the value of Gödel's scheme in the arsenal of scientific methods or even to compare it with the opposite (heuristic) view expressed in the motto of the Royal Society. But enough will be said of the singular state of foundations 50 years ago, heated up by dramatic 'controversies' over almost half a century, and of alternatives in the recent literature, to limit one's expectations.

Readers are warned that it has not been possible to take full account of the many papers, ranging from over 80 scientific notebooks to some exercise books from his schooldays, which Gödel left to the Institute of Advanced Study at Princeton. The latter, with the support of the N.S.F., made available microfilms of almost 5000 pages (partly in old-fashioned Gabelsberger shorthand), mainly from the very productive years 1938-1945. As a result it was possible to document most of the points I remembered from our conversations over more than 20 years. But even the small part of his *Nachlass* that I have seen has altered completely my picture of his extraordinarily methodical working habits, about which he had been very reticent; for example, he has left a stack of envelopes full of library chits for books he borrowed, and, presumably, read. (Another surprising discovery was a bundle of drafts for lectures both on elementary and on advanced logic, written with love and care and relaxed precision, in a style different not only from his publications, but also from his letters and conversations.) Gödel himself was equally reticent about his personal history, but his wife talked more freely about it, usually in his presence. Part I of this memoir, which covers such matters, also uses material from a family history of Gödel's mother, written in 1967 by Dr Rudolf Gödel, his only sibling, a year after her death, and supplemented in 1978. Some points of detail were cleared up by letters from Gödel to his mother, which his brother put at my disposal, and by documents from the Archives of the University of Vienna which Professor E. Hlawka obtained for me (and which will be available to the public in 1989). Evidently, the sections of part I which are based primarily on the memories of members of the family or of myself, will have to be cross-checked; not so much because of exaggerated discretion but because of 'the influence of the observer on the observation', close observers tending to have a lopsided view.

I. Life and career

Family background

Gödel was born on 28 April 1906 at Brünn in Moravia, then called the Manchester of the Austro-Hungarian Empire, and now Brno (since it became part of Czechoslovakia after World War I). Gödel's father Rudolf, whose family had come from Vienna, was an early 'drop-out', but practical and energetic. He became managing director and part-owner of one of the leading textile firms. He was 14 years older than his wife Marianne, whose father, Gustav Handschuh, had come from the Rhineland, where he had been a poor weaver, to find success at Brünn, also in textiles. The mother had a broad literary education, partly in France. But she was also a competent and imaginative *Hausfrau*, to whom both her children were very much attached. She was brought up as a Protestant, her husband was only formally Catholic, and the children received no religious training. Gödel's older brother has remained unmoved by religion. Gödel himself developed quite early unorthodox theological interests, had a life-long dislike of the Catholic Church, and a soft spot for new sects, in the New World, of which he spoke often in conversation, and also wrote at some length to his mother, for example in a letter dated 18 March 1961.

Gödel's family cultivated its German national heritage; a bit self-consciously as was usual among German-speaking minorities of the multi-racial Austro-Hungarian monarchy. Naturally, this continued after World War I, and is beautifully reflected in one of Gödel's essays, written during 1920/21 at the *Staatsrealgymnasium in Brünn mit deutscher Unterrichtssprache*, on the superiority of the austere life led by Teutonic warriors over the decadent habits of civilized Rome. Most of the family friends were later very enthusiastic about the successes of Germany under Hitler. Gödel's mother, who apparently had happy memories of her school days in France, is said to have been sceptical, almost alone among her friends and neighbours.

Growing up in Brünn and Brno, 1906–1923

Gödel is remembered as a generally happy, but rather timid and touchy child, unusually troubled when his mother left the house or when he lost a game. Around 1914, at the age of eight, concern for his health began to take up more and more of his daily life; the next paragraph gives only a bare outline.

At the age of six Gödel had a painful bout of rheumatic fever, but resumed a normal life after he got better. At eight—pretty evidently after reading about possible complications of the disease, in some medical book or other—he became convinced he had a weak heart. The conviction remained to the end of his life. Occasionally he developed some of the appropriate symptoms, for example, at the end of the sixties, more than 50 years later. He saw a well known heart specialist in New York. When the e.c.g. and other tests were normal, Gödel felt frustrated—having overlooked that his particular symptoms were perfectly normal for anybody who worried about having a weak heart. This kind of oversight was by no means exceptional in his medical history. Some of Gödel's exaggerated reactions in later life, though surely going back to a natural predisposition, must have been reinforced by the peculiar difficulties of ill health in childhood. Examples of those reactions, ranging from excessive caution both in everyday life and in the presentation of his work, to distrust of the views of others, especially in medical matters, are sprinkled throughout this memoir. The caution and its frustrations go with the childhood coddling and the vicious circle to which the latter leads. The distrust goes with the logical trauma of listening to explanations by doctors and other healthy people, for example, of that vicious circle, especially for a very inquisitive child like little Gödel whom the family called 'Mr Why', *der Herr Warum*. Be that as it may, the distrust was there, and delayed appropriate treatment of an ulcer in the forties when his life had to be saved by several blood transfusions; in his final years it aggravated the prostate trouble which he called 'weakness of the bladder', well known to be desperately depressing at best.

But most of his life he managed well enough. If preoccupation with his health limited his energies, he was also careful not to waste them, as his diaries show. His powers of concentrated work and sustained interest were evident already at school (as shown by his home work on geometry in one of the exercise books he kept or his reputation never to have made a mistake in Latin grammar), and continued into the sixties when his wife still spoke of him, affectionately, as a *strammer Bursche*. Incidentally, he came upon his first romantic interest without much waste of time: she was the daughter of family friends who were frequent visitors. She was regarded as an eccentric beauty. Because she was ten years older his parents objected strongly and successfully, apparently unimpressed by the neat balance between her age and his valetudinarian habits.

Vienna, with two interludes at Princeton, 1923–1938

As Gödel mentioned in conversation he was originally undecided between mathematics and theoretical physics. The elegance of the three-year lecture cycle by the number theorist P. Furtwängler, a pupil of Hilbert and one of the founders of class field theory, tipped the balance. Another singular aspect of those lectures (which Gödel did not mention, possibly because of the medical history involved) may have had equal weight. Furtwängler was paralysed from the neck down, and lectured from his wheel chair without notes, while a scribe wrote the proofs on the board. This virtuoso performance was all the more spectacular because Furtwängler, like his cousin the famous conductor, had an exceptionally fine head.

But Gödel's principal teacher was the analyst H. Hahn, who was actively interested in foundations, and a member of the *Wiener Kreis* (Viennese Circle), a band of positivist philosophers around M. Schlick, who was shot and killed during a lecture in 1936. The meetings of the *Kreis* were held in a seminar room, off a corridor that led to the department of mathematics—and mathematicians tended to drift in and out of the meetings. Gödel attended more regularly. By a lucky chance it is possible to document what he later remembered as his (negative) reaction; by reference to the record (3)*of a meeting on foundations organised by the *Kreis*, a few months before he discovered the incompleteness theorem. On pp. 147–148 he gives a brilliantly succinct and precise analysis of the inadequacy of consistency as a sufficient condition for sound mathematics—contrary to formalist positivist doctrine. His analysis uses

* The numbers in parentheses refer to the Bibliography, pp. 223–224.

freely, almost ostentatiously just those concepts which are anathema to the doctrine—without a word about the latter, as if it were not worth mentioning. A year later, still only 25, he used similarly elegant tactics in a letter to Zermelo (of 12.10 1931) reprinted in Grattan-Guinness (1979), after Zermelo's criticism of the incompleteness theorem at the 1931 meeting of the German Mathematical Society, cf. Zermelo (1932).

Gödel's paper on incompleteness was accepted as *Habilitationsschrift* (on 1.12.32, by Hahn, as being well above the norm). In March 1933 Gödel was made *Privatdozent*, unpaid lecturer, a title which was abolished in 1938 when Austria became part of Germany. A candidate was required to have either independent means (which was called *reich*) or a job, quaintly reminiscent of the rules for retiring officers of the Austro-Hungarian army (a genuinely rich wife would do). Gödel's father had left the family comfortably off when he died in 1929 at the age of 54 from a painful abscess on the prostate. The mother moved to Vienna, took a large flat, and shared it with her two sons till 1937 when she returned to her villa in provincial Brno. Rudolf, the elder son, was already an active and successful radiologist without being wholly absorbed in his profession. The mother was enchanted by the theatre in Vienna where her long-standing literary interests were brought to life, and the sons went with her. And if Gödel preferred musicals, as he did all his life, he was very willing to form opinions on Art and Literature, and to defend them energetically, especially when they were unorthodox.—Though his work was quickly recognized in Austria and abroad, at home among his family he always went out of his way to 'hide his light under a bushel' as his brother put it.

After Gödel's first visit to Princeton (1933/34) he had a nervous breakdown. It began with severe anxiety when he got off the boat. (He telephoned his brother from Paris, who almost went to meet him there.) Wagner-Jauregg was called in, a Nobel Prize winner, and at the time perhaps even more famous than Freud, at least in Austria. No indications of psychosis were found. But there were two frustrations, each perhaps sufficient to trigger a breakdown in someone of Gödel's personality. More than twenty years later he still spoke of the frustrations of—tacitly—his bachelor life in Princeton where he had just spent a year. The second stress awaited him in Vienna.

At 21, a couple of years before his father died, he met Adele Porkert at a Viennese night spot, *Der Nachtfalter*. She had been briefly married before, and was six years older than Gödel. Once again his parents, especially the father objected. In fact, Gödel did not marry Adele till 1938.—I visited them quite often in the fifties and sixties. It was a revelation to see him relax in her company. She had little formal education, but a real flair for the *mot juste*, which her somewhat critical mother-in-law eventually noticed too, and a knack for amusing and apparently quite spontaneous twists on a familiar ploy: to invent—at least, at the time—far-fetched grounds for jealousy. On one occasion she painted the I.A.S., which she usually called *Altersversorgungsheim* (home for old-age pensioners), as teeming with pretty girl students who

queued up at the office doors of permanent professors. Gödel was very much at ease with her style. But this is not all: in a sense the principal logical theme of this memoir goes back to her banter. She would make fun of his reading matter, for example, on ghosts or demons (but never of pages of logical formulae which have their funny side too if she only knew). Quite often, the topics she mentioned explicitly, fitted perfectly what I had read between the lines in his publications without paying much attention, for example, to ghosts and universes with cyclic time considered in (21) and (22), and further discussed in part V below. Since I had noticed the connection spontaneously, presumably showing the pleasure which goes with this kind of *Aha-Erlebnis*, he found it worthwhile expanding on it; in a totally natural style, fully and freely—very much in contrast to his almost staggering responses, logical slaps in the face as it were, when he felt in duty bound to have an opinion on uncongenial matters. As already mentioned on p. 151, his wife's conversations also shed light on his personal life or, at least, suggested how to find out more about it.

Breaking the Austrian connection (1938–1939)

Gödel, by and large, had the political views which were standard in his youth, in his immediate surroundings and in large parts of Central Europe. America was the land of opportunity, Germany was efficient, Austria *schlampig*. But granted all this, his aversion, after World War II, to Austrian academic institutions seems out of all proportion, and remained a total puzzle to his family, as documented, for example, by his mother's letter of 28 January 1963 to her brother Karl. He was offered, and refused, sometimes for mind-boggling reasons, membership and later honorary membership of the Academy of Sciences in Vienna, and the highest national medal for science and the arts. (He had no chance to refuse an honorary doctorate of the University of Vienna, since it was awarded posthumously.) He had accepted other honours, and was to accept more; for example, he was delighted to be a Foreign Member of the Royal Society although England remained *Perfidious Albion* for him. (He was also made an Honorary Member of the London Mathematical Society in 1967, and a Corresponding Fellow of the British Academy in 1972.) And if the Academy of Sciences of Vienna is not of quite the same level, neither is the American Academy of Arts and Sciences, and he was a member.

The story is not heroic, but it is beautifully coherent. Gödel was a most remarkable logician, he never pretended to be a dashing hero; nor was he impressed by heroes. (He admired General Eisenhower while his wife was a great fan of General McArthur.)

When Austria became part of Germany in March 1938, he was not made *Dozent neuer Ordnung*, (paid) lecturer of the New Order, in contrast to most university lecturers who had held the title of *Privatdozent*. He was thought to be Jewish. (For the same reason he was once attacked in the street by some rowdies whom his wife chased off with her handbag.) He was convinced that nowhere except in Austria could there be such a *Schlamperei*, such a careless error. As he told me, he left Austria for Princeton, crossing Russia on the Trans-Siberian Railway, at the end of 1939 because he did not wish to be conscripted into the German army. Of course he felt he was not physically fit for military service; but given the evidence he had of *Schlamperei*, the risk was too great.

However, by and large, life went on smoothly for him in Austria during the spring and the summer of 1938: according to his diaries, he worked actively, read widely, and travelled to Göttingen to lecture on his work in set theory. In autumn, after the Munich agreement, he married. He spent the first term of 1938/39 at the Institute (for Advanced Study) at Princeton, the second at Notre Dame, where he prepared some beautiful lectures. He returned to Austria in spring 1939. In short, his misfortunes in 1938–39 were minor compared not only to what went on more or less quietly around him but also to the much publicized hardships during popular uprisings (*Volkserhebungen*) of the past, like the French or Russian revolutions.

The fact is that he was bitterly frustrated. Once again, despite great care he had not escaped trouble. Specifically in the words of the *Dozentenbundsführer*, in a letter of 30 September 1939 concerning Gödel's application of 25 September (1939) for a *Dozentur neuer Ordnung*, Gödel was not known ever to have uttered a single word in favour or against the National Socialist movement although he himself moved in Jewish-liberal circles (and though the letter acknowledges mitigating circumstances, it neither supports nor rejects Gödel's application, which was accepted on 28 June 1940). Incidentally, the *Schlamperei* may have added a touch of insult to injury, if something was still left of the views in his essay on Teutonic warriors mentioned on p. 152: certainly, most of the essays already reflect perfectly the views he held all his life.

A bit more courage or highmindedness might have reduced Gödel's bitterness about his particular predicament. But, as the fate of his mother shows, even those commodities were not enough to ensure a cool head at the time. Till 1944 she stayed in her villa in Brno, openly critical, losing most of her former friends, and worrying her son, Rudolf, who was running the X-ray department of a hospital in Vienna. By 1944 both expected the defeat of Germany. She had had a good offer for her villa, toyed with it, but did nothing despite her almost daily criticisms (of the National Socialist regime): in effect, she did not expect reprisals by the Czechs after the war, not even confiscation of German property, let alone the deportations. Fortunately, she herself moved to Vienna, but not by calculation. She happened to be there with her son, there was a heavy raid, and they simply wanted to stay together. After the war the Austrian government negotiated with the Czechs, and according to the treaty the mother got the usual, inadequate compensation for her villa, one tenth of its assessed value. The fact that the same rate was almost universally applied to confiscation by the Germans was quite irrelevant for Gödel since, logically, two wrongs do not make a right—and he never got over the injustice to his family. He himself was always most punctilious, and incidentally helped his mother as soon as possible.

The New World: the first 30 *years,* 1939–1969

Gödel was well prepared to like America, given his general views and his particular resentment against Austria and its bureaucracy (in particular, the academic bureaucracy, which he knew well). Almost every letter to his mother between 1946 and 1963 which I have seen contains some variation on this theme. He became a U.S. citizen in 1948. He was especially attached to the I.A.S., of which he was an ordinary member till 1946, and a permanent member till 1953 when, at the age of 47, he was made professor. In a touching letter, of 25 March, he tells his mother that he would not have any lecturing duties though the salary was even higher than at universities. (He had the illusion that he was expected to have opinions on all details of I.A.S. business.) He saw a good deal of von Neumann who is said to have astonished his first wife on their honeymoon in Vienna, in the early thirties, by the long hours he spent with Gödel talking about mathematical logic and foundations.

In the forties, except while weakened by an ulcer (and his own treatment of it, as mentioned on p. 153), Gödel worked with great intensity. A turning point was his wide-ranging essay (19) on Russell's mathematical logic. It collects together a number of incisive points, most of which are formulated in a more relaxed style in his unpublished notes from the thirties mentioned on p. 151 (and used below). There are also some quite different, and much better known points, reviewed at the end of part IV, for instance those that have led to the label: Gödel's platonism. He could use (19) to take stock of his whole logical experience without the slightest trace of self-indulgence: Russell's writings touched on every issue that could conceivably cross anybody's mind. Having thus arrived at his mature (heuristic) views sketched on p. 150, the time was ripe for Gödel to apply them outside the narrow area of mathematical logic too.

The place, the I.A.S., was right for an excursion into the general theory of relativity. Einstein was there, and Gödel, perhaps more than most, was impressed by Einstein's singular success in using philosophical analysis for—presenting—his special theory of relativity; with a bit of luck, 'singular' would allow for repetitions. Einstein was enchanted by Gödel's combination of elegance and precision, and they saw each other constantly till the death of Einstein. It may be difficult to decide how Gödel's work on general relativity (described in part V) was influenced by their conversations, as so often when a decision has few consequences, and so, practically speaking, does not matter. At any rate one can be sure that Gödel would not have brought up the subject before he had something new to say. Gödel's mother was overawed when she heard of the friendship, and began to read about Einstein. In a letter of 8 January 1951, Gödel recommends her not to be afraid of abstractions in Einstein's expositions, and not to try to understand everything at a first reading, but to go about it as she would read a novel.

In the early fifties Gödel's achievements began to be formally recognized: by honorary doctorates at Yale (D.Litt.) and Harvard (D.Sc.), the Einstein

Award split with Schwinger, the Gibbs Lectureship of the American Mathematical Society. In 1955 he was elected to the National Academy of Sciences. As far as the next 15 years or so are concerned, it is doubtful—and certainly impossible for me to decide—whether my picture is representative of his principal interests; I met him in autumn 1955, and remained in close contact with him till his illness at the end of the sixties. But what I know is sufficient to correct two widespread impressions: (i) though courteous he lacked sensibility and warmth, and (ii) his conduct of I.A.S. business was impenetrable.

In connection with (i) I myself witnessed a degree of understanding, whether intuitive or as a result of reflection, which is exceptional by any standards. Before I met Gödel I was of course impressed by the clarity of purpose shining through every line of his, but not carried away, mainly because it seemed to me—and to Gödel in 1930–31 as he told me later—that at the time it was a matter of months before somebody would stumble on the completeness and incompleteness theorems, his most famous results; cf. part II. (For specialists: In those days I was more impressed by the 'broad sweep' of Hilbert's programme, and especially by Herbrand's originality in logic whose theorem was a much subtler business: it was not even properly understood or used for many years.) Worse still, I was simply put off by his general essays (19) and (20), particularly by the most widely quoted passages, mentioned on p. 157, and I made no secret of the fact. With patience and unerring judgment Gödel led the conversation to points of common interest. In no time I saw for myself the many civilized passages of (19) and (20), which are hardly ever quoted. In due course I even went back to the offensive passages, and saw them in a different light, particularly in connection with so-called intuitionistic notions (described in more detail on p. 185). Later, a different obstacle appeared, as so often when things are going too well. Given common logical interests, and, as readers may have guessed on p. 153, a touch of hypochondria also on my part, there were exchanges on those minute reactions, to bugs or drugs, to which doctors will not even pretend to listen. In the after-glow, the conversation occasionally strayed to Gödel's general views on men and events and his all-pervading distrust. Another set of impatient questions: Did he expect me to find, behind his actions, the kind of devious motives he saw in others? Was he not frustrated to let others govern the world since he knew so well what was good for it? (and almost in the same breath) How well did he know the world since he was constantly surprised by what happened? Again he—and he alone—helped: apparently without a trace of resentment or even irritation, he avoided general topics—until his illness. At the same time he continued to ask me about my own doings and preferences, with a convincing mix of curiosity and personal sympathy. I remember only one occasion when I reciprocated, one evening when both he and his wife were in particularly good form. Since they so clearly liked being hospitable, why did they not have (other) guests more often? Gödel had noticed that most people showed more excitement in company than they felt, and he found this very tiring. Clearly, at times he needed very few data to reach, painlessly, a very sound conclusion.

In connection with (ii), especially in his selection of logicians for temporary membership at the I.A.S., his practice followed quite simply from his general heuristic principles explained at the beginning of the memoir: he gave preference to applicants whose work used at least implicitly or was likely to use philosophical analysis. He tried to judge this by reading their publications repeatedly, but generally not carefully. He seems to have been pretty successful. Besides this 'long-shot', of philosophical analysis, he also encouraged others, for example, the filigree work classifying sets of natural numbers by so-called Turing degrees: he thought it might suggest new ideas in cardinal arithmetic. In the fifties he looked, in vain, for logicians interested in the partition calculus of Erdös and Rado. (Given that the mathematical interest of logic, especially of its elaborations, is marginal, his encouragement of a few long-shots was reasonable anyway, and Gödel never pressed for having a horde of logicians at the I.A.S.) Once he had made a selection he avoided contact with people who were not temperamentally congenial to him; particularly introverted, tongue-tied, and generally affected personalities made him uncomfortable. He was fond of keeping pests at a distance by means of ambiguous remarks reminiscent of de Gaulle (Messieurs, je vous ai compris); for example, it would be interesting to see the work in print. He never edited any journal. Presumably, he did not usually give his simple reasons for his selections. After all, he always stressed the conflict between his views and the *Zeitgeist*, to which, naturally without empirical checks, he supposed his colleagues at the I.A.S. to be subject. He was more disappointed than he let on by his occasional failures to persuade them; but not nearly as much as he would have been had he realized that he was battling a *Zeitgeist* from another time, the early thirties; and then not what it was, but what the *Wiener Kreis* would have wanted it to be.

The final years, 1969–1978

The events during this period would have unsettled Gödel at his best. His wife suffered two strokes and a major operation. There were—obviously interrelated—changes for the worse in America and at the I.A.S., the country and the institution to which he was so much attached. For example, student radicals were making headlines, and—admittedly, less charismatic—professors at the I.A.S. could hope to make, at least, the correspondence columns of the *N.Y. Times*: an issue was bound to present itself, and did. (This appeal to the *Zeitgeist* was not congenial to Gödel.) More subtly, there was a general air of despondency among the large number of able but jobless young mathematicians who were herded together at the I.A.S., constantly talking to each other, and so reinforcing each other's illusions about clever tactics for getting a job.

But the decisive factor was his own illness, mentioned already on several occasions. This is not the place to give a detailed medical history which, however, will be essential for a correct interpretation of what he said or wrote during those years. The particular character of the self-doubts which go with even mild prostate trouble are well known: usually there is a grain of truth, but magnified out of all proportion. This spoils completely the victim's perspective of his work over the years. (Except for p. 197 and p. 209, Gödel's views in the seventies quoted below, correspond to earlier publications, notes or conversations.) Superficially, at least in the early seventies, the changes appeared minor to those who had not known him well. After all, his mind remained nimble; only his exquisite sense of discretion had obviously gone. Perhaps as a result he was more gregarious than before; less formidable, as a perceptive secretary at the I.A.S. recently said. Even if this brought him some solace, it did not seem to me to go very deep—and accounts of his close family since his death have more than confirmed this impression. Actually, several of us who knew him well were alarmed already at the end of the sixties: his efforts not to show his depressions were evident, and soon became too much for me to watch. There were some bright spots: the U.S. National Medal of Science in 1974, after an honorary doctorate, in 1972, from Rockefeller University which gave him pleasure. In 1967 he had received one from Amherst College.

Gödel died, sitting in a chair in his hospital room at Princeton, in the afternoon of 14 January 1978.

II. Axiomatization and formalization

Gödel's first two famous results, which appeared in (1) and (4) about 50 years ago, concern *formal rules* or, as we should now say, computer programmes. Put simply, (1) establishes the 'positive' result that Frege's rules for elementary logic, of truth functions and quantifiers, proposed some 50 years earlier, generate exactly the logical truths in the precise mathematical sense corresponding to Leibniz's truths in all possible worlds. Paper (4) shows that the rules of *Principia Mathematica*, P.M. for short, and in fact those of a large class of 'related' systems, do not generate exactly the true arithmetic theorems (built up logically from polynomial equations with integral variables and coefficients, among the formulae of P.M. expressing such theorems). Even without going into refinements of the statements and proofs and leaving more ethereal foundational schemes for later, readers will imagine easily the striking implications of these simple, memorable results.

Thus, 100 years ago, (1) would have had the glamour: simple mechanical rules can be proved *mathematically* to replace logical reasoning, at least, its results, not necessarily the details of the process—and logic is about all possible worlds, so to speak, the height of abstraction! At that time, (4) would merely have ratified the general impression that arithmetic is too difficult to be formalized, another word for 'mechanized'; after all, the diophantine equation $x^2 = y^3 + k$ is hard enough.

Today, Frege's rules, even without (1), still stand out as the first convincing example of non-numerical data processing by mechanical means. Examples of simple mathematical proofs, as in (1) and (4), showing what can or cannot be done 'in principle' by such means, are obviously essential for orientation, and, at least occasionally, useful in practice, provided they are used with discretion and imagination. For realistic expectations this should be compared to the use of whole numbers in place of formal rules and of elementary theorems about them, where much skill is needed to find properties studied in higher number theory which are really significant for the bulk of scientific or other uses of whole numbers. It would not be hard to work up a parallel between (4) and the irrationality of $\sqrt{2}$ in the uses of formal rules and of whole numbers respectively.

But in between, at the time of (1) and (4), the latter had all the glamour. For one thing, P.M. had claimed to provide great weight of *empirical evidence*, in three heavy volumes, for the possibility of formalizing 'all' of mathematics, and certainly arithmetic. What is more, the claim was widely accepted including even Russell's contention that only empirical evidence, taken from mathematical practice (as codified in texts, etc.), was relevant. (4) was shocking, especially if one glanced at the proof. P.M. had left out an obvious type of argument which reflects on its own rules (and implies in a simple way a certain true arithmetic statement that cannot be derived in P.M. at all): P.M. had proposed a mathematical model of a certain phenomenon, mathematical practice, and had forgotten to look at the mathematical properties of the model itself! A moment's thought makes (1) almost as disturbing as (4) for Russell's doctrine of empirical evidence. What was the difference in the 'degree of confirmation' of the claims of P.M. as far as logic and arithmetic were concerned? Anyhow, what was the claim? To describe—and perhaps to perpetuate—the defects of current practice or to find out something about the potentialities of mathematics and the mathematical imagination? And was P.M. any worse than what, for example, is done in studies of non-mathematical reasoning, by linguists and the like? Incidentally, in part V several examples will be given how reflection on (4) and on its development in mathematics throws light on various arguments in the natural sciences too, the kind of thing one expects from a useful philosophy of science.

Returning to the aims of P.M., mathematicians had lapped up the idea of beginning with a formalization of all of mathematics; for example, Bourbaki's treatise starts with a chapter on set theory—not exactly P.M., but (4) applies too. In their manifesto, Bourbaki (1948), they get round to asking themselves about the point of this enterprise—and conclude on pp. 37–38 that it is 'the least interesting side of the matter' or that formal rules of inference serve for 'logical hygiene' (rarely applied since the rules are barely quoted later—more like ritual ablutions). If anything, (1) serves as logical hygiene in giving a logical justification for the choice of the formal rules! Obviously, the notion of *set* is here to stay; but there is not a shred of evidence in Bourbaki (1948) that the ritual of giving formal axioms and rules for sets is of effective use in the later development—more effective than a description of the intended notion, for example, as in part III below.

To anticipate: since 1948, modest, but sound answers have been supplied by mathematical logic to the questions implicit in Bourbaki (1948). The possibility of defining many mathematical notions and problems in elementary terms has found uses, foreshadowed in (1) by the so-called finiteness theorem; and derivations built up by elementary rules are easy to unwind; cf. p. 182 for details. As to (4), incompleteness results explain quite well why certain questions, for example, about groups, have not been settled yet, though more difficult arguments than those of (4) are needed. More positively, just because of incompleteness, we know more if a theorem can be formally derived by given rules than if it is merely true, and, perhaps less obviously, we know more if a (true) theorem can be derived by given rules, but not by a subset of those rules. As always, the discovery of the terms in which this additional knowledge is to be expressed is a principal part of research; successful examples are to be found on p. 174 or p. 175. In short, slowly, the early ritual is becoming a scientific tool.

But also—and this is much more striking—the tools found are pitiful compared to the original expectations associated with mathematical logic. Specifically, Boole (1854) looked for the *laws of thought* in propositional algebra, and Hilbert (1930, p. 9) thought that he had found the laws in his own favourite rules—a mind boggling exaggeration since, as already mentioned, even the positive result in (1) concerned only results, not the details of reasoning, treating the latter as a matter of black boxes. Then there was the retreat to logic as providing a *standard of rigour*, an 'ultimate' criterion for checking proofs, the 'hygiene' which is not applied—in fact, one applies more often interpretations, clever cross checks, to verify formal derivations. The development of logic since (1) and (4) has moved away from the aims mentioned; in particular, soon after (4), the emphasis on formal rules for the special purpose of building up derivations and representing proofs was quietly dropped, as reflected in the terminological change from

formal undecidability of a particular problem $P_{\mathscr{F}}$,

depending on the formal system $\mathscr{F}$ under consideration, used in (4), to

recursive undecidability of a class of problems (including $P_{\mathscr{F}}$).

A readable account of this matter is in the article on Hilbert's tenth problem in Browder (1976). More about the whole matter of representing proofs is to be found at the end of parts II–IV.

We leave this disturbing side of (1) and (4) with the few snippets above. Of course the latter do not convey even approximately the bearing of (1) and (4) on the ideas current at the time, let alone on the principal people active in logic. Russell, Hilbert and Brouwer were not narrow specialists: they were fascinated by the turmoil of ideas current during the first three decades of this century, a very special period in the development of science. There was an unbounded confidence in high theory, as already mentioned in part I in connection with Einstein. There was progress with understanding phenomena where, previously, one just did not even know where to begin—and so Kant's odd question how this or that experience was possible at all (*überhaupt möglich*, instead of the ordinary scientific question, what things are like) seemed appealing. And last but not least, there were extraordinary successes of building up the physical world or, at least, matter from a few particles—so why not mathematics and mathematical reasoning from a few primitives, set and membership, and a few rules of inference? Nothing remotely like existing logic is even a candidate for an analysis of mathematics or mathematical reasoning comparable in scope to those successes in the natural sciences.

Accentuating the positive: a piece of forgotten history

Gödel's results and even Hilbert's conjectures which were refuted so simply that they have been described as 'blind spots', appear in a totally different light if we go back to the last century, to what even now are *Aha-Erlebnisse*. Two of them were already mentioned, namely set-theoretic (or: broad axiomatic) definitions of familiar structures by Peano's and Dedekind's axioms, and Frege's formal rules for elementary logic (in the precise sense explained on p. 165). The third is the exposition of geometry in Hilbert (1899) with striking examples of a mathematical scheme for choosing a formalization —in contrast to the business about empirical evidence in P.M. Today the principle of choice is better illustrated by considering the ordered field of the real numbers instead of geometry, passing from *arbitrary* Dedekind cuts to those defined by *elementary* formulae (about ordered fields), and thus to a natural if not very well-known axiomatization of so-called maximal ordered fields. In the context considered, the reference to arbitrary sets or cuts could really be described (by Hilbert) without exaggeration as a mere *façon de parler* because, as far as results—and, at least at the time, also proofs—were concerned, Dedekind's arbitrary cuts gave no more than those defined by elementary means. Hilbert was quite conscious of the obvious relation between this discovery and an age-old ideal of *Methodenreinheit*, as he stressed in the peroration to Hilbert (1899); 'age-old' in that it goes back to the time of the Greeks when Archimedes was criticized for using properties of space to prove theorems about the plane; cf. Knorr (1978). For elementary theorems, you use elementary cuts. Number theorists will think of heated but inarticulate arguments about impure methods, analytic number theory at one time, l-adic cohomology now. Incidentally, though this was not stressed by Hilbert himself, his later, much more famous consistency programme is also a particular case of this search for pure methods: so-called finitist theorems should have finitist proofs (of which old-fashioned school mathematics is typical). A neat, but purely technical observation of Hilbert was that this aim is assured under suitable conditions if the *formal consistency* of a system $\mathscr{F}$ is proved finitistically, the aim being now restricted to finitist theorems derived in $\mathscr{F}$ itself; cf. p. 172 or, for a more pedantic exposition, the section on Hilbert's second problem in Browder (1976).

From this point of view, Gödel's paper (1) establishes that logical purity can be achieved in principle, and (4) that arithmetic purity cannot be achieved; in fact, the result (4) is so general that it is quite insensitive to any genuine ambiguities in the notion of purity of method.

Legalistically, Gödel's papers only settle questions about the possibility of purity of method. But inspection of the arguments suggests quite strongly that the *whole ideal of purity of method is suspect, even when it can be achieved.* (As will be seen in part IV, Gödel turned the ideal upside down, wanting to prove finite combinatorial theorems by use of properties of very large infinite cardinals.) In any case, today there are plenty of examples in ordinary mathematics where impure methods are employed: the *restriction* to pure methods has to be 'justified' (when it is appropriate at all), at least as much as the use of impure methods. The most familiar reason for restricting methods of proof is the greater generality of the theorems proved, their validity for more cases (of interest). Trivially, where purity can be achieved, the essential difference between pure and impure proofs cannot be analysed in terms of validity. But

> the validity of a theorem, in fact, the validity of a proof, is only a small part of the significant knowledge contained in the proof: it just happens to be the part which is most easily put into words.

And if that part is regarded as the specifically *logical* aspect of proofs, then logic is marginal for understanding the actual phenomena of proofs. As already anticipated on p. 162, in practice if not in rhetoric, this conclusion has been accepted, and new aims, mentioned there are pursued.

The remainder of part II consists of more technical material with special emphasis on warnings, including Gödel's own about the consistency criteria in (3) or the significance of the second incompleteness theorem, which have made little impression. It did not seem appropriate to include standard proofs of (1) and (4) since very efficient expositions are available in the literature, for example, Barwise (1977). At the end of part II, the general observations on foundational schemes made on p. 162 are sharpened, with a summary on the passage from foundations to technology.

Axiomatizations and formalizations: some reminders

Although the axiomatic tradition goes back to Euclid, it was changed radically about 100 years ago by two new methods (and aims).

First, by use of the notion of set, which had just become prominent through the work of Cantor, familiar objects together with—what are regarded as—their principal features, could be *defined* axiomatically, as one says: up to isomorphism. The still most famous examples are Peano's axioms for the natural numbers with the successor relation as principal feature, and Dedekind's for the (ordered field of) real numbers; but cf. also Zermelo's axioms in part III for segments of the so-called cumulative hierarchy of sets. This use of axioms (as definitions) distinguishes them from Euclid's, which were not intended to be, and are not definitions unique up to isomorphism, since they are satisfied both by the full (uncountable) plane and by the part consisting of points constructible from two points by means of ruler and compass. In modern terms, the new axioms use a richer, so-called *non-elementary* language; in contrast to Euclid, arbitrary *subsets* of the sets (of numbers) involved are used to state induction and completeness (for Dedekind cuts) in Peano's, resp. Dedekind's axioms.

The second new element was introduced by Frege, his famous formal rules (of inference). They were intended as an analysis or 'definition' of *logical deduction from axioms*, more precisely (as we realize now), from *elementary* axioms, built up from relations (between the objects of some domain D) by means of the logical operations $\neg$ (not), & (and), $\vee$ (or), $\Rightarrow$ (implies), $\forall x$(for all elements of D), $\exists x$(for some elements of D). In particular, such elementary axioms do not use the new non-elementary quantifier: for all sub*sets* of D, needed for the definitions in the last paragraph. Systems of elementary axioms together with Frege's rules are called *formalizations*.

Realistically speaking, neither the new definitions nor the new rules were needed for mathematical practice at the time (nor before: the *Disquisitiones* of Gauss would not be improved by starting with Peano's axioms or by writing the proofs of the law of quadratic reciprocity in Frege's formalism.) But clearly there was a raw interest to the two enrichments of the axiomatic tradition. It fired the imagination of mathematicians and philosophers. Readers can well imagine how the surprisingly compact definitions (in the language of sets and logic) of Peano and Dedekind made them into ideals for all definitions in mathematics, and how Frege's simple rules led to wild exaggerations about the laws of thought, mentioned on p. 162. For all we know, these exaggerations served as a useful body guard, protecting the new interesting methods until their significance was discovered too.

One of the first convincing indications of significant uses is to be found in Hilbert (1899): the use of non-elementary axiomatizations for a systematic choice of formalizations, already alluded to on p. 163. To be precise, the passage involved was not explicitly formulated by Hilbert, but fits very well his work on the foundations of geometry, where Dedekind cuts turn up as non-elementary axioms of continuity, which explains the connection between Hilbert (1899) and the exposition below.

From non-elementary axiomatizations to formalizations

The passage is best illustrated by the step from Dedekind's axioms to a very natural formalization of elementary real algebra, known in the trade as the theory of *ordered real closed fields*. The principle, elaborated on pp. 101–103 of Browder (1976), is to restrict cuts to those defined by elementary formulae about ordered fields, instead of arbitrary cuts; this is expressed by an *infinite* axiom schema corresponding to each formula. No other change is made since the rest of Dedekind's axioms are elementary anyway. Incidentally, real closed fields were not considered by Hilbert himself, but were stumbled on 'empirically' in the twenties by Artin and Schreier.

Though there are many real closed fields, for example, of all the real numbers and of the algebraic real numbers, *every elementary proposition which is true in one such field is true in all the others*. This was established by several logicians around 1930, including Tarski and Herbrand, but also Gödel who, as he mentioned in conversation, did not publish the result when he learnt that Tarski had found it independently. They showed that all elementary formulae F (without free variables) about those fields are *decided*, that is, either F or $\neg F$ is derivable from the axioms by means of Frege's rules. In fact, for each F, a *finite* subset $\mathscr{S}_F$ of the infinitely many axioms is determined which is sufficient to decide F. Equivalently, if F is true for the field of real numbers then F follows logically from $\mathscr{S}_F$: the formalization is *complete* (in this sense).

Remarks for specialists. First, logically less complicated cuts are sufficient, namely those defined by (the least zero of) polynomials of odd degree and the (lesser) square root of positive elements. Secondly, the famous result, in Milnor (1958), on division algebras over $\mathbb{R}$ conveys the flavour of the implications of the facts above. Thus the result of Milnor (1958) is true for all real closed fields. But nobody has developed K-theory in that context sufficiently, and the only known proof of the general result uses the transfer principle mentioned above. Again, the fact that there is no division algebra of dimension 16, is expressed by an elementary formula, say F_{16}. By the finiteness principle, for suitable N_{16}, F_{16} holds automatically for all fields in which all polynomials of odd degree $\leqslant 2N_{16}+1$ have a zero, and positive elements have square roots; incidentally the least N_{16} is not known. (A bound for N_{16} is part of any pure derivation of F_{16} from the formal axioms.)

Peano's axioms also illustrate the passage to formalizations, but with an added twist on the choice of 'principal features' of the structure considered. Apart from equality, Peano's axioms mention only one relation, say S, for the successor. So, taken literally, the passage leads to the successor axioms and induction restricted to (elementary) properties defined from S alone. Again, this formalization decides every elementary formula (about S); but precious little can be expressed about the natural numbers in this way. Substantially more is expressed by elementary formulae about addition, for example, about congruences. Here the passage starts with Peano's non-elementary axioms together with the usual recursion equations for $+$ (in terms of S and $=$), which define addition implicitly. Again, the resulting formalization decides all its formulae. Unquestionably, Hilbert expected similar complete formalizations for additional number-theoretic functions defined by recursion equations, for example, multiplication. Now the expressive power of the formalism is considerable: every diophantine problem can be stated.

Methodenreinheit: how to test philosophical ideals

The three formalizations in the last subsection for ordered real closed fields and for arithmetic of the successor relation and of addition, fit perfectly Hilbert's ideal of purity (on p. 150): to settle elementary problems, one does not need arbitrary cuts or sets, but only cuts defined by elementary formulae, and only elementary instances of induction, built up logically from the relations used to state the problems.

The *logical* question is to settle to what extent purity of methods can be achieved—in all of mathematics, parts of mathematics, in fact, in logic or metamathematics itself. But this leaves open the *philosophical* question whether purity of methods is at all basic, in the sense of fundamental, to mathematical knowledge, the sort of thing one cannot know too much about.

If purity is not basic then work done with this ideal as principal aim will have to be reexamined under the maxim: *dégager les hypothèses utiles*, appropriate to the assessment of tools. The discovery of good uses (as in the remarks for specialists on p. 166) becomes a major problem in contrast to the study of fundamental laws, which can be relied upon to have applications.

Defects of ideals are generally seen most clearly in areas where they have been realized, and so the results can be compared both with earlier expectations and with alternatives (which violate the ideal in question). In the cases under discussion, algebraic and number-theoretic purity, plenty of comparisons are available since, with time, impure proofs have become more common in practice, not less. Moreover—and this is often neglected—(i) their actual reliability or 'security' is obviously unaffected by the possibility of pure proofs if that possibility has not been realized, and (ii) impure methods are not only used heuristically, for discovering conjectures and proofs, but have turned out to be essential for *checking* proofs.

Far from being a mere aberration, the neglect of (i) and (ii) is typical of what happens in the kind of intellectual void left by the two omissions mentioned already on pp. 161–162. First of all, the unproblematic uses of formalization (or, generally, purity of methods) have not become widely known; so there is a tendency to thrash about for some uses, and the easiest thing is to cling to dubious doubts which are to be removed by formalization, as in the business of logical hygiene. But also there is the void created by simply not saying out loud what (knowledge) is gained by impure proofs, for example by analytic proofs in number theory: knowledge of *relations between the natural numbers and the complex plane* or, more fully, between arithmetic and geometric properties. It is precisely this knowledge which provides effective new means of checking proofs: if this conflicts with some ideal of rigour, so much the worse for the ideal (which is being tested).

In short, the whole matter of formalization and purity of method is just much subtler than suggested by generalities about mathematical rigour (however persuasive the latter may be at first glance; cf. part VI). A corollary to this observation is of course that the significance of Gödel's incompleteness theorem is a subtle business too. For if we do not restrict ourselves to complete systems even when they *are* available (as in the examples of algebraically or number-theoretically pure methods on pp. 165–166), then incompleteness has lost its apparent philosophical sting: since its raw interest is clear, it is a *problem* to analyse its interest(s), philosophical or otherwise.

Below, reversing the historical order, Gödel's work in (4) on incompleteness, in particular, of any (pure) formalization of the elementary theory of $+$ and $\times$ on the natural numbers, mentioned on p. 166, will be presented first, because it requires less background on 'abstract nonsense' about logical validity, needed for (1). Applications of (4) and (1) are summarised on pp. 174–176 and pp. 180–183, followed by a broad discussion of their bearing on the most popular foundational schemes.

Formalization and numerical computation: generalities

Ever since the introduction of Frege's rules, it was evident that numerical computation was a particular case, and in some ways, even typical of all formal deduction. Thus computations of polynomials with integral coefficients and arguments ($\geqslant 0$) are formal deductions from the (elementary) axioms:

$$n+0 = n, \quad n+m' = (n+m)' \quad \text{where } ' \text{ means the successor,}$$

$$n \cdot 0 = 0, \quad n \cdot (m') = (n \cdot m)+n,$$

where the rules are substitution, and equating equals to equals (a computation evaluates an expression without variables as a *numeral*, $0, 0', 0'' \ldots$). Computations can be checked mechanically, and so the formalization above is *complete for equations between numerical expressions*, say t_1 and t_2:

If $t_1 = t_2$ is true (for the usual interpretations of $0, ', +, \cdot$) then

$t_1 = t_2$ can be derived by the rules above.

If, further, the usual formal rule for existential quantifiers is added, and a diophantine equation, say $P = Q$ in the variables $x_1, \ldots x_n$, has integral solutions then $\exists x_1 \ldots \exists x_n (P = Q)$ can be formally derived by the rules mentioned: the latter are *complete for solvability of diophantine equations*. More generally, one expects some sort of parallel between

formal derivability and solvability of diophantine equations,

stressed early in the century by Hilbert who saw here a unity between school boy arithmetic and all of (formalized) mathematics. Taken literally, the parallel equates Hilbert's tenth problem—to give a general method for deciding whether any diophantine equation has a solution—with deciding whether any formula F has a derivation by means of given formal rules $\mathscr{F}$. This remained, in fact, the source of Hilbert's later conjectures: the pay-off for replacing the allegedly difficult abstract notion of truth by the apparently wholly manageable notion of formal derivability, was to have been the effective decidability of derivability in properly formalized branches of mathematics—as in the case of real closed ordered fields.

Today we know that the parallel above holds literally in that there is one diophantine equation (in just 9 variables!) with a parameter p and an effective way of finding a value of p corresponding to any pair $(F, \mathscr{F})$. Hilbert's tenth problem has a negative solution, and his conjectures about the decidability of formal derivability were false.

But at the turn of the century, in fact, up to (4), weaker variants of the parallel had not been excluded, for example, that no one formal system 'coded' all formal procedures (for each set of rules $\mathscr{F}$, derivability in $\mathscr{F}$ is formally decidable, but not by a method adequately represented in $\mathscr{F}$). But the price for this possibility would have been high since the obviously elementary character of (verifying) formal derivability would not be reflected in an adequate definition, $D_{\mathscr{F}}$, for: derivability by means of $\mathscr{F}$. Part of the work in (4) established the definitional 'adequacy'—technically, completeness for formal derivability—of a general class of formal systems including (Hilbert's) *pure number theory*, the system derived by the passage on pp. 165–166 from Peano's axioms together with the recursion equation for $+$ and $\cdot$.

As a final preliminary, a curious blind spot has to be mentioned. In all the discussions of decidability and completeness (of formalizations) in the first three decades of this century, an obvious connexion was not noticed; completeness, for example, of pure number theory would yield the following decision method in a 'finite number of steps' for formal derivability, in particular, for Hilbert's tenth problem. Given a diophantine equation $P = Q$ in n variables, it is enough to lay out the formal derivations in some ω order, try them out one by one, until a derivation of either $\exists x_1 \dots \exists x_n (P = Q)$ or of $\forall x_1 \dots \forall x_n (P \neq Q)$ is reached. Completeness ensures that this process terminates. This blind spot is a glaring oversight if one means 'finite number of steps' literally, without consideration for the practical value of such a method by trial and error.

Incompleteness of formal systems for number theory and beyond

To fix ideas, the reader may wish to think of pure number theory. In any case no details of the system will be used in the simple sketch below, which supports Gödel's claim, on p. 150, that (4) did not need new mathematics. In fact, the sketch uses only (i) Cantor's diagonal argument (the class of all sets of natural numbers is not enumerable), here applied to sets and enumerations defined by restricted means, and (ii) the particularly elementary character of the set of formally derivable formulae (compared above to the set of solvable diophantine equations), so to speak, the *raison d' être* of formalization itself.*

* As already mentioned on p. 164, more efficient expositions are available in the literature. The interest of the proof below is that it follows Gödel's presentation in his letter to Zermelo, already mentioned on p. 154, rather than his publication (4), where a relation to the so-called liar paradox is prominent. (In conversation Gödel could not resist the temptation of paradoxical formulations. In publications he dramatized the trauma of ever having been taken in himself by a paradox.) Zermelo's criticism, though clumsily worded, was closely related to (i), the inadequacy of any formal language for defining (all) sets of natural numbers, but failed to stress (ii). So, in particular, it fails to pin-point the difference between the formal systems (of number theory) involved in the incompleteness theorem and those on pp. 165–166 for the field of real numbers or the semi-additive group of natural numbers, which do decide every formula.

(i) Let $\mathscr{C}$ be a class of (formulae defining) number-theoretic predicates with one and two arguments, closed under identification of variables and negation; thus if $F(n, m)$ is in $\mathscr{C}$, so is $\neg F(n, n)$. Then there is no (binary) predicate in $\mathscr{C}$ which enumerates all (monadic) predicates in $\mathscr{C}$. This means, as usual, that no formula $F(n, m)$ in $\mathscr{C}$ has the property:

for each formula $G(m)$ of $\mathscr{C}$ there is a number $\bar{g}$: $\forall m[F(\bar{g}, m) \Leftrightarrow G(m)]$.

A counter example is obtained by taking $\neg F(m, m)$ for $G(m)$, and putting $m = \bar{g}$.

Warning: In contrast, there is in general no obstacle to enumerating all *formulae G* by giving them numbers in such a way that simple syntactic operations are defined by formulae in $\mathscr{C}$, for example, ***substitution*** σ:

> $\sigma(g, n)$ is the number of the formula (without variables) obtained when the variable in G with (one variable and) number g is replaced by the nth numeral (in some standard notation, like $0, 1, 1+1, (1+1)+1, \ldots$ or $0, 0', 0''$ on p. 168).

Far from being subtle, the difference is so crude that in ordinary mathematics it would hardly be mentioned; for example, in the case of polynomials $x^n + a$, say, with numerical a, the *defining* expressions are numbered by an enumeration of the pairs (n, a) which can certainly be done polynomially by: $\frac{1}{2}(n+a+1)(n+a)+n$. But an enumeration of the functions $x^n + a$ *defined* by these expressions, which is a function of triples (x, n, a), cannot be done polynomially. (Here, in the case of functions, we have $=$, where in the case of predicates above we had $\Leftrightarrow$.) Incidentally, contrary to a widespread misunderstanding, there was nothing particularly novel in Gödel's numbering of formulae or derivations, that is, finite sequences of formulae: this was implicit in Cantor's well-known enumeration of finite sequences of elements taken from an enumerated set.

(ii) Let $\mathscr{F}$ be a formal system, given with a numbering of its formulae with one or no free variable, where (the value of the numeral) $\bar{n}$ is the number of the formula N. Then $\mathscr{F}$ *is incomplete*, provided some formula D of $\mathscr{F}$ (called $D_{\mathscr{F}}$ above) defines *derivability* (in $\mathscr{F}$), and $\mathscr{F}$ is *sound*, that is, for formulae N without free variables

$$(\Rightarrow) \text{ if } N \text{ is derivable then } N \text{ is true.}$$

For if $\mathscr{F}$ were complete, $D[\sigma(n, m)]$ would define an enumeration of the monadic predicates of $\mathscr{F}$, which, by (i), must fail at $D[\sigma(\bar{g}, \bar{g})]$ where $\bar{g}$ is the number of $\neg D[\sigma(m, m)]$ with variable m:

$$\text{neither } D[\sigma(\bar{g}, \bar{g})] \text{ nor } \neg D[\sigma(\bar{g}, \bar{g})] \text{ is derivable.}$$

The false one is not derivable by $(\Rightarrow)$, the other (true) one is not derivable because of (i). Now, $\sigma(\bar{g}, \bar{g})$ is the number of $\neg D[\sigma(\bar{g}, \bar{g})]$, and the latter is not derivable, but D is assumed to define derivability. So $\neg D[\sigma(\bar{g}, \bar{g})]$ is true, but not derivable.

At the time there was great interest in weakening the conditions on $\mathscr{F}$ and D, especially ($\Rightarrow$), that is, to avoid the reference to truth (of N) in favour of derivability. Inspection of the argument above leads to:

(*) if $D(\bar{n})$ is derivable, so is N,

(**) if $\neg D(\bar{n})$ is derivable, then N is not derivable, and

(=) if $\bar{g}'$ is the numerical value of $\sigma(\bar{g},\bar{g})$, then $D(\bar{g}')$ and $D[\sigma(\bar{g},\bar{g})]$ (and their negations) are jointly derivable.

(a) Derivability of $\neg D(\bar{g}')$ and so, by (=), of $\neg D[\sigma(\bar{g},\bar{g})]$, contradicts (**) for $\bar{n} = \bar{g}'$ since G' is $\neg D[\sigma(\bar{g},\bar{g})]$ itself. (b) Derivability of $D(\bar{g}')$ implies, by (*), that G' be derivable, contrary to (a). On p. 173 the conditions above will be further weakened. In particular, in accordance with the basic parallel on p. 168, between (checking) computations and derivations, the converse of (*) will be used (for relevant N: the *completeness* of $\mathscr{F}$ for *derivability* in $\mathscr{F}$ expressed by D.

Gödel (4) gave a detailed verification of (*) and (**) for a specific definition $D_{\mathscr{F}}$, his $\mathscr{F}$ being (an improved formulation of) the system of *Principia Mathematica*, which claimed to give a 'complete' formalization of mathematics. But (4), p. 190, gave also general conditions on systems $\mathscr{F}$ to which the argument applies, and soon afterwards the analysis of Turing showed that arbitrary formal systems containing a certain minimum of number theory (or of the theory of finite sets) satisfied those general conditions.

By p. 168, today $D(\bar{g}')$ can be replaced by the assertion that a certain diophantine equation has a solution. But technically it was certainly much easier to find $D(\bar{g}')$, an assertion about formal derivability in P.M. which is undecided in P.M. (than one in familiar mathematical terms).

Gödel's strategy for going into details, further elaborated in Kleene (1978) and its review, avoided controversy. But even without those details, the proof given here, on the *assumptions* (*) and (**), establishes beyond a shadow of doubt the following inadequacy of (any) formal systems $\mathscr{F}$ for elementary number theory, already raised on p. 168:

> *Either* such a simple notion as formal derivability cannot be defined in $\mathscr{F}$ in the sense of (*) and (**) in $\mathscr{F}$ is incomplete (in the sense that a true formula, namely $\neg D(\bar{g}')$, cannot be derived in $\mathscr{F}$).

For specialists: an even more general argument of the same type applies to the case of set theory for any set $\mathscr{A}$ of axioms, not only for formal systems. Suppose $\mathscr{A}$ can be justified at all for the intended meaning, set out in Zermelo (1930) and on pp. 190–191 below, that is, for appropriate segments of the cumulative hierarchy, including the segment α. Then *either* α cannot be defined in set-theoretic language *or* if D_α is a definition then $\exists x\, D_\alpha(x)$ is not decided by $\mathscr{A}$. Of course, $\exists x\, D_\alpha(x)$ is about the 'abstract nonsense' of sets while $D_{\mathscr{F}}(\bar{g}')$ above is about the 'formal nonsense' of derivability.

Actually, Gödel's own proof, in terms of definability, is so simple that it can be applied to situations which have little in common with formal systems; to sets of axioms which are not recursively enumerated or 'listable', to languages with infinitely long formulae or so-called infinitary rules, and the like; cf. the sections on such matters in Barwise (1977). Some of these generalizations are in fact needed in connection with the new questions, on p. 174, which involve a *rethinking of the role of*—necessarily incomplete—*formal systems in mathematics*. But first it is appropriate to go into a reformulation of $\neg D(\bar{g}')$, which attracted great attention in the first decade after (4), called 'Gödel's second incompleteness theorem'; it comes under the heading:

Consistency and consistency proofs

Consistency (of $\mathscr{F}$, for short: Con $\mathscr{F}$) means that there is no formula F for which both F and $\neg F$ can be derived in $\mathscr{F}$. Since $\neg$ is intended to mean: not, $\mathscr{F}$ had better be consistent. This is not at issue. Rather,

What use is (mere) consistency?

Once again, diophantine problems are typical. Let P be a polynomial with integral coefficients and $(n_1, \ldots, n_k)$, for short: $\boldsymbol{n}$, a list of its variables. In the technical jargon of p. 168, $\mathscr{F}$ is to be complete for computation (and hence for computability: if $\exists \boldsymbol{n}\,(P = 0)$ is true, it is formally derivable in $\mathscr{F}$; the analogue for derivability, $D(\bar{g}')$, came up on p. 171).

Significance of Con $\mathscr{F}$: If $\forall \boldsymbol{n}(P \neq 0)$ is derivable in $\mathscr{F}$, then it is true (that P has no solution in integers). For, by completeness for computations, if $(\bar{n}_1, \ldots, \bar{n}_k)$ were a solution, $\bar{P} = 0$ and hence $\neg \forall \boldsymbol{n}\,(P \neq 0)$ could be derived in $\mathscr{F}$. So $\neg$ Con $\mathscr{F}$, when $\forall \boldsymbol{n}\,(P \neq 0)$ is the formula F in the definition of Con $\mathscr{F}$. This 'significance' of Con $\mathscr{F}$ is also relevant to the matter of number-theoretic *purity*, already referred to on p. 167. Suppose Con $\mathscr{F}$ and the completeness of $\mathscr{F}$ for computation are both proved by 'pure' methods, for example, in Hilbert's pure number theory. Then if $\forall \boldsymbol{n}\,(P \neq 0)$ is derivable in $\mathscr{F}$, it also has a pure proof (with an obvious extension to other preferred methods of proof).

Warning. The consequences above of Con $\mathscr{F}$ obviously do not extend to formulae $\exists \boldsymbol{n}\,(P = 0)$ since, if $\forall \boldsymbol{n}\,(P \neq 0)$ is formally *undecided* in $\mathscr{F}$, the *false* formula $\exists \boldsymbol{n}\,(P = 0)$ can be added consistently. (Thus, (*) on p. 171 is not assured by Con $\mathscr{F}$.) As mentioned earlier, Gödel gave that warning in (3) in the clearest possible terms, actually before the discovery of incompleteness. But (3) made much less of an impression than such *conneries* as:

> In mathematics, consistency ensures existence (of what?).
>
> An inconsistent system would be dull because every formula, G, could be derived in it, by use of $\neg F \Rightarrow (F \Rightarrow G)$.

In later 'popular' writings, Gödel always treated such *conneries* respectfully.

Gödel's second incompleteness theorem (described by him, on p. 196 of (4), as *merkwürdig*: a curiosity). Inspection of the proof of (a) on p. 171 shows that $\mathrm{Con}\,\mathscr{F} \Rightarrow G'$ is derivable (in $\mathscr{F}$) provided $\mathscr{F}$ is *demonstrably* complete for derivability (defined by D) since then condition (**) follows from $\mathrm{Con}\,\mathscr{F}$. So $\mathrm{Con}\,\mathscr{F}$ *is not derivable* (since G' is not). A looser formulation says: $\mathrm{Con}\,\mathscr{F}$ *cannot be proved* (in $\mathscr{F}$) *if* $\mathrm{Con}\,\mathscr{F}$ *can be proved to be significant* in the sense above.

Examples of formal systems which do prove their own consistency. Given formal rules $\mathscr{F}$, and a numbering of their derivations, we pass to new rules $\mathscr{F}_1$ by adding the following requirement on derivations (with number d and end formula F_d):

For all pairs of (the finitely many) preceding derivations in $\mathscr{F}$, that is, $d' \leqslant d$ and $d'' \leqslant d$, the end formula of one is not the formal negation of the other ($F_{d'}$ is not the formula $\neg F_{d''}$).

Evidently, $\mathrm{Con}\,\mathscr{F}_1$ is proved in the most elementary way: we stop before an inconsistency turns up. But also: if $\mathscr{F}$ is consistent then

$\mathscr{F}$ and $\mathscr{F}_1$ have not only the same theorems but the same derivations!

only the procedure for *checking* derivations is more elaborate in $\mathscr{F}_1$. Also—and this is philosophically interesting—though logical texts rarely consider systems like $\mathscr{F}_1$, the latter mirror quite well, albeit crudely, an essential method used in practice for checking proofs: *comparison with background knowledge* (here represented by d': $d' < d$).

As a corollary, $\mathscr{F}$ is seen to be incomplete provided (i) $\mathscr{F}$ is consistent and (ii) demonstrably complete for derivability (in $\mathscr{F}$ and $\mathscr{F}_1$, defined by D and D_1, resp.). This improvement was mentioned on p. 171. In the notation used there, if $\bar{g}_1$ is the number of $\neg D_1[\sigma(m, m)]$,

$$\text{neither } D(\bar{g}_1') \text{ nor } \neg D(\bar{g}_1') \text{ is derivable}$$

(in $\mathscr{F}$ or, equivalently by (i), in $\mathscr{F}_1$). N.B. There are $\mathscr{F}$ satisfying (i) and (ii), in which $D(\bar{g}')$ *is* derivable!—As on p. 171, (a) $\neg D_1(\bar{g}_1')$, and hence $\neg D(\bar{g}_1')$ is not derivable because condition (**) is ensured by (i) and (ii), as mentioned above. (b) If $\bar{g}_2$ is the number of $D_1(\bar{g}_1')$, and hence G_1' is the formula $\neg G_2$ then, by $\mathrm{Con}\,\mathscr{F}_1$ (derivable in $\mathscr{F}_1$)

$$D_1(\bar{g}_2) \Rightarrow \neg D_1(\bar{g}_1')$$

is derivable in $\mathscr{F}_1$. So if G_2 were derivable, by (ii), $D_1(\bar{g}_2)$ would be, and so $\neg D_1(\bar{g}_1')$ would be derivable, contrary to (a). Specialists will find further references to the literature on pp. 113–114 of Browder (1976); specifically, there is the matter of so-called canonical numberings of formulae and derivations (unique up to formal equivalence), which are perfectly analogous to coding finite sequences of sets by sets (unique up to appropriate equivalences), but also—and this is much more interesting—novel questions arise concerning the second incompleteness theorem for systems which were not known at the time of (4), for example, so-called cut-free rules.

Consistency proofs. As Gödel himself stressed, back in 1931 on p. 197 of (4), his second theorem is irrelevant to any sensible consistency problem. In any case, if Con $\mathscr{F}$ is in doubt, why should it be proved in $\mathscr{F}$ (and not in an incomparable system)? Gödel's practice followed his theory. His last self-contained publication, (24) in 1958 which goes back to (11), was presented as a consistency proof. Between 1931 and 1958, as his notebooks at Princeton show, he studied other such proofs, especially one discovered around 1935, but published posthumously in Gentzen (1969), pp. 201–213; cf. p. 262 of its review for its checkered history, now more fully documented by the correspondence between Gentzen and P. Bernays, left by the latter to the E.T.H. at Zürich.* Very much in contrast to the break with traditional aims, advocated throughout this memoir, Gödel continued to use traditional terminology. For example, the original title of Spector (1962), extending (24)*, did not contain the word 'consistency'; it was added for the posthumous publication at Gödel's insistence. He knew only too well the publicity value of this catchword, which —contrary to his own view of the matter—had made his second incompleteness theorem more spectacular than the first.

Some lessons from the incompleteness theorems

(a) As far as the first theorem is concerned, there are two lessons (independent of the foundational schemes which, as already mentioned, are left for the end of part II). The first and principal lesson is related to the questions on p. 162:

What more can we expect to know from a proof of a theorem by means of (incomplete) rules for, say, number theory or set theory than if we merely know that the theorem is true? And, of course, as a corollary:

What do we know about a problem if it is not decided by given rules?

At least at the present time, it is not so much the *general* incompleteness theorem for formal systems that has found uses, but incompleteness tied to specific objects, like (size of) ordinals in the easy argument (for specialists) on p. 171 or rate of growth on number-theoretic functions in the examples below.

The second, subsidiary lesson is that, in the cases mentioned, incompleteness of suitable *informal* systems is needed for the most rewarding results; in other words, the generalizations mentioned on p. 172.

The points above are illustrated by theorems of the form: some diophantine equation, D, in say 9 variables (by p. 170, the typical case), has *infinitely* many solutions

$$(*) \qquad \forall n\, \exists m_1 \ldots \exists m_9 [m_1^2 + \ldots + m_9^2 > n \,\&\, D(m_1, \ldots, m_9) = 0].$$

If (*) is *true*, then for each n, such $m_1, \ldots, m_9$ can be computed by a programme which tries out each 9-tuple, in short, recursively. But if (*) is *proved* by

* For specialists. Gentzen used functionals of lowest type, defined by unfamiliar equations, intended to operate on so-called choice sequences; in other words, with special emphasis on continuity. Gödel's (24) used functionals of all finite types, defined more elegantly, but intended to operate on rules (except that the last sentence of (24) does not fit the intention). Spector used so-called bar recursion, again for all finite types, for which continuity (in a suitable sense) is again essential.

restricted means, $\mathscr{F}$, bounds for $m_1^2 + \ldots + m_9^2$ (in terms of n) can be specified. The literature speaks of demonstrably recursive functions, defined by the class $\mathfrak{R}_{\mathscr{F}}$ of programmes which can be proved (in $\mathscr{F}$) to terminate. A significant part of proof theory describes the functions defined by $\mathfrak{R}_{\mathscr{F}}$ in familiar mathematical terms. (This was the aspect stressed in the original title of Spector (1962) before Gödel insisted on adding 'consistency'; cf. p. 174.)

An obvious conclusion is that even if (*) is true, but the least $m_1^2 + \ldots + m_9^2$ grows too rapidly with n, then (*) cannot be proved by means of $\mathscr{F}$. The converse is not true because, for some N,

$$(\dagger) \qquad \forall m_1 \ldots \forall m_9 [m_1^2 + \ldots m_9^2 > N \Rightarrow D(m_1, \ldots, m_9) \neq 0]$$

may be true, but not derivable in $\mathscr{F}$. A very simple piece of logic shows that the converse does hold if all true propositions of the form (†) are added to $\mathscr{F}$. (The resulting system is not formal! but needed.) If $\mathscr{F}^+$ is the new system, the class $\mathfrak{R}_{\mathscr{F}}^+$ of programmes is greater than $\mathfrak{R}_{\mathscr{F}}$, but not the class of functions defined. Thus *metamathematical* knowledge of

underivability of (*) in $\mathscr{F}^+$ gives information about bounds for (*),

of ordinary *mathematical* interest. Though this connection had been publicized for more than 20 years, the first convincing use was made only recently; cf. the chapter by Paris and Harrington in Barwise (1977) on a problem in (combinatorial) partition theory. For logically more complex assertions than (*), a more sophisticated connection by means of so-called functional interpretations is used; cf. p. 111 of Browder (1976).

Without exaggeration: the answers above totally reverse the unsophisticated aims of using formal systems for an overview of mathematics, for example, for arranging problems according to the means needed for their solution. The new aim is to start with a problem P (one wants to know about), and to look for a bunch $\mathfrak{F}_P$ of *relevant* systems (in the arsenal of systems with manageable metamathematical properties). The metamathematical study, proof-theoretic or model-theoretic, of particular systems $\mathscr{F}$ is here only a preliminary, for example, to get some idea of the sort of problems P' for which $\mathscr{F} \in \mathfrak{F}_{P'}$. As on p. 167, a different strategy would apply if ever genuinely fundamental systems turned up, for example, related to the laws of thought (on p. 162).

(b) As to uses of the second incompleteness theorem, above all, it provided the first, much needed *cross check on proposed consistency proofs*. The early literature on the subject—supposed to 'secure' mathematics!—had a particularly high density of errors. The most famous are in Ackermann (1924), pointed out on pp. 44–46 of von Neumann (1927), and in Herbrand (1930), corrected in Dreben *et al.* (1963), also discovered, but not quite corrected by Gödel in the early forties; cf. his *Arbeitshefte* IV and V at Princeton. (Though in the meantime others have also observed the points of detail made there by Gödel, his sure touch remains exceptional.) Another good use of the second theorem, which however always requires some imagination, can be seen as follows, by reference to the basic significance of Con$\mathscr{F}$ (on p. 172): if

$\forall \boldsymbol{n}\,(P \neq 0)$ is derived in $\mathscr{F}$ from the *false* formula $\neg\,\mathrm{Con}\,\mathscr{F}$, $\forall \boldsymbol{n}\,(P \neq 0)$ is still true, simply because $\mathrm{Con}\,\mathscr{F}$ is not derivable, and so $\mathscr{F}$ together with $\neg\,\mathrm{Con}\,\mathscr{F}$ is consistent. An easy exercise shows that $\forall \boldsymbol{n}\,(P \neq 0)$ is then derivable in $\mathscr{F}$ itself (but such a derivation may be more difficult to find). An even better use relies on the details of consistency *proofs* which derive $\mathrm{Con}\,\mathscr{F}$ from some 'mathematical', manageable principle, say P. So the *false* formula $\neg\,P$ is consistent with $\mathscr{F}$ too. For suitably complex P

F can be derived in $\mathscr{F}$ itself if F can be derived from $\neg\,P$,

even for certain F which are (logically) much more complicated than $\forall \boldsymbol{n}\,(P \neq 0)$; cf. pp. 116–117 of Browder (1976) and p. 197 of part III below (on consequences of the continuum hypothesis) for examples.

The next topic is the completeness theorem for elementary logic in the sense explained on p. 165.

Elementary logic in the twenties: background to (1)

Evidently, to document Gödel's own view (p. 150) on his good use of traditional philosophical notions in (1), a word on the knowledge about elementary logic which had accumulated before (1), is needed. For balance, other interesting consequences of that early knowledge, which its authors did not recognize, will be used to illustrate negative effects of (ill digested) traditional philosophical aims.

Remark. An at least comparably important obstacle to progress in the twenties was the emphasis on the false conjecture that logical validity or formal derivability by means of Frege's rules was mechanically decidable. As a result a fair number of partial, and certainly not very memorable results cluttered up the literature of the twenties. As on p. 169, before (4) it was not realized that a proof of the conjecture would have solved Hilbert's tenth problem (and more, as Gödel stressed on p. 194 of (4) in his discussion of the matter); for the diophantine equation $P = Q$ has a solution if and only if $\exists x_1 \ldots \exists x_n\,(P = Q)$ follows purely logically from the usual axioms for successor, addition, and multiplication; cf. p 168 above.

(a) *Non-categoricity of elementary axioms.* One of the best known results had been realized before 1920—by Loewenheim, and proved very simply by Skolem. The existence of non-isomorphic models of Euclid's axioms, or of real-closed ordered fields, mentioned on p. 164 and p. 166, is typical of all elementary axioms. In particular, if each of a countable collection of such axioms is true for some structure S, there is a countable (or finite) part of S, say S_0, in which they are all true too. The idea of the proof, which incidentally is the clue to Gödel's work in part III, is perfectly illustrated by means of the formula $\forall x \exists y \forall z\, R\,(x, y, z)$ where R does not contain quantifiers ($\forall, \exists$). For if $\forall x \exists y \forall z\, R\,(x, y, z)$ holds in S, so does $\forall x \forall z\, R\,[x, Y(x), z]$ for some function Y with arguments and values in S. For any element $\boldsymbol{a}$ of S, the set S_0 generated from $\boldsymbol{a}$ by Y will do (where $\boldsymbol{a} \in S_0$ and if $b \in S_0$ also $Y(b) \in S_0$); thus

$S_0 = \{a, Y(a), Y[Y(a)], \ldots\}$. (If there are also operation, not only relation symbols in R, S_0 is required to be closed for the corresponding operations.) Evidently, S_0 is countable or finite. So if S is uncountable, then

$$\forall x \exists y \forall z R(x, y, z)$$

is not categorical; this is generalized on p. 180.

Readers probably know—and certainly can easily imagine—the thoughtless conclusions which were drawn from the simple results above. At one extreme, differences between infinite cardinalities were rejected as 'meaningless' because such cardinalities cannot be distinguished by elementary properties. At the other extreme the elementary formalism or 'language' was rejected as hopelessly inadequate because it cannot be used to express even such brutal properties as differences in cardinality.

What was overlooked for a remarkably long time, was the *positive* aspect of the results above: from the validity of an elementary formula in all countable structures follows its validity in all uncountable structures too. Without exaggeration: some result of this kind is needed to make the 'abstract nonsense' about validity in arbitrary structures (called 'truth in all possible worlds' on p. 160) useful at all; not because of any illegitimacy of the notions involved but because a formula might fail to be logically valid only because it is false in some odd structure that nobody wants to know about. Specialists can think of examples in so-called second order classical logic, and to some extent in intuitionistic propositional logic where propositions about so-called lawless sequences are needed; cf. p. 186 below.

Bibliographical remark. Skolem (1922) went even further in reducing the 'abstract nonsense'. Suppose F is an elementary formula with relation symbols $R_1, \ldots, R_m$. Then relations $R_1^F, \ldots, R_m^F$ can be (quite explicitly) defined in pure number theory with the property: if F_ω is obtained from F by replacing the symbols R_i by R_i^F, and if the quantifiers in F_ω range over the natural numbers ($+$ and $\cdot$ in R^F having their usual number-theoretic meaning) then:

if F is true in any structure at all, or, as one says,

if F has a model then F_ω is true (for the natural numbers).

In other words, logical validity is not only equivalent to validity in countable structures, but to validity in structures defined in this restricted way. Skolem himself did not state this result: he noticed only the very marginal improvement that, in contrast to his earlier proof of Loewenheim's theorem, the proof of the refined result did not use the axiom of choice.

(b) *Two formulations of completeness* (for logical validity). One formulation occurs in Hilbert-Ackermann (1928), obviously written by the second co-author. It says just what one would expect. A system of rules, say $\mathscr{L}$ (for 'logic'), formulated in terms of elementary logic is *complete* if, for every elementary formula F,

F is derivable in $\mathscr{L}$ provided F is logically valid that is, true in all structures (in which the relation symbols of F are interpreted). Pedantically, one can also consider the converse, called *soundness* of $\mathscr{L}$, which is usually verified by inspection.

There is an obvious analogous notion of *completeness for logical consequence* (of F from a set $\mathscr{F}$ of formulae); in the case of a finite set $\mathscr{F}$, say, $\{F_1, \ldots, F_n\}$ this reduces to validity of $(F_1 \& \ldots F_n) \Rightarrow F$. It will not have escaped the reader's notice that the matter of completeness is neatly by-passed in the formalizations on pp. 165–166, which were Hilbert's principal interest, since they ***decide*** every proposition: no set of sound rules can do more! (in the sense of generating more theorems). Further—and this certainly did not escape Hilbert's notice!—the completeness in question is formulated *purely formally*: for every elementary formula F (about the structure considered) either F or $\neg F$ is formally derivable. Sure, the reason for being interested in this formal property is that it ensures that all true F are derivable in the formalization. But the wording respects the ideal of *Methodenreinheit* (here applied to formal derivability), and the formulation in Hilbert-Ackermann (1928) violates it.

Soon afterwards, despite the handicap of a recent stroke, Hilbert (1930) tried to correct this violation by a pure version of:

Completeness of $\mathscr{L}$ modulo a formal system, say $\mathscr{Z}$, for pure number theory:

$$(F \text{ is derivable in } \mathscr{L}) \text{ or } (\neg F_\omega \text{ is derivable in } \mathscr{Z}),$$

where F_ω is obtained from F when the relation symbols, say R, of F are replaced by suitable expressions of $\mathscr{Z}$, for example, the definitions R^F supplied by the improvement in Skolem (1922), mentioned in (a). Once again the blindspot (cf. p. 169) intervened. Hilbert and others overlooked the fact that completeness in his pure sense would prove the (false) conjecture on p. 176, providing a method for deciding in a finite number of steps whether F is derivable in $\mathscr{L}$ (provided of course that $\mathscr{Z}$ is sound). As on p. 169, one lays out the formal derivations of the systems $\mathscr{L}$ and $\mathscr{Z}$ in linear order and tries them out alternately. After a finite number of steps one arrives either at a derivation in $\mathscr{L}$ of F or at one in $\mathscr{Z}$ of a formula of the form $\neg F_\omega$. Since the conjecture is false, so is Hilbert's pure version of completeness: for *any* (sound) rules $\mathscr{L}'$ and $\mathscr{Z}'$ for logic and (extensions of) number theory resp.

(c) Before (1) a good deal of formally pure work, concerning transformations of formal derivations, had been done, particularly, the very original work of Herbrand (1930), already mentioned on p. 158. Without exaggeration: as we see things now, part of the interest of this kind of work comes from the fact that it does *not* presuppose completeness of the formal rules, since, for example, a transformation may be particularly efficient if applied to derivations that happen to be built up by an incomplete *sub*set of given, possibly complete rules. But it was hard to see this highly positive side of the matter. For one thing, there was no hint of it in Herbrand (1930). In addition, there were the formal errors mentioned on p. 175. Last but not least, there was the terribly complicated though correct formulation of his *Théorème fondamental*, again without a hint of possible uses in ordinary mathematics. (The irony of the matter is that even the simplest case of the *Théorème*, applied to purely existential formulae, has turned out to be at least as useful as the completeness theorem, especially if one is interested in explicit bounds.) Instead, Herbrand used the *Théorème* to get some not at all memorable, partial results on the ill-fated decision problem for elementary logic (on p. 176). Concerning completeness, *Remarque* 2 of §6.4 refers to (the possibility of) a proof but rejects the matter out of hand because the abstract so-called semantic notion of logical validity was not precise enough for Herbrand (to deserve attention).

In short, though the completeness problem solved in (1) had been stated in the twenties, there were mixed feelings about it. By (b) it certainly did not fit in with the ideal of purity of method. By (c), at least one formal counterpart to completeness, in Herbrand (1930), simply had more mathematical content. What Herbrand overlooked was that another step was needed before the average logician or mathematician had enough confidence in the subject to want to look at a monster like the *Théorème fondamental*. In contrast, almost anybody could understand completeness (or misunderstand it, thinking of it as a confirmation of Hilbert's aim). Being simple and memorable, it helped to put elementary logic 'on the map'.

Elementary logic: completeness and finiteness theorems

In (1) Gödel established the impure version of the completeness of Frege's rules in the sense explained in (b) above. Translated into the notation used in (a), (1) shows that, for every elementary formula F,

(*) (either F is derivable by Frege's rules) or ($\neg F_\omega$ is true).

A comparison with Skolem (1922) documents beyond a shadow of doubt—for anybody prepared to look at the proof of Skolem's dull result—Gödel's view that all ingredients needed for the proof of the completeness theorem were available in the twenties.

At this point it is worth recalling Hilbert's pure version of completeness which was seen to be false (on p. 178). If differs from (*) in one place:

derivable in $\mathscr{X}$ in place of *true*;

yet, the difference is quite essential. (Derivability of $\neg F_\omega$ can be mechanically verified whenever it holds, truth of $\neg F_\omega$ in general cannot, in contrast to Hilbert's expectations on p. 168.)—*Warning*. Inspection of (1) shows that

(either F is derivable by Frege's rules or $\neg F_\omega$) is derivable in $\mathscr{X}$.

But *this* replacement of 'true' by 'derivable in $\mathscr{X}$' is of no obvious consequence —if anything, it hides the essential differences between the pure and impure versions. A moment's thought shows that it is typical of the ritual of formalization.

To return to (1), Gödel noted on p. 358 in *Satz X*, more or less, another fact which is much more often used (in the sense of being directly appealed to) in applications of elementary logic.

Finiteness theorem (for infinite sets $\mathscr{F}$ of formulae). If each finite subset of $\mathscr{F}$ has a model so does $\mathscr{F}$ itself. As a corollary, the non-categoricity result on p. 176 extends to all sets $\mathscr{F}$ with infinite models as follows: for an arbitrary set I (of new constants), $\mathscr{F} \cup \{i \neq j\colon \text{for distinct } i,j \in I\}$ has a model of cardinality $\geqslant \operatorname{card} I$.

Actually, Gödel stated the theorem only for countable $\mathscr{F}$ although it holds for arbitrary $\mathscr{F}$, and although he himself formulated corresponding results for uncountable sets of propositional formulae in (8). But applications in mathematics where the unrestricted formulation is actually required, were discovered only later as the subject of *model theory* (of elementary logic) developed. Incidentally, Gödel stated the finiteness theorem in impure terms first, in *Satz IX* on p. 357 of (1), mixing in formal derivability: $\mathscr{F}$ has a model or else some finite subset, say $(F_1, \ldots, F_n)$, is formally inconsistent, that is,

$$\neg\,(F_1 \,\&\, \ldots\, F_n)$$

can be formally derived. (To use this form in model theory, the completeness theorem *is* needed.)

In retrospect the finiteness theorem is seen to fit in well with the only obvious sense of a formal derivation from an infinite set $\mathscr{F}$, namely, that only a finite subset of $\mathscr{F}$ be used. When asked whether this aspect had led him to the finiteness theorem, Gödel could not remember having been conscious of it at the time; and about (8) he remembered stating the result first for countable sets, and noting afterwards that the proof nowhere used countability. Realistically speaking, it is of little interest what one is *not* conscious of; in any case, Gödel never claimed to have followed consciously his heuristic principles, on p. 150, at the time of (1) and (4), but to have *discovered* later that they apply. (In contrast, his ideas in part IV and part V were developed after that discovery.) Be that as it may, there is no doubt that Gödel's views fit the later development of logic.

Elementary logic: its need for non-elementary notions

Before knowledge of elementary logic—either of elementary definitions or of formal rules—can become an effective part of our intellectual reflexes, one needs some general orientation on the kind of questions where this knowledge is likely to be relevant; this is easiest by *contrast* (with non-elementary notions).

Elementary formulae. Obviously, one needs to know results which hold for elementary, but not for all axioms; for example, the finiteness theorem above certainly does not apply to Peano's non-elementary axioms together with the infinite set of formulae $a \neq n\colon n \in \omega$ (every finite subset of the latter is satisfied by some a in models of Peano's axioms). Again, non-categoricity, on p. 176, puts a premium on non-isomorphic structures, say S and S', which share the same elementary properties, but one of them, say S', is more manageable. For then, with some imagination, one may find an elementary problem which is difficult for S' but not for S, as in the transfer results on real closed fields (p. 166). A more delicate strategy, discovered in the last 25 years, involves general operations on structures which preserve elementary properties. With a bit of luck, such operations, suitably applied to S and S', may produce isomorphic structures, thereby showing that S and S' have the same elementary properties. The (still) best-known application establishes relations between the p-adic fields and the fields of formal power series with integral coefficients modulo p. As long as only very simple questions about p-adics were treated, mathematicians got by with a vague perception of some relation between such fields (and exploited their knowledge of formal power series; cf. for example, Chevalley (1936) in the thirties). For more difficult problems, some 30 years later, the precise relation of elementary equivalence was needed; cf. pp. 132–133 of Barwise (1977) on the work of Ax-Kochen and Ershov.

Bibliographical remarks. Ershov's teacher was the first to state the finiteness theorem for uncountable sets of formulae, and used it for interesting results in group theory, cf. Malcev (1936) and (1941). Ax-Kochen make use of ultraproduct constructions, which preserve elementary properties, a fact first exploited, albeit inadvertently, in Skolem (1934) to establish the existence of a non-standard model of (all true statements about $+$ and $\cdot$ over) the natural numbers; Gödel's review dismissed this result, actually an immediate consequence of his finiteness theorem, but for formally incorrect reasons; cf. the review of Kleene (1976).

Secondly, it must be easy to *recognize* notions which have elementary definitions. This is a delicate matter, especially for the logically perceptive mathematician who has been sold on the idea that *all of mathematics is formalized*, say, in some universal system of set theory. Though, of course, elementary formulae (in the sense of p. 165) can be formally separated from the others in the universal system, the separation seems artificial, and is less easy to remember than if, following Gödel, non-elementary definitions are understood too, and so can serve for contrast. At the other extreme, less perceptive mathematicians or logicians, are led to apply their knowledge of elementary logic indiscriminately, for example, to the universal system itself—generally with disappointing results (according to the principle already quoted on several occasions about what is true in general). Specialists will easily think of such results for those non-standard models which are defined by mere use of the finiteness theorem; others can guess the kind of disappointment involved from the ritual formalization of the impure completeness theorem on p. 179.

In short, as a general rule elementary logic is most rewarding mathematically when applied to structures defined by (sets of) formulae which are elementary as they stand, not merely because they are thought of as expressions in a universal formal system. This includes of course non-elementary notions which are demonstrably equivalent to elementary ones, for example, the notions of order*able* and formally real fields. Up-to-date texts on model theory give general conditions for such equivalences (covering the standard example above). One of the rare exceptions to the general rule is elaborated on p. 199, where it *is* useful to go back to the definition of a non-elementary notion (free basis of a group) in the universal system, and to apply formal incompleteness properties of that system.

Logical inference. Readers have already a sufficiently good general idea of the difference between pure and impure proofs from the examples in real algebra on p. 166. (To be pedantic, a logically impure proof of an elementary formula F proves the *validity* of F, Val(F), not F itself.)

On the banal side—and contrary to the false impression mentioned repeatedly—the advantage of a logically pure proof hardly ever lies in greater certainty, the usually shorter impure proof being used for checking (by p. 167). But there is an advantage in *additional* information, for example, bounds (N_{16} on p. 166), which can be read off more easily from pure proofs. In contrast, unwinding of impure proofs, even if it is theoretically possible, tends to pass the point of diminishing returns; for more detail, cf. p. 111 of Browder (1976). *Warning* (against another widespread misunderstanding). Though bounds are more easily read off from pure proofs, *better* bounds are liable to be established by means of impure proofs. There is an obvious potential conflict here in restricting *both* definitions of objects *and* methods of proof (as is done in so-called doctrinaire constructivism): a given problem may have a very simple solution or bound, but this fact cannot be established by the restricted methods of proof. All this is plain horse sense.

The place of pure logical inference *within* impure proofs is more delicate. The issue is general, but most dramatic in the case of purely logical theorems. Modern mathematics provides many examples. Thus the notion of ordered field has an elementary definition, say O, and so an elementary theorem T about such fields is a logical truth: $O \Rightarrow T$. But the latter, or rather $\mathrm{Val}(O \Rightarrow T)$, is often established by impure proofs, involving the embedding of ordered fields in particular real closed fields, and applying set-theoretic and topological operations to the latter. The heart of the proof is to spot relevant (set-theoretic or topological) properties P of the structures so obtained; only the implication

$$(*) \qquad P \Rightarrow \mathrm{Val}(O \Rightarrow T)$$

is derived purely logically (often this part of the argument is not mentioned at all; in the phrase of Bourbaki (1948), the derivation of (*) is the least interesting side of the matter). Seeing those properties P makes many mathematical proofs, as has often been said, more like *perception* (with all its problems) than a sequence of formal steps.

Reminders (on the use of scientific experience). Though the examples just given of logically impure proofs are commonplace today, they were not known 100 years ago—and still are not known to many authors of logical texts, in whose own experience logically pure proofs have a much greater relative significance (frequency). Thus, except for those with uncommon philosophical talent, their limited experience is not sufficient for a correct sense of proportion on pure and impure proofs in possible mathematical reasoning. At the other extreme, some of the reservations by philosophers and mathematicians about logic depend equally on defective knowledge of this subject, but with a difference: impure proofs have not been widely advertised, while the best-known (and, often, only known) claims for the interest of logic are the pretensions about laws of thought or formal rigour mentioned on p. 162. They are considered next, in a partial review, which sharpens the general picture painted at the beginning of part II.

Foundational schemes: Russell, Hilbert, Brouwer

In each case, the logical properties of the schemes themselves will be recalled first, and then they are tested by inspection of scientific experience in the style of pp. 166–168. In (a) and (b) below the famous foundational schemes of Russell and Hilbert are reviewed briefly. In (c) Brouwer's less famous 'anti-formalist' views are explained and examined. The schemes of the 'anti-formalists' Poincaré and Zermelo are more conveniently discussed at the end of parts III and IV.

(a) Russell's aim of a *universal* system for all of mathematics has a clear logical or mathematical sense and a less obvious empirical sense. By the first incompleteness theorem the logical aim cannot be achieved. The philosophical significance of the logical aim is problematic, for reasons given already in the discussion on pp. 168–168 of Hilbert's 'universal' (complete) systems for *branches* of mathematics, which he favoured for the sake of *Methodenreinheit*.

Russell's empirical aim has been achieved, at least for *existing* mathematical practice (by use of current set theory in place of P.M.)—partly by the simple device of restricting practice to a given system. The second incompleteness theorem, in particular, footnote 48[a] of (4) on the restriction to finite types (in P.M.), shows up a defect, a kind of blind spot, of this practice. As mentioned repeatedly, formal independence from such a universal system explains (empirically) why certain well-defined problems have not yet been settled, for example, in odd corners of group theory. But—and these are empirical facts too—(i) such problems are relatively rare, (ii) by p. 161, in contrast to formal definitions of, say, Bourbaki's basic structures, the specifically formal axioms and rules of the universal system are barely mentioned in the later development, and, last but not least, (iii) those structures can be applied perfectly well to familiar objects like the natural numbers, which are normally *not* thought of as defined set-theoretically at all. By (ii) and (iii), the two properties characterizing Russell's ideal, of a system which is both formal and universal, are hardly used in practice.

It seems plain—in accordance with p. 182 on the use of scientific experience for refuting foundational schemes—that the conclusions above would be less convincing without our experience with universal systems.

(b) Hilbert's scheme is a kind of opposite extreme to Russell's empirical aim and, especially, to the doctrine mentioned on p. 161 that *only* empirical case studies can support universal systems. (As a matter of historical curiosity, neither Hilbert nor Russell ever stressed that particular difference between them.)

The difference is very well expressed by Hilbert's favourite slogan, in Hilbert (1931), which eventually replaced the modest business of purity of method: his aim was a *final solution* of all foundational problems by *purely mathematical means*. (Outside mathematics Hilbert liked big words like 'final solution', or 'world formula' in relativity theory rather than little things, like the perihelion of Mercury.) Actually, his aim is more modest than it sounds because of the tacit assumption (which alone makes the aim even remotely plausible) that only those foundational problems which concern proofs of finitist theorems are 'real'. (By p. 168, the latter are of the same general character as theorems asserting that some diophantine equation is insoluble.) The 'final solution' was to establish the autonomy 'in principle' of the subject—exactly in the same sense as pp. 165–166 establish the autonomy of real algebra.

Despite Hilbert's severe restriction, eloquently criticized in Godel's (3), the first incompleteness theorem is enough to exclude a final solution. To be final, it would have to provide a method which decides every finitist problem, so to speak: here and now (and certainly every diophantine inequality; equivalently by p. 172, its consistency with some formal system which is complete for numerical computation). Otherwise, if that system leaves the problem undecided, tomorrow we might think of another system which settles it. The new system would have to be justified, and so on *ad nauseum*.

The second incompleteness theorem is also relevant to Hilbert's scheme, but—by p. 174, and contrary to an almost universal misunderstanding—in a much more subtle way, involving the following fact of experience: (i) For any formal rules or axioms actually used in mathematical practice (in contrast to those experimented with in foundational studies), somebody has an abstract interpretation in mind which establishes their consistency instantaneously. The second incompleteness theorem refutes an additional conviction (apart from the business of a 'final solution'), formulated by Hilbert, but widely current at the turn of the century: (ii) Set-theoretic and other abstract notions constitute a mere *façon de parler*, and thus can be eliminated straightforwardly. The second theorem pinpoints a particular class of counter examples to (ii) since the specific use of abstract notions in the instantaneous consistency proofs of (i) cannot be so eliminated. (For systems which prove their own consistency, the corresponding abstract notions cannot be eliminated from the proof of the equivalence between $\mathscr{F}$ and $\mathscr{F}_1$ on p. 173.)

Though the use above of the second incompleteness theorem has unquestionable elegance and charm, detailed inspection of scientific experience establishes more. As to (i), abstract notions are essential not only for consistency proofs (which constitute a kind of singularity in mathematical reasoning), but, generally, in algebra and number theory. Moreover—and this is a philosophical defect of the aim of eliminating abstract notions from proofs—when this can be done, essential knowledge contained in the proof is liable to be lost. As to (ii), by the turn of the century, there had hardly been time to learn to use set-theoretic methods efficiently: Hilbert's conviction was quite consistent with the 'empirical evidence'—which is not the same thing as being supported by the evidence! In fact, those methods were used particularly cautiously, even though, including the paradoxes, less mistakes had been made with sets than in finitist consistency proofs in the twenties; cf. p. 175 and, for a more scholarly documentation on these and similar points, pp. 114–116 of Browder (1976).

Finally, at least as a matter of common sense, the foundational problems about the 'certainty' or 'security' of mathematical knowledge which Hilbert had in mind, do not seem at all promising. After all, though surely not the only reliable means of knowledge, mathematical proofs have long stood out by their certainty. Further analysis of that certainty, in terms of anything remotely resembling existing ideas, is therefore at best a calculated risk, and the more specific aim of increasing that certainty still further, *assumes* that the certainty already achieved is not 100%. Here it should be recalled from p. 182 that the unwinding of impure proofs into pure ones, originally presented as eliminating dubious abstract principles, simply yields other information. More generally, preoccupation with certainty is liable to draw attention away from other possibly genuinely problematic and therefore less sterile aspects of proofs.

Remark (for readers accustomed to the traditional foundational literature). The preceding paragraph obviously conflicts with several old ideas; for example, (i) contrary to the tradition going back to Descartes, doubts and assertions, including restrictions and extensions of principles of proof, are here treated symmetrically, or (ii) contrary to the opening paragraph of (19), here logic is not expected to set general norms prior to all science—not even to all mathematics. Of course, having survived, these old ideas sound plausible enough in the abstract. But they evidently conflict with scientific experience, and inspection of the latter shows up their *obvious* oversights; for example, in the case of (i), doubts can be dubious too, and, in the case of (ii), norms valid for literally all imaginable experience are liable to be useless for any particular domain. (This last point is illustrated in (a) above too by the weakness of universal systems). The price for dropping the simple-minded ideas (i) and (ii), about the nature of knowledge as one says, is high; cf. (c) below on the problems involved in a useful representation of proofs.

(c) Brouwer's *intuitionistic* doctrine is best known for its polemical side, about defects of set-theoretic definitions, and of (Hilbert's problems about) formal rules. The corresponding positive side is Brouwer's aim of doing what Russell and Hilbert neglected: to make the mental activity of proofs, not only formal derivations, into the principal subject of foundational studies. An essential, though by no means well known element of that positive side is a new interpretation of the logical operations, as maps from proofs to proofs. (Readers familiar with the intellectual climate of the first quarter of this century, mentioned already on p. 162, will recognise here the then-privileged place of mind in nature.) Naturally, for this different interpretation, some of the familiar formal laws of logic fail; what is less well known is that new ones hold.

This positive side was not much stressed by Brouwer himself, whose polemics insisted on a reform of mathematics (or at least of its exposition). Moreover, he never presented so charmingly simple a scheme as those of Russell and Hilbert or, for that matter, as (i) and (ii) in the *Remark* at the end of (b) above. Nevertheless, what he said is clear enough to be examined in the style of (a) and (b). Once again, results by Gödel and by others profiting from his work, correct wide-spread first impressions—both of intuitionistic doctrinaires and their critics—about the logical properties of Brouwer's scheme.

(i) Since Brouwer's doctrine stressed inadequacies of formal systems, the doctrinaires could be expected to err in the opposite direction—not seeing what formal systems could do. For example, p. 102 of Heyting (1956) says that no such system can embrace all valid methods of proof. This is true, and in fact made quite specific by Gödel's second theorem: no system 'embraces' methods which use its own validity. (Actually, the idea of 'embracing' the totality of proofs is mind-boggling even when specialized to proofs of the one 'theorem': $0 = 0$). But this leaves the question whether a formal system 'embraces' all its valid theorems, in other words, its completeness, naturally, for the intended intuitionistic interpretation. In the fifties and sixties such matters were taken up, and several *positive* results were obtained; as on p. 177, with special care to reduce the (intuitionistic) 'abstract nonsense': not, however, down to arithmetic, but to the subject of so-called lawless sequences; a compact exposition of that material is given in Troelstra (1977). Again, though Brouwer repeatedly objected to formal consistency as a sufficient criterion of soundness, he neither saw its significance (on p. 172) nor pin-pointed its limitations as exactly as Gödel did in (3).

(ii) On the other side of the fence, the critics, perhaps encouraged by Brouwer's dramatic 'contradictions' with ordinary logic—'contradictions' with a different interpretation of the logical operations!—objected to (his) supposedly paralysing restrictions on mathematical practice. Gödel was one of the first to expose in (11) the triviality of these particular, still widely believed objections. Since then we have learnt, slowly, to set out the bulk of mathematics quite elegantly by efficient use of intuitionistic methods—to be compared to p. 185 (and especially part IV) on the slow exploitation of specifically set-theoretic methods. Gödel was also one of the first to recognize genuine defects, as in (iii) below.

(iii) Specifically, in his early notes on (24) preserved at Princeton, he pin-pointed a principal defect of Brouwer's logic (which is also not yet widely known). In terms of this memoir: provided the comparison applies, the unwinding of derivations built up by intuitionistic formal rules is of about the same order of complexity as for the corresponding 'usual' systems; for example, in the case of theorems (on p. 175) which show that some diophantine equation has infinitely many solutions, the 'unwinding' consists in computing the nth solution of the equation (in some given ordering).

In short, by (i)–(iii), the original impressions (of all concerned) about the *logical* properties of Brouwer's scheme were about as wrong as those of Russell and Hilbert about their schemes. (Naturally, the corrections of the more famous errors have also become more famous.) But given (1) and (4), which show how easy it is to correct that kind of error, it was a foregone conclusion that the logical properties of Brouwer's scheme would be straightened out sooner or later.

In contrast, a philosophical assessment of Brouwer's scheme is more delicate. Since he proposed a reform (not analysis) of ordinary mathematics, experience of the latter is not enough. Instead it is necessary to apply a basic lesson from general scientific experience, on the *choice of data* needed to represent relevant features of the principal objects of study. As already mentioned, Brouwer's scheme was to study the mental activity of proofs. His polemics certainly show up the superficial character of known representations —not only in formal systems, but in ordinary texts with diagrams and all the rest. But he has no satisfactory answer to the question:

What better scheme is there than the known representations of proofs?

According to p. 480 of Brouwer (1977), he proposed to explore (his) deepest consciousness, presumably to arrive at ultimate reasons, as others have chased final causes. This sort of pretentiousness is of course suspect because it generally goes with simple-mindedness. But here it is possible to be more precise. The proposal errs by ignoring the basic lesson alluded to above, as follows. Reports from (his) deepest consciousness may be *quite enough for us to recognize the* (mental) *object involved, but useless for its theoretical study*— perhaps to be compared to reports on the shape and colour of minerals or plants in natural history. (Going into 'deepest' consciousness then corresponds to a meticulous description of nuances in shape, and shades of colour). True, such data are amply sufficient for recognizing the—mental or physical— object meant; but they are not adequate for a theory. Thus, in the case of minerals, rough knowledge of the molecular structure tells us much more about their physically significant properties than do very precise superficial data. In the case of proofs, a similar improvement would be expected from even a crude idea(lization) of the memory structures involved. As matters stand today, Brouwer's aim was shortsighted; for though the others neglected the potentially interesting topic of (actual) proofs altogether, what *he* had to add to the subject added too little, and stopped him from looking for results which are independent of our ignorance (about proofs).

Evidently, this ignorance concerns a theoretical analysis since, practically speaking, we know a great deal about proofs, using them constantly as tools. It is precisely in such circumstances that only a really substantial theoretical advance has a chance of competing with unanalysed practical knowledge or perceptive aperçus; as a corollary, simple-minded schemes are then intellectually especially unsatisfactory. But though unsatisfactory—and this is a general lesson of part II—a study of such schemes can be fruitful; the (mathematical) uses of incompleteness properties on pp. 174–176, or of elementary formulae and their model theory on pp. 180–183 developed from Gödel's studies of Russell's and Hilbert's simple-minded schemes. A price paid for the philosophical weakness of these schemes was the imagination needed to find the reinterpretations which lead to applications of Gödel's results.

From foundations to technology

The need for such reinterpretations is not particularly unusual in the sciences, especially if the work in question was originally used to refute a theory. But the frequency can be expected to be particularly high in the case of those foundational schemes or theories which, in line with Kant's view mentioned on p. 162, make 'possibilities in principle' their primary object. For given that preoccupation they will be satisfied with answers that *simulate* striking properties of the (mathematical) phenomena under study. This cannot be expected to tell us much about the phenomena themselves. But it will lead to technological progress, provided—as is natural—the answers are formulated in familiar, say mechanical terms. For then there is a chance that the effects which originally struck us can be achieved by those familiar means too, perhaps even more economically than by the things originally considered. Achieving a given effect—rather than understanding a given (natural) phenomenon—distinguishes technology (from science). Evidently, the word 'technology' is suggested by the relation (on p. 160) between Frege's rules for logic which had the ethereal purpose of analysing deduction, and the application of computers to non-numerical data. But the word also applies quite well to the mathematical uses of Gödel's results.

The parallel with technology applies also to the relative difficulty of discovering foundational results (which usually correspond to first impressions) and effective uses which, by above, require imagination. In contrast, by p. 167, in the case of what are normally called fundamental sciences, the applications look after themselves. Incidentally, the relative difficulty of those two kinds of discoveries is badly obscured by the slogan of 'pre-established harmony', so popular among logicians from Leibniz to Hilbert.

III. Good questions about sets

Older readers may still remember the embarrassing level of traditional 'debates' about sets and their properties. At one extreme there was the fixation on the paradoxes despite the fact that, for example, the most famous version, due to Russell, has a perfect parallel in arithmetic if one assumes that there is a greatest integer; cf. pp. 597–600 in volume 19 of the present series. Even more thoughtless were the sweeping generalities stirred up by those paradoxes. The most innocent *connerie* was the idea that, somehow, axiomatization would be a safeguard, as if there were no inconsistent formal systems (like Frege's). The most pretentious was the appeal to a general theory of knowledge, along the general lines familiar from part II, for example, p. 161, p. 162, and, especially, p. 187 on the matter of proofs. In the particular case of sets, the stress was on definitions (rather than proofs) from which sets were supposed to be 'constructed'—to be compared to the then-current business of sense data from which physical objects are 'constructed'; all this despite the fact that—corresponding to p. 186 on proofs of $0 = 0$—any *one* set has a truly mind-boggling 'totality' of definitions, and that sense data tend to fall to pieces on a closer look while (most) objects do not.

By the end of the twenties, at least some mathematicians had become sufficiently familiar with the vague mixture of things called 'sets' to decide which objects they wanted to talk about—instead of relying on accepted usage or on its (premature) codification in formal axioms. Some basic distinctions had been made, to be compared to distinctions between natural, rational, real or complex numbers: without such distinctions, properties of $+$ and $\times$, which are common to all of those numbers, are trivial for any one kind (and 'paradoxes' result if one puts together properties which are of interest for different kinds). In particular, in Zermelo (1930) there is a lucid description of what is nowadays called the *cumulative hierarchy of sets*, that is, sets generated by iterating the *power set operation* $\mathfrak{P}$:

$$\mathfrak{P}x \text{ is the collection of } \textit{all} \text{ subsets of } x;$$

more precisely, iterating $\mathfrak{P}$ transfinitely up to some stage α. Zermelo (1930) contains also the non-elementary axiomatization, mentioned on p. 164, for all segments of that hierarchy up to so-called *Grenzzahlen* α, also called 'inaccessible' cardinals. Thus, in contrast to Peano's or Dedekind's axioms, Zermelo's are not categorical, but determine a family. A trivial modification yields categorical axioms for such specific segments as the first (where $\alpha = \omega$) or the next (the first uncountable inaccessible). As an immediate pay-off the familiar axioms are seen to be valid by inspection; the more 'elementary' the axiom, the more segments α satisfy it. Secondly—a point which Gödel liked to stress, for example, in (20)—Frege's formulation

$$\exists x \forall y [y \in x \Leftrightarrow P(y)]$$

is *obviously* false for all α when $P(y)$ is $y = y$; for example, for the segment up to ω (of hereditarily finite sets), all the—infinitely many—objects y in that segment satisfy $P(y)$, but every set x in the segment is finite.

Gödel was the first to find really striking differences between the non-elementary axiomatizations in Zermelo (1930) and their formalization obtained by the passage on pp. 165–166. The latter is nothing else but the formal axiom schema familiar from current texts on logic or from introductions to mathematical texts. With one proviso, those differences are very well illustrated by the differences between the full Euclidean plane or, more simply, our geometric imagination, and its 'thinned' part constructible by use of ruler and compass, already mentioned on p. 165; both the full and the thin plane satisfy Euclid's elementary axioms, but only the former satisfies the non-elementary continuity axiom. By and large, the geometrically most obvious properties are easier to verify for the Euclidean plane, even when they hold for the thin part too, for example, the existence of a regular polygon of 17 sides. Also, Euclid's axioms do not decide every elementary formula since already the Greeks asked questions which have a different answer for the full plane and its thin part. The proviso is connected with the logical form of questions common in geometry and in set theory. The former are often purely existential, and so a solution for the thin plane is automatically a (refined) solution for the full plane, as in the case of a regular polygon of 17 sides. In set theory the logical form is more complicated, and so solutions to formally the same problem will be incomparable; in the case of the plane, the set $\{(x,0): (x^3-2)x = 0\}$ consists of one point in the thin part ($x = 0$), but of two points in the full plane. The much more heavily publicized comparison with the parallel axiom is wholly irrelevant to Gödel's contribution since it has nothing to do with the difference between elementary and non-elementary axioms (the parallel axiom is undecided by the remaining axioms of Euclid *together* with full continuity). For specialists: The business of the parallel axiom corresponds quite well to relatively easy analyses of the non-elementary axioms in Zermelo (1930), sufficiently illustrated by the easy incompleteness argument for set theory on p. 171.

Except for their last sections (pp. 201–204 and pp. 209–213), parts III and IV are meant for specialists, either mathematicians or historians of mathematics. The reason was given already on p. 150: modern set theory, in particular, the material of parts III and IV, has turned out to be of some interest when regarded as a specialized branch of mathematics; but its original appeal as a foundational system has turned out to be deceptive, as argued in Bourbaki (1948), with a few exceptions (in corners of advanced mathematics) discussed in part II and illustrated on p. 199 below.

Background: fat hierarchies of sets

The simplest particular cases of the hierarchies of sets described in Zermelo (1930) are C_ω and $C_{\omega+\omega}$ where

$$C_0 = \phi, \text{ the empty set}; \quad C_{\alpha+1} = \mathfrak{P}C_\alpha, \text{ and}$$

$$C_\omega = \bigcup_n C_n : n \in \omega; \quad C_{\omega+\omega} = \bigcup_n C_{\omega+n}.$$

C_ω or, more pedantically, the structure $(C_\omega, \in_\omega)$ satisfies the familiar axioms of set theory *without* the axiom of infinity, $C_{\omega+\omega}$ satisfies those of Zermelo (1908), from which the formal system called 'Zermelo's axioms' in the current literature, is derived. (Interested readers are advised to stop a moment, and actually verify a couple of axioms, remembering that C_ω is the collection of hereditarily finite sets by p. 189, and that, for $n \geqslant 1$, $C_{\omega+\omega}$ is the closure of $C_{\omega+n}$ under power set and subset formation.) More simply, also for α

beyond $\omega+\omega$,

$$C_\alpha = \bigcup_{\beta<\alpha} \mathfrak{P} C_\beta$$

without distinguishing between successor and limit ordinals α.

Zermelo (1930) introduced an additional parameter, an arbitrary collection U (for *Urelemente*) of distinct atoms without any elements, and a corresponding hierarchy $C_\alpha(U)$ with $C_0(U) = U$. This is useful, and used in practice; for example, in number theory the natural numbers are thought of as atoms. But the $C_\alpha(\emptyset)$, called C_α above, are good enough for the present purpose.

Background: non-elementary axiomatizations

In terms of p. 164 the 'principal feature' of the hierarchies in Zermelo (1930) is the (binary) membership relation $\in$. There are three non-elementary axioms. *Well-foundedness of* $\in$ for arbitrary predicates P:

$$\exists\, uP(u) \Rightarrow \exists\, x[P(x) \;\&\; (\forall\, y \in x) \neg P(y)].$$

(Its contrapositive is called: $\in$—induction.) This holds for all C_α since they are 'built up from below', and so, if $u \in C_\alpha$, there is a least $\beta \leqslant \alpha$, for which some $x \in C_\beta$ and $P(x)$.

Comprehension, again for arbitrary predicates P:

$$\forall\, x\, \exists\, y\, \forall\, z(z \in y \Leftrightarrow [z \in x \;\&\; P(z)]).$$

This too holds for all C_α. For if $x \in C_\alpha$, $x \subset C_\beta$ for some $\beta < \alpha$, and hence also $y \subset C_\beta$. But since $C_\alpha \supset \mathfrak{P} C_\beta$, $y \in C_\alpha$ too.

Replacement for arbitrary *functional* relations R (or, equivalently, predicates of ordered pairs): If the domain of R is restricted to a set $\in C_\alpha$, so is the range.

This holds for $\alpha = \omega$ and, if one wishes to be pedantic, also for $\alpha = 2$. It does not hold for $\alpha = \omega+\omega$, etc. A principal result of Zermelo (1930) is this:

Granted the rest of the axioms (which are elementary anyway), replacement holds only for the family C_α where α is *strongly inaccessible*, that is, in terms of Cantor's cardinal arithmetic: for $\beta, \gamma, \beta_\delta$ (all $<\alpha$)

$$\beta^\gamma < \alpha \quad \text{and} \quad \sum_{\delta<\gamma} \beta_\delta < \alpha.$$

Equivalently, card C_α = card α.

Moreover, and this is the non-elementary *axiomatization of the family of strongly inaccessible* C_α: if any structure (D, E) with domain D and the binary relation E on D, satisfies the axioms of Zermelo (1930), then (D, E) is isomorphic to some $(C_\alpha, \in)$ in that family.

Digression on the passage to formalizations of set theory (which, in contrast to those derived from Peano's or Dedekind's axioms, are better known than the non-elementary axiomatizations). As always, the (three)—infinite—schemata*

* Such schemata can be finitely generated by introducing a second type of variable, for predicates usually denoted by capitals X, a new binary relation symbol η ($x\ \eta\ X$: X applies to x), and axioms for the X corresponding to the (finitely many) syntactic rules for building up formulae. In the particular case of set theory, those X are called 'classes', and, as Gödel observed in (18), $\in$ can be conflated with η in a natural way.

of formal axioms arise from the (three) non-elementary axioms. But there are some curiosities. For example, non-elementary comprehension implies not only the schema for formulae F with a single free variable, z, but also

$$\forall u_1 \dots \forall u_n \forall x \exists y \forall z(z \in y \Leftrightarrow [z \in x \,\&\, F(z, u_1, \dots, u_n)]).$$

This schema, with 'parameters' u, is not formally derivable from the other one. (This has a parallel in the case of Dedekind's axioms, but not, as an easy exercise shows, in the case of Peano's.)

More interestingly, the formalizations are satisfied by C_α for suitable *accessible* α too. The proof is similar to the one of p. 176 for Loewenheim's theorem, but the result is incomparable: (i) the structures C_α involved are not countable, since already $C_{\omega+1}$ has the cardinal of the continuum, (ii) not only the (elementary) logical symbols retain their standard meaning; for example, $\mathfrak{P}$ does too. Typically, these simple facts were mentioned relatively late, in §5 of Montague and Vaught (1959), and are still not prominent in the literature. We now return to the principal topic.

Consequences of the non-elementary axioms. One of the easiest is the *axiom of choice*, for example, in the form: for any set x of disjoint (unordered) pairs $\{u, v\}$, there is a set y intersecting each pair in exactly one element. This is true for each α. For if $x \in C_\alpha$, all the $\{u, v\}$, and hence u and $v \in C_\beta$ for some $\beta < \alpha$. So $y \subset C_\beta$ and hence $y \in C_\alpha$. In other words, for the fat (or 'full') hierarchy, the axiom of choice is quite evident. The fact that this axiom was used tacitly till Zermelo (1904) should be compared to similar uses in geometry of axioms for *order* which were not listed by Euclid: such tacit uses do not cast doubt on the soundness of the axioms (for the intended meaning) though possibly on the competence of the axiomatizers.

Cantor's *continuum hypothesis* CH asserts, in effect, that any subset of $C_{\omega+1}$ (which is in 1–1 correspondence with the real numbers), is either in 1–1 correspondence with $C_{\omega+1}$ itself or with a member of $C_{\omega+1}$ (equivalently, countable or finite). CH therefore concerns only elements of $C_{\omega+4}$. Now, for the natural definition, say $\mathscr{C}_{\omega+4}$ of $C_{\omega+4}$, the non-elementary axioms are obviously categorical (even without replacement), that is, if the formula $\mathscr{C}_{\omega+4}$ (c) holds in any model (D, E) of the axioms for an object c in D, then (c, E_c) is isomorphic to $(C_{\omega+4}, \in_{\omega+4})$ where $E_c = E_\cap(c \times c)$. So

CH is decided by the non-elementary axioms,

just as, say, the prime pair conjecture is decided by Peano's axioms; only we don't know which way. (Being purely universal, Fermat's conjecture is a less suitable analogue because, by p. 172, if the conjecture were proved consistent with number theory, the same methods would prove the conjecture itself.) A moment's reflection on GCH, the so-called generalized continuum hypothesis, conveys a feeling for the content of non-elementary decidability. (GCH is obtained from CH when ω is replaced by arbitrary infinite α; the restriction to infinite α is needed since $\alpha = 0$ and $\alpha = 1$ are the only finite α for which C_α satisfies GCH.) Thus GCH is not *obviously* decided by the non-elementary axioms; it is not if, for example, GCH is true for all infinite α less than the first *un*countable strongly inaccessible cardinal, but not for all. Then GCH is true in the smallest segment C_α which satisfies those axioms, but not in all such segments.

Bibliographical remarks. Zermelo (1930) has made little impression. For one thing, though the non-elementary character of the axioms is prominent enough, there is no hint of such easy, but memorable consequences as those listed in the last paragraph. Perhaps more significantly, two basic points were slurred over. First of all, the reader was not prepared for the striking effect of adding the replacement axiom (on the ordinals α for which the axioms are satisfied by C_α). As early as 1931, Gödel alluded to some reservations, evidently on this score, in his correspondence with Zermelo who did not take them up, and Gödel repeated them throughout the thirties in his notes for lectures and courses. Those reservations go well with the fact that the axiom was a latecomer, having been introduced in the twenties by Fraenkel (in a restricted form, for definition by transfinite recursion) and in Skolem (1922) for formal reasons, but was first properly used only by von Neumann (1928). There it replaces the power set axiom for developing a good part of then-current set theory. But, above all, it is used for a *canonical well ordering*—by $\in$, of what are now the standard (set theoretic) 'numerals' for ordinal numbers—in which all well orderings can be embedded. As we see things now, von Neumann's work suggests a *thinning* of the hierarchy C_α. For any so-called regular cardinal ρ, for example, $\aleph_1$ (in Cantor's notation for the first uncountable), let

$$C_\alpha^\rho = \bigcup_{\beta<\alpha} \mathfrak{P}^\rho C_\beta^\rho,$$

where $\mathfrak{P}^\rho x$ is the set of all those subsets of x which have cardinal $<\rho$. Then, for $\alpha \geqslant \rho$, $C_\alpha^\rho = C_\rho^\rho$, and C_ρ^ρ satisfies the non-elementary axiom of replacement (but generally not the power set axiom). Familiarity with such C_ρ^ρ is a useful preliminary for really effective use of replacement. In this way one also comes to see the principal open problem presented by the hierarchy C_α: not the innocuous power set operation, but the *number of its iterations*; incidentally, this is quite parallel to the 'problem' presented by C_ω if one 'believes' only in finite sets x: for each such x, $\mathfrak{P}x$ is not problematic, but the notion of 'arbitrary' (finite) iteration is. In fact, the problem remains open; it is a principal subject of part IV.

But for the majority of potential readers of Zermelo (1930) at the time, the operation $\mathfrak{P}$ *was* problematic, the key words being 'vicious circle principle' or 'impredicativity'. Consequently, quantification over arbitrary predicates, so essential to non-elementary axioms, seemed to be an evasion of the problem on the part of Zermelo. Perhaps it was; in any case, all he did was to repeat his would-be telling terminology of *definite Eigenschaften* (predicates) which had been ineffective since Zermelo (1908) where it was first introduced. The irony is that he never seems to have spotted the crucial ambiguity between (i) definite in the sense of well-defined, perhaps even: decidable (as in (4):

entscheidungsdefinit) and (ii) having a definite extension (as is implicit in Cantor's explanation of sets as: varieties which can be grasped as a unity, varieties being defined by predicates). As to (ii), this is ensured by the restriction of the comprehension axiom to: $z \in x$; in Gödel's terms in (20), sets y are sets-of-something: of x's. But for the majority of readers hung up on the business of definitions, or on predicativity in the jargon of the day, only sense (i) was natural. Certainly, Zermelo himself had made great progress in the twenty five odd years before Zermelo (1930) appeared, but not enough to find the *mot juste* sufficient to remove that hang-up.

A much easier method towards this end would have been to look at the alternatives which predicativist critics had offered, specifically, what have come to be known as 'ramified hierarchies'. (Of course, they were originally intended as hierarchies of definitions, while nowadays we look at the sets so defined). The literature ranged from Poincaré's reflections on the matter to hoary details in Principia, and to particular examples in Weyl (1918). Zermelo himself may have had too little confidence in Poincaré's predicativist philosophy to look at those alternatives; for one thing, he had had bad experience of Poincaré's reflections on the mechanical theory of heat, which are criticized in Zermelo (1896). Incidentally, that paper has also made little impression on mathematicians, although it contains the first really elegant proof of the *recurrence theorem* for dynamical systems.

Background: a few steps towards a thin hierarchy

The thinning meant here is Gödel's formal variant of the predicativist ramifications in the last paragraph. Earlier attempts were much clumsier; as so often, but perhaps exceptionally so here (or, on p. 186, in Brouwer's logic) since, by p. 189, it goes against the grain to think of definitions instead of the objects defined (or of proofs instead of the theorems proved). Even so, in retrospect the complications appear pretty marginal, mainly because of pointless restrictions: to sets of integers (instead of abstract sets) or to so-called simple (instead of cumulative) types.

In any case, if the original intentions of ramified *definitions* were to be formalized after Zermelo (1930), the natural scheme was to use as definitions: formulae of the elementary language of set theory with (the usual meaning of) its one principal symbol $\in$, and to let quantifiers range over a given—in applications: 'previously' defined—set x. Then a formula

$$F(z, u_1, \ldots, u_n) \text{ defines the subset } \{z\colon F(z, u_1, \ldots, u_n)\} \text{ of } x$$

where the u mean elements of x. One writes

$$\mathfrak{P}^{-}x \text{ for the collection of all sets defined in this way.}$$

$$L_\alpha = \bigcup_{\beta<\alpha} \mathfrak{P}^{-}L_\beta \text{ defines the ramified hierarchy (by ordinal recursion).}$$

For $\alpha \leqslant \omega$, $L_\alpha = C_\alpha$. In sharp contrast: for $\alpha > \omega$, $L_{\alpha+1}$ has the same cardinal as L_α (and if the parameters u, in the terminology of p. 192 were omitted,

$\mathfrak{P}^{-}x$ would even be countable for any x). By the footnote on p. 191, it is clear how to replace $\mathfrak{P}^{-}$ by a finite number of operations with a more algebraic look, at the price of slowing up the growth of the hierarchy. This was done by Gödel in (18), using seven operations, but below, following his original exposition in (16), $\mathfrak{P}^{-}$ itself will be used. The hierarchy is *ramified* in the sense that (at each stage new definitions of any one set are introduced, but also) new subsets of L_α appear beyond $\alpha+1$, in contrast to C_α; for example, when $\alpha = \omega$, new sets of integers appear in $L_{\omega+n+1} - L_{\omega+n}$ for each $n < \omega$. An immediate consequence is that, in general, the comprehension schema is not satisfied by the sets $\in L_\alpha$, certainly not for $\omega < \alpha \leqslant \omega + \omega$.

By the turn of the century it was not unusual to begin analysis with the principle of the least upper bound: in logical terms, the comprehension schema was used at the very start. This seemed desperate if, like Russell in Principia, one wanted a (theory of some) ramified hierarchy to provide a 'universal' system for mathematics. He introduced the so-called *reducibility* axiom which says, in effect, in the case of subsets of L_ω, that no new ones appear in L_α for $\alpha > \omega + 1$. These tactics seemed equally desperate, especially coming from a philosopher who had compared the advantages of the axiomatic method to those of stealing over honest toil; more so than Zermelo's mildly evasive business of 'definite predicates' on p. 193.

Already back in 1931, Gödel concentrated on another weakness of the ramified hierarchy in Principia: it stopped at $\omega+\omega$, for no good reason. More specifically, footnote 48^a of (4) points out that the consistency of (the appropriate formal theory of) $L_{\omega+\omega}$ can be proved in $L_{\omega+\omega+1}$. It might be added that, by 1930, transfinite definitions, for example, of the real closure of an arbitrary ordered field, were common in mathematics—and usually there was a very good reason for stopping at some stage α: either when no new objects are introduced after α or when the objects accumulated by that stage satisfy some clearly stated (closure) condition.

Footnote 48^a was essentially negative, containing no hint, even remotely satisfactory for predicativist aims, where to stop (beyond $\omega+\omega$). But Gödel *discovered a problem* for which this was irrelevant.

Constructible sets: reculer pour mieux sauter

Gödel's first decision was not to stop the hierarchy L_α at all. More formally, with Zermelo (1930) as background he did not stop before κ, the first uncountable inaccessible ordinal defined on p. 191. As to the broad question, what one wants to know about L_κ, a good start is: Which of the usual axioms of set theory are satisfied by L_κ?

Several are verified immediately, for example, extensionality or pairing, but also the non-elementary axiom of well foundedness of $\in$, simply because $(L_\kappa, \in_\kappa)$ is the restriction of $(C_\kappa, \in_\kappa)$ to $L_\kappa \times L_\kappa$. There are pleasant surprises.

The first is closure under the power set operation:

$$\forall x \exists y \forall z (z \in y \Leftrightarrow z \subset x).$$

Let $x = L_\omega$.

Proof: There are $2^{\aleph_0}$ subsets of L_ω, so $\leqslant 2^{\aleph_0}$ such subsets in L_κ. Suppose they appear in $\{\alpha : L_{\alpha+1} - L_\alpha \neq \phi, \alpha < \kappa\}$, say Ω_κ. Since κ is regular and $> 2^{\aleph_0}$, Ω_κ has an upper bound $\bar{\alpha} < \kappa$. Then $y \in L_{\bar{\alpha}+1}$ since y is defined by the formula: $z \subset L_\omega$ (in $L_{\bar{\alpha}}$). Clearly, the argument applies not only to κ, but to any cardinal β which is regular and $> 2^{\aleph_0}$. N.B. As it stands, the argument leaves open whether, for such β, new subsets of L_ω appear in $L_\kappa - L_\beta$; cf. p. 199. The proof above is certainly not difficult, once one has understood that y is to be the set of subsets of L_ω that occur in L_κ or L_β (and not $\mathfrak{P}L_\omega$ itself). Of course, the proof by inspection that, for limit numbers α, C_α satisfies the power set axiom, is even simpler; cf. p. 190.

The replacement property, which implies comprehension (by taking characteristic functions), presents *the* new aspect. There is no evidence at all that the *non-elementary* version is satisfied in L_κ, but the *formal schema* is, by induction on the logical complexity of the (elementary) formulae defining the functional relation involved, and use of familiar closure properties of sets of ordinals; cf. the few lines on p. 456 of Barwise (1977) needed for a full proof.

The *axiom of choice* also holds, and again the proof is a little more involved than mere inspection (on p. 192). But it also gives more: a rather simple *explicit definition*, by recursion on $\alpha < \kappa$, of a well ordering of L_κ. Suppose the elements $\boldsymbol{u}$ of L_α are well ordered by $<_\alpha$, which induces a well ordering of finite sequences $\boldsymbol{u}$ of L_α (also written $<_\alpha$). As usual, elementary formulae F are numbered. For $x \in L_{\alpha+1} - L_\alpha$, let F_x be the first formula which defines x (in L_α):

$$x = \{z : F_x(z, \boldsymbol{u})\} \text{ for suitable } \boldsymbol{u} \text{ in } L_\alpha.$$

Let $\boldsymbol{u}_x$ be the first such $\boldsymbol{u}$. Then the elements of $L_{\alpha+1} - L_\alpha$ are ordered lexicographically according to $(F_x, \boldsymbol{u}_x)$. It turns out that the (natural) definition of $<_\kappa$ uses only quantification over elements of L_κ. Incidentally, similar care is needed in checking that the (natural) definition, say $\mathscr{L}$, of constructibility is invariant, that is, it defines L_κ both when its quantifiers range over L_κ and over C_κ, and that $\forall x \mathscr{L}(x)$ holds in L_κ; this is often written: $V = L$. Using the parallel on p. 190 we can fairly say that the definition of the well ordering $<_\kappa$ of L_κ is easier than Gauss's construction of a regular polygon with 17 sides.

Bibliographical remarks. (a) In keeping with his reservations, mentioned on p. 193, Gödel first tried to do without the replacement property, and to describe the constructible hierarchy L_α only for $\alpha <$ card $C_{\omega+\omega}$; in particular, without using von Neumann's canonical well ordering (p. 193). Instead, well orderings had to be defined (painfully) in $C_{\omega+\omega}$; further details are in the review of Kleene (1978) based on Gödel's conversations and on his notes for lectures at Notre Dame, now at Princeton. The simplification achieved by using von Neumann's notations, for which *higher types* are needed, provided the second memorable lesson, after footnote 48^a of (4), in Gödel's education on the virtues of transfinite iteration. (b) The titles of (16)–(18) about *consistency* properties of formal set theories do not even mention the notion of constructible set although he considered the use of that notion as his most significant contribution in the area. (This was not a late afterthought, for example, in his comments reported in Kleene (1978), but is already stressed in his notes for lectures in the thirties.) Actually, his choice of titles involved him in painful details: it had to be verified that the properties of C_κ used to establish facts about L_κ, were formally derivable from the axioms listed in the theories considered. Gödel's strategy of going into details avoided controversy at the time, as in (4), mentioned on p. 171. But it also left the (false) impression that the most urgent, if not the only fruitful problem was to complement his work by establishing the consistency of the *negations* of the propositions he had established for the constructible sets, in other words, to show their *formal independence*. This turned out to be of a different order of difficulty; cf. p. 200. (c) Gödel himself paid a price for his cautious tactics in (b); for example, in footnote 2 of (20), he recognized the absurdity of stressing the consistency of the axiom of choice since, by p. 192 above, it is as easily seen to be true for the hierarchy C_α as the other axioms. A more startling oversight (corrected in (26), p. 271 and p. 273) occurs in §3 of (20). There he assumed the formal independence of the continuum hypothesis, CH, and played with the idea that CH should be judged by its arithmetic 'fruits', that is, its arithmetic consequences. Certainly, by (3) on p. 172, mere consistency leaves open the possibility that CH has new, even false arithmetic consequences; but a glance at his own definition of L, in particular, at $C_\omega = L_\omega$, shows that CH, and even $V = L$ has none at all. Gödel's oversight is natural enough if consistency is regarded as an end in itself. The opposite view, already described in part II, was publicized for nearly a decade before a convincing though temporary use was made of it by Ax and Kochen (1965) in their proof of the decidability of the theory of p-adic fields (on p. 628, by means of the CH), alluded to on p. 176 above. For a realistic view of Gödel's heuristic ideas on p. 150, two more points are relevant. (d) He himself missed several interesting results by giving attention only to the theorem stated, not to the details of its proof. This concerns less formal errors, for example, at the end of (14), but certainly his review of Skolem (1934), mentioned already on p. 181. Returning to L: according to Gödel's notes, not he, but S. Ulam, steeped in the Polish tradition of descriptive set theory, noticed that the definition of the well-ordering (on p. 196) of subsets of ω was so simple that it supplied a non-measurable PCA set of real numbers (when all objects involved are taken from L). (e) Conversely as it were, Gödel tended to be uncritical of logically exciting claims, for example, regarding non-standard models (in 29), admittedly, written in the seventies; cf. p. 160. He attributed the scepticism of number theorists to broad prejudice, mysteriously connected with the recursive undecidability of Hilbert's tenth problem. In fact, later developments more than justify the suspicions of the majority.*

GCH: a variant of the axiom of reducibility

We now come to Gödel's principal discovery about the (thin) hierarchy L_α. To understand the issue, it is necessary to recall p. 192 on the continuum hypothesis (in its intended sense, that is, for the fat hierarchy C_α), and the objects involved: *elements* of $\mathfrak{P}\omega$, *subsets* X of $\mathfrak{P}\omega$, and *mappings* of X onto $\mathfrak{P}\omega$, resp. ω. These objects $\in C_{\omega+4}$. In contrast, by p. 196, when the CH is meant for some L_α, the corresponding objects—elements of $L_\alpha \cap \mathfrak{P}\omega$ etc.—can occur at levels far beyond $\omega+4$. Also, inasmuch as there are liable to be more of all these objects in L_α than L_β for $\beta<\alpha$ (and more in $C_{\omega+4}$ than in L_κ), the truth or falsity of the CH may well be sensitive to the length of the segments L_α (and to the kind of hierarchy) considered. A corresponding sensitivity is found in a more familiar formulation of the CH in terms of *cardinal arithmetic*: card $\mathfrak{P}\omega$ is the first cardinal $>\omega$. This is the least ordinal which is *not* in 1–1 correspondence with the set ω by a map in the stock of sets considered. The ordinal is denoted by ω_1 and by $\omega_1(L_\alpha)$, if all maps (of ω onto initial segments of the ordinals), resp. all such maps in L_α are considered. Evidently, $\omega_1 = \omega_1(C_\alpha)$ for all $\alpha > \omega_1$, but $\omega_1(L_\alpha)$ is liable to be $<\omega_1$, even though L_α and C_α have the same ordinals; cf. the example on p. 190, where the least integer n which satisfies $(\forall x > n)[(x^3-2)x \neq 0]$ for all x constructible by ruler and compass, is $<$ than the least integer with that property for all real numbers x. In fancy language, already used on p. 196: while the property of *being an ordinal* is invariant or absolute, the property (of ordinals) of *being a cardinal* $>\omega$ is not. This point is often overlooked in the (popular) 'debate' on the CH, where the *orderliness* of the ordinals (in C_κ or L_κ) is contrasted with the *mess* of $\mathfrak{P}\omega$ (in C_κ): a similar mess is involved in the collection of maps (in C_κ) of ω onto initial segments of the ordinals. It does not seem at all surprising that we have not (yet) decided whether the two 'messes' match. This fact is perfectly consistent with p. 192 on the non-elementary decidability of CH; after all, we don't even know how to match up the surely less 'messy' set of prime pairs with the ordinals $<\omega$, though the matter is certainly decided by Peano's non-elementary axioms, as mentioned on p. 192.

Returning to the GCH: by a quite simple use of the theorem of Loewenheim on p. 176 (cf. also p. 192) Gödel established the GCH for L_κ, and by careful formalization in (18), even a little more: for any model (D, E) of formal set theory, the 'inner model' defined by the condition $\mathscr{L}$ on p. 196, always satisfies the GCH. This result is a consequence of the following more delicate property of the constructible hierarchy (where 'cardinal' refers to constructible maps):

* For specialists. The best-known claim for non-standard models is in Robinson & Roquette (1975), where incidentally arbitrary ones are used, in no way tailored to their problem. But the only novelty is their use of a (known) generalization of Roth's theorem to arbitrary number fields on p. 158, which has nothing to do with non-standard models.

Reducibility for cardinals. Let α^+ be the first cardinal $>\alpha$. Then all subsets of L_α which occur in the hierarchy at all, $\in L_{\alpha+}$ (This is the sharpening of closure under the power set operation on p. 196).

A proof, in less than a page, can be found on pp. 465–466 of Barwise (1977). It is remarkably similar to some early expositions by Gödel, especially in his notes for general lectures, for example, to the American Mathematical Society in December 1938. As he mentioned in conversation, the idea of the sort of argument involved, occurred to him when he learnt Skolem's proof (on p. 176) as a student.

Few other memorable properties of L were discovered in the 30 years after (18) until the so-called Souslin hypothesis was shown to be false for L in Jensen (1972): there is a dense ordering in L without end points, which is complete (for cuts in L), and any set (in L) of non-overlapping intervals is countable in L; but the ordering is not order isomorphic to the real numbers (of L). Incidentally, Gödel's notes, for example, in *Arbeitsheft* XI, 47–54, contain material on Souslin's hypothesis and the related matter of Aronszajn trees, but, apparently, nothing in relation to L. As is clear from its title, Jensen (1972) concerns the details of L_α also for ordinals α which are *not* cardinals. Such concern would have appeared marginal (Kleinarbeit†) even only 10 years earlier, since the significance of such L_α was first established in the sixties in so-called generalized recursion theory; cf. ch. C.5 of Barwise (1977).

The discoveries in Jensen (1972) about L were used by Shelah to solve a purely *algebraic-sounding* problem about certain (abelian) groups satisfying a condition W (for 'Whitehead'). They have a free basis, say B_G, provided G is countable, the only case that arises in the (topological) context where the condition W was first introduced. The problem, explained in detail in the very readable exposition Eklof (1976), was whether

(*) all groups satisfying W have a free basis.

Though the word 'set' is not mentioned in (*), the stock of sets considered is clearly liable to be relevant (as in problems on cardinals on p. 198). The more sets, the more groups (satisfying W): the (universal) proposition (*) is more difficult to satisfy. But for any given group G: the more sets, the more subsets of G, and so the better the chance of there being a basis B_G (in the stock considered). Shelah established (*) for all constructible G and B_G, by essential use of Jensen (1972). The example illustrates two points of general interest.

First, in terms of current mathematical jargon: how easy is it to guess whether some phenomenon in group theory is set-theoretical (as one speaks of, say, gravitational phenomena)? That is, whether knowledge of set theory is relevant or even decisive. Flash judgment does not seem reliable; for example, in the superficially similar case of the—particular, but also uncountable—abelian group G_0 of *bounded sequences of integers with pointwise addition*, the

† Gödel's *Arbeitshefte* contain some attractive *Kleinarbeit* too, for example, XV, 11–13, or XVI, 38–40, on the axiom of extensionality; but this is superseded by the much more thorough analysis in Scott (1961).

constructible part of G_0 has a (constructible) free basis, simply by use of the continuum hypothesis, as observed in Specker (1950). But a quite different, much more informative proof in Nöbeling (1968) establishes the result, and more, for G_0 itself.

Secondly: how useful is it to eliminate (some particular) specifically set-theoretical restrictions? in the case above, of uncountable groups satisfying W: to constructible sets. As in similar cases, the answer will have to wait till group theorists are familiar enough with such groups or their constructible parts to have a chance of spotting whatever uses such objects may have; in other words, until St. Thomas's *adaequatio rei intellectu* applies (to those group theorists). Fundamentalists in set theory follow the simple rule that all 'hypotheses', not known to be satisfied by (suitable) segments of the *full* hierarchy, should be eliminated. But, at least in terms of the guiding parallel on p. 189, this simplicity is spurious. For example, in number theory, the full Euclidean plane or the field of all complex numbers is not always most relevant; in fancy language, it may be more rewarding to embed the numbers in some subfield with suitable properties (which we happen to know). So if set theory is ever to become significant for number theory—if ω or C_ω is to be embedded in some variant of the full hierarchy, with the axioms of formal set theory (or of a subsystem!) playing the role of the axioms for fields above—a prerequisite is that we should *know* something about that variant. As matters stand today, the constructible hierarchy has at least as good a chance of being useful as the full hierarchy from which it is extracted: after all, we know literally more about the L_α than the C_α. We now return to a topic, already broached on p. 197, which has led to a sophisticated arsenal of 'subhierarchies'.

Formal independence results. Part II provides general orientation on the topic, in particular (on p. 188), on the imagination needed to *discover* uses. As so often in such cases (cf. p. 165), as it were to protect the results in question, a body guard of exaggerations has developed; for example, the *connerie* that problems about a specific structure like $C_{\omega+\omega}$ are 'meaningless' when they do not happen to be decided by the sort of properties so far codified in axioms. (This *connerie* is involved in regarding the CH as 'settled' by its formal independence.) In view of the last paragraph, the body guard is now superfluous. Since Cohen (1963) a great number of subhierarchies have been introduced to establish the (formal) independence of most propositions mentioned in the last few pages, including Souslin's hypothesis and (*) on p. 199. (Readers may wish to verify that the former is decided by the non-elementary axioms, while (*) is not, at least: not obviously; cf. the discussion of CH and GCH on p. 192.) For the purposes of this memoir, there is no need to enter into details. But a general outline of this successful work is relevant in relation to both Gödel's heuristic views on p. 150, and to his own (early) results contained in his notes at Princeton.

For one thing he observed some simple *conditional* independence results, rediscovered—in one way or another—in the fifties; cf. Hajnal (1956), Lévy (1957), Shoenfield (1959). Specifically, suppose that $V = L$ (on p. 196) is independent of the remaining axioms (*without* the axiom of choice), and some set, say *a*, whose members are constructible, is not constructible. Then a new hierarchy, $L_\alpha[a]$, defined by putting $L_0[a] = \{a\}$ (in place of: $L_0 = \phi$), satisfies, for suitable α, the usual axioms *including* the axiom of choice, but not: $V = L$. Thus $V = L$ is also independent of the axiom of choice. As on p. 196, there are obvious possibilities of refinement. In view of the rediscoveries of such extensions it is fair to say that not the general principle, but the discovery of particular sets *a* needed for specific (absolute) independence results presented the principal difficulty.

Around March 1942, in *Arbeitshefte* XIV–XVI, Gödel made extensive notes for proving the formal independence of the axiom of choice (for sets of pairs of integers), and hence of $V = L$. The general idea goes back to (9), on so-called modal logic, and its topological models. With present experience it is not too difficult to complete the proof. But something essential—in Gödel's words (in conversation): a *method*—had been missing; cf. also his letter of 1st May 1968, where he corrected the description of Cohen (1963) given in the *Statement* in support of his own election to the Royal Society, as being a 'refinement' of his work. Gödel had just as much admiration for the later reformulation of Cohen (1963) in terms of so-called boolean-valued models (which are more obviously related to his ideas in 1942). Again, not the 'broad principles' involved in that later work, but their appropriate use, constituted the progress. For example, Church (1953) explicitly considered boolean-valued models for propositional logic, and, as explained in some detail in Scott's introduction to Bell (1977), the notion of forcing—though not the catchy name—had so to speak forced itself on several people who toyed with set-theoretic models of intuitionistic logic in the late fifties.

Some logical and foundational lessons

The development of set theory followed a pattern which seems to be often successful at the *beginning* of research. After some experimentation in the general area of experience under investigation, one selects objects in the area which seem to lend themselves to theory; this presupposes of course that some non-trivial facts are known about those objects. In the general area of sets (and definitions), the most successful selections were those of the full cumulative hierarchy generated by the power set operation, and—at the other extreme—of the ordinals, generated by iterating the operation: $x \mapsto x \cup \{x\}$; in the latter case, some functions have to be added since not much can be expressed by (elementary formulae built up from the order relation) $\in$ alone; cf. p. 166 concerning the successor or, for that matter, the order relation on the finite ordinals. The non-trivial facts known at the start are the familiar axioms. Later the classes of objects selected are restricted or enriched to realize structures with additional properties; in the case of sets, the full hierarchy is restricted, ordinals (or constructibles) are enriched; cf. $L_\alpha[a]$ above. Gödel took a lively interest when, in the fifties and sixties, the area of experience adumbrated by Brouwer in his writings on choice sequences, began to be studied according to the pattern above. A particular kind of such sequences, mentioned already on p. 186, turned out to have a simple theory: lawless sequences. The term is due to Gödel who objected to their original name: absolutely free, their principal property being that no restriction may be imposed on them beyond a finite number of values (a restriction on restrictions reminiscent of anti-trust laws which are intended to ensure a free market, but nevertheless are felt to be a shade short of absolute freedom). Compounds of lawless sequences, later called 'projections', have played much the same role as compounds of L and a to form $L[a]$ on p. 201. Parts of the story are to be found in Troelstra (1977). Gödel's interest is significant for a correct estimate of what has come to be known as his 'platonism'. He never questioned the possibility of a *part* of mathematics which is intended to be about our own 'constructions' or choices. Thus, once objects of this sort, in particular, lawless sequences had been described, the search for non-trivial facts about them was, for him, just as well-determined a project as his own search for axioms of set theory (and of course easier, since the pioneers in set theory had already discovered the more obvious interesting properties). But he did not regard that part as at all useful for mathematics itself, let alone as the whole of legitimate mathematics.

Gödel himself was less interested in the general pattern above than in the use of (his) more specific experience in set theory for other parts of logic. As early as 1936, commenting in his note book on a report by Bernays of Gentzen's lecture to a philosophical congress in Paris, he felt that the actual details of his proof of reducibility on p. 199 should be useful for a consistency proof of analysis; and nearly 40 years later, in his still unpublished additions to (24), he repeated this impression, though less explicitly. Evidently, the idea was that the 'collapse' should not stop at countable substructures, but should somehow go on to (suitable families of) finite orderings. Even if successful, this idea would no doubt have to be supplemented by the difficult step from foundations to technology in part II, presumably, by the discovery of a significant problem in set theory itself which is solved by use of that idea.

In accordance with his heuristic views, Gödel took little notice of what seems to be the principal *foundational* lesson to be learnt from the work described in part III, in particular, on formal independence results (pp. 200–201): the contrast between research at an early and at an advanced stage of a subject, well illustrated by the difference in meaning of 'axiom'. For example, Martin's axiom in ch. B.6 of Barwise (1977) and Jensen's $\diamondsuit$ in B.5 were discovered by inspecting (elaborate) proofs like many axioms of current mathematical practice, and are not meant to be seen by inspecting familiar objects like C_α on p. 192. However, in a brilliant programmatic lecture (25) in 1946, Gödel derived a foundational lesson, in the traditional sense of 'foundations', from his own work on *definability*. The lecture contains a second lesson of this sort, on (higher) infinite cardinals, which belongs to part IV. Incidentally, though the topics of these lessons are so to speak at opposite poles of the early 'debate' on sets, the inadequacies of formal systems in part II are central to both.

The most obvious inadequacy of any formal system for analysing even approximately the possibilities of definitions follows from *diagonalization*, as recognized already by Poincaré. In (25) Gödel pointed out how the use of ordinals in the constructible hierarchy prevents diagonalization, and thus provides a class of definitions with better closure properties. In conversation he mentioned that for a while he thought of it as exhausting *all* definitions, in fact, 'L' stood for 'law'; cf. also the footnote on p. 211 of (26): 'constructible' means definable. But he soon noticed that, for example, once one understands (not only L_α, but) the C_α, quantification over sets in C_α is also meaningful, and so he arrived at the notion of *ordinal definability* in (25). Without being dogmatic or even particularly specific about a more realistic candidate for an idea(lization) of the possibilities of humanly intelligible definitions, Gödel felt that L provided at least an idea for such an idealization. He also mentioned, in passing, that, so to speak at the lower end of the spectrum, the familiar class of computer programmes (for recursive functions) escaped diagonalization too —but for a different reason: only the larger category of programmes for *partial* functions has a (partial) recursive enumeration. Similar ideas on definitions were pursued in the fifties and early sixties (but without reference to (25) which appeared only later), arriving at *subclasses* of the class of recursive definitions because now definitions were required to be justified by appropriate proofs. This was achieved by restricting the ordinal logics in Turing (1939) by a so-called autonomy condition: before an ordinal was introduced, it had to be (formally) proved to be one. Here diagonalization was prevented even though everything in sight was recursively enumerated, since only proper segments of the system are justified according to the scheme adopted. The claim was that, in this way, one had a (simultaneous) characterization of certain *informal notions of proof and definition*. Not surprisingly, whatever its formal merits, the weaknesses of such a characterization are similar to those pointed out on pp. 185–187 in Brouwer's attempts to make proofs into a principal object of study. In fact, with all the additional detail in front of one, the criticism can go further. It concerns *growth* (and here it does not matter if one means, literally, growth of neurological connections or simply of understanding). Evidently, the introduction of hierarchies is reminiscent of growth, but the specific laws of growth implied by the particular hierarchies have no visible counterpart in experience. The weakness of those characterizations is not a matter of principle, for example, a conflict with empiricist methodology; (successful) rational mechanics and (unsuccessful) hydrodynamics of ideal fluids are not one bit less *a priori* than those hierarchies. The difference is that so-called reasonable assumptions about our reason are just much wider off the mark than our ideas about 'rational' behaviour of the planets. There is a charming description on p. v of the Preface to Dedekind (1888) of 'rational' ideas about *reading*: spelling out words is reading in slow motion, with the logical corollary that, for literal certainty, one *ought* to slow down (and accept the literal text including printer's errors, rather than an obviously intended meaning). Be that as it may, the silent majority of logicians has not taken up the part of (25) on definability and provability; certainly less than the other ideas in (25) on axioms of infinity which provide a beautiful illustration of Gödel's heuristic views on p. 150.

Fortunately for the purpose of *testing* those views concretely, in the last 25 years also other candidates for new axioms of set theory have come up. They concern infinite two-person games and winning strategies for one of the players. (Incidentally, Zermelo (1912) is one of the first papers on such 'determinate' games). The work on those other axioms has been as different, both in style and content, from Gödel's (25) as can be imagined. So, for contrast, it will be briefly described in part IV below.

IV. Axioms of infinity and determinacy

The axioms in question are intended to hold for suitable segments C_α of the fat cumulative hierarchy on p. 190. Evidently, this aim makes sense only for those who know a basic minimum about that hierarchy. Gödel tried to convey this minimum knowledge in three publications in the forties, in terms varying according to—what he considered to be—his audience: for philosophers in (19), sophisticated mathematicians in (25), school masters in (20). Instead of speaking of a 'minimum knowledge', he spoke of the 'reality' of the C_α (as will be described in more detail on pp. 209–210). In any case, here we accept the aim. But before one gets to specific problems there are at least two further broad preliminary questions:

What do we naively want to know about C_α? Gödel concentrated on Cantor's continuum problem, that is, whether the CH is true or false (which, by p. 192, concerns only $C_{\omega+4}$). Believing it to be false, he regarded a refutation of the GCH as an easier first step. He took the formal undecidability by means of current axioms for granted, and so new axioms had to be discovered. By p. 200 there is also the more delicate matter:

Is it at all rewarding to study the C_α further? Or are there variants of C_α which are, perhaps, less easy to describe, but more manageable (by use of what we already know of the C_α): should one *reculer pour mieux sauter*?

Gödel did not encourage the interest of the naive question to be questioned by the others. Instead he gave in (25) a beautifully plausible account of likely ways to find new axioms, in other words, of continuing the process which has led to the currently used axioms.

Enriching the language of formal set theories

The most obvious loss in the passage (on p. 165) from non-elementary axiomatizations to formalizations is that not arbitrary predicates, but only those defined by elementary formulae are used. So the most obvious step is to write down (new) axioms with the aid of some of those lost predicates. This is easy, contrary to a wide-spread misunderstanding (generated by the idea that there is something 'universal' about the usual systems of set theory). Specifically, in terms of numberings n of formulae N (on p. 170): though, by diagonalization, the predicate T of natural numbers, called 'truth definition' by Tarski,

$$T(n) \quad \text{if and only if } N$$

is not definable explicitly, T has an obvious implicit definition (by recursion on the number of logical operators in N), also known in the literature as 'Tarski's adequacy conditions'. The definition involves, as an auxiliary, an enumeration $S(n,x)$ of all monadic predicates defined by formulae N with one free variable, any finite sequence of sets being coded by one set. Now, given a formalization $\mathscr{F}$ of set theory, let $\mathscr{F}^+$ be obtained by adding the relation symbol S to the formalism and the implicit definition as a new axiom, the axiom schemata of $\mathscr{F}$ being extended to all formulae in the enlarged formalism. Then $\mathscr{F}^+$ is stronger than $\mathscr{F}$: for example, the consistency of $\mathscr{F}$ can be proved in $\mathscr{F}^+$. In short, one of the inadequacies of formal languages (on p. 203) is that *not all implicitly definable predicates are explicitly definable.*

Those with special interest in geometry would think of extending the language of set theory by symbols for geometric relations, and the axioms by propositions expressing geometric properties of those relations (with the proviso that all sets of real numbers or subsets of $C_{\omega+1}$ considered, represent geometrically meaningful figures).

Gödel had a different idea, going back to footnote 48^a of (4), and his other fruitful contacts with higher types mentioned in (a) on p. 196.

Gödel's programme: axioms of infinity

He pointed out in (25) that, for current formalizations $\mathscr{F}$ of set theory, the extension $\mathscr{F}^+$ above *can be replaced by an axiom $I_{\mathscr{F}}$ in the usual language of set theory*, where $I_{\mathscr{F}}$ is seen to be valid by the same considerations as $\mathscr{F}$ (as in the 'general argument' on p. 171), and all theorems of $\mathscr{F}^+$ in the usual language can be derived from $I_{\mathscr{F}}$ in $\mathscr{F}$. In fact, $I_{\mathscr{F}}$ is nothing else but the proposition used in footnote 48^a in (4) some 15 years earlier, the existence of the least C_α for which $\mathscr{F}$ is obviously valid. The argument is standard: though no enumeration of the monadic predicates definable in formal set theory can be explicitly defined, there is such an enumeration, say $S(n,x;y)$, for the predicates

$$N_y(x)\colon x \in y \text{ and the quantifiers in } N \text{ are restricted to range over } y.$$

Gödel concluded that, if such a modest use of higher types—actually, more than—replaced the most natural alternative (extension by enlarging the language of set theory), then a little more imagination would do miracles. Of course, nothing in mathematical practice gives even a hint of any more imaginative use, unless, following Gödel on p. 265 of (26), one regards analytic number theory as an instance of passing to type $\omega+1$ in order to solve problems about C_ω. But then there are plenty of open problems in mathematical practice: so why stick to its traditions?

To summarize his programme as it were, Gödel proposed to solve every problem by use of a suitable axiom of infinity. Naturally, he was not specific about the term, but the idea was clear enough: a new axiom of infinity is to be satisfied by some C_α, but only for $\alpha \gg \beta$ if C_β satisfies the already established axioms. (And: the bigger the α the better).

Certainly, the evidence for Gödel's programme was not worse than the evidence, mentioned on p. 166, which Hilbert had for his programme (of *Methodenreinheit*).

Later work. There can be no question of summarizing here the massive work done on Gödel's programme over the last 35 years. It is described at length in Kanamori and Magidor (1978), where complete references are given to the literature mentioned in the rest of part IV. But two directions of such work, firmly established by the end of the fifties, are worth noting specially. First, a new *style* of axiom was discovered, known to logicians under the name of (Lévy's) *reflection principle*, and to mathematicians as (Grothendieck's) *axiom of universes*; 'new', even though the early instances are formally derivable in current set theory $\mathscr{F}$ or from $I_{\mathscr{F}}$ on p. 205. The general idea is that all properties of C_α, stated in some given (elementary or non-elementary) language, should also be satisfied by some *element* x of C_α—either simultaneously, or by an x depending on the property considered. The idea corresponds clearly to the (intended) unending character of the hierarchy C_α. Already the simplest case of a non-elementary language, so-called Π_1^1—reflection, ensures that C_α is closed under all earlier schemes, for example, in Mahlo (1912), of building up the hierarchy 'from below'; cf. Bernays (1961). Evidently, except for properties stated in the most primitive language, reflection principles are *not* satisfied by C_ω. The second line of work goes in the opposite direction as it were: some simple set-theoretical property $P(\alpha)$ about α and its power set $\mathfrak{P}\alpha$, which holds for $\alpha = \omega$ (and, usually trivially, for $\alpha = 2$), is asserted or, at least studied, for certain $\alpha > \omega$. One typical example, going back to work in Poland in the thirties, is the existence of a two-valued measure on $\mathfrak{P}\alpha$ which is additive for subsets of $\mathfrak{P}\alpha$ of cardinal $< \alpha$. Another typical example is derived from the partition theorem in Ramsey (1928). By the early sixties, any $\alpha > \omega$ which has the properties P considered, was known to be larger than all familiar cardinals; for example, for any such α, there are α strongly inaccessible cardinals $< \alpha$. Far from being disturbing (for Gödel's programme), this knowledge is a prerequisite if $\exists\, \alpha P(\alpha)$ is to deserve the name *axiom of infinity* at all! After all, one wants here cardinals α which differ from ω 'as much as' ω differs from, say, 2. However, with remarkably few exceptions the particular properties P that people have stumbled on, are very poorly understood; mostly, one does not know if they are satisfied by any $\alpha > \omega$ at all, nor even whether superficially similar P are not satisfied. (A notable exception is the property called 'weak compactness' which follows from Π_1^1—reflection.) Incidentally, though (25) was published only in the sixties, work on those new axioms, especially of the second kind, could have profited from Gödel's presentation in 1946, since Tarski and Erdös were principal contributors; the former was present at the lecture, the latter was in contact with Gödel.

The first genuine implications of axioms of infinity for questions outside cardinal arithmetic were discovered in the early sixties: if ($\alpha > \omega$ and) α *is measurable then some subset of* $C_{\alpha+1}$, and even of C_ω is *not constructible* (as shown by Scott, respectively Rowbottom; in fact, the constructible subsets of C_ω are then countable). Whatever the defects of the particular property of measurability may be, one sees from the proofs how an arithmetic question might be settled by 'looking down on ω from above'. For people more familiar with another sense of 'L', for Lebesgue measure rather than for constructible sets, an implication discovered later by Solovay is more instructive: every PCA set of real numbers is L-measurable. The required coverings (by open sets) of such a set and of its complement are defined by use of the assumed measure on the first measurable cardinal $> \omega$.

However, Gödel's particular candidate, Cantor's continuum problem, is left, demonstrably, undecided by any (consistent) axioms of infinity so far proposed. Indeed, it is fair to say that the only memorable result on *cardinal exponentiation* discovered in the last 70 years (by Silver) can be proved by methods not too different from those current at the turn of the century. *If* α *is of cofinality* $> \omega$ *and* $< \alpha$, *and if, for all* $\beta < \alpha$: $2^{\omega_\beta} = \omega_{\beta+1}$, *then* $2^{\omega_\alpha} = \omega_{\alpha+1}$; a nice proof is on pp. 388–389 of Barwise (1977).

Before taking stock of work done on Gödel's programme and, particularly, of his heuristic views, the quite different direction of research mentioned on p. 204, has to be summarized.

Axioms of determinacy

In the fifties, when the theory of games was popular, certain so-called infinite games attracted special attention in Poland, where infinitistic generalizations had been popular for a quarter of a century. Suppose G is a set of sequences of natural numbers. Two players choose alternately natural numbers x_{2n+1} and x_{2n+2} for $n = 0, 1, \ldots$ If $\boldsymbol{x}$ stands for the sequence $x_1, x_2, \ldots$, the first player has *won* if $\boldsymbol{x} \in G$. A *winning strategy* for that player is, by definition, a function f_1 (with finite sequences as arguments and with numerical values) such that, for all choices x_{2n}: $n \geqslant 1$, the sequence

$$f_1(\langle\,\rangle),\ x_2, \ldots f_1(\langle x_2, x_4, \ldots, x_{2n}\rangle),\ x_{2n+2}, \ldots \in G,$$

where $\langle\,\rangle$ is the empty sequence. Similarly, a winning strategy for the other player is a function f_2 such that, for all x_{2n+1}: $n \geqslant 1$,

$$x_1, f_2(\langle x_1\rangle), \ldots, x_{2n+1}, f_2(\langle x_1, \ldots, x_{2n+1}\rangle),\ x_{2n+3}, \ldots \notin G.$$

Another way of writing these conditions is

$$\exists x_1 \forall x_2 \ldots \exists x_{2n+1} \forall x_{2n+2} \ldots (\boldsymbol{x} \in G),$$

$$\text{resp. } \forall x_1 \exists x_2 \ldots \forall x_{2n+1} \exists x_{2n+1} \ldots (\boldsymbol{x} \notin G),$$

for short, W_1 and W_2. A kind of dual to such 'games' without an end are 'games' without a beginning where the winning conditions, say W'_1 and W'_2, for the two players are

$$\ldots \forall x_{2n+2} \exists x_{2n+1} \ldots \forall x_2 \exists x_1 (\boldsymbol{x} \in G),$$

$$\text{resp. } \ldots \exists x_{2n+2} \forall x_{2n+1} \ldots \exists x_2 \forall x_1 (\boldsymbol{x} \notin G)$$

(and the distinguished player is now the one with the last move).

Steinhaus, later in collaboration with Mycielski, experimented with the unpromising proposition, called *axiom of determinacy*: $W_1 \vee W_2$ (formulated for all sets G of sequences: the x_n are arbitrary sets, not only natural numbers). The proposition is unpromising because the general idea behind it is nothing else but an extension of the well-known law for *negating finite sequences of quantifiers*. But a glance at its proof shows that it uses, in an obviously essential way, the fact that finite sequences have a beginning and an end; in particular, this is needed for the two basic properties of negation: the laws of contradiction and of the excluded middle. In fact,

$$\neg (W_1 \,\&\, W_2) \text{ holds, but not necessarily } W_1 \vee W_2.$$

Incidentally, by Galvin and Prikry (1976), neither $W'_1 \vee W'_2$ nor $\neg (W'_1 \,\&\, W'_2)$ need hold.

For about two decades many articles, of uneven quality, were published on determinacy, and certainly none that is remotely comparable in distinction to Gödel's (25). But finally, Martin (1975) proved that *all Borel games are determinate*, that is, $W_1 \vee W_2$ holds if G is a Borel set. This is not only of interest to the subject of infinite games, but easily the *most convincing contribution to Gödel's programme so far*. More specifically, the proof proceeds by transfinite recursion on the countable ordinals $\alpha (< \omega_1$, the first uncountable ordinal), where α is the number of applications of Borel operations (complementation, projection, countable unions) used to generate the set G_α. In a very transparent way, the determinacy of $G_{\alpha+1}$, a set of sequences $\boldsymbol{x}$: $x_n \in X$, is derived from the corresponding result for a suitable set G_α of sequences $\boldsymbol{x}$: $x_n \in \mathfrak{P}X$. In set-theoretic terminology, the proof uses C_{ω_1} to establish the determinacy of all Borel sets of sequences of natural numbers, a proposition about the quite familiar, low level $C_{\omega+2}$ of the cumulative hierarchy.

Even if not for Gödel, for mathematical practice the assumption of C_{ω_1} is an 'axiom of infinity'. Of course, its interest is established up to the hilt by the *particular* proof of Martin (1975)* since the transfinite iteration of the power set operation is seen to be useful by inspection. But more is true by work going back to H. Friedman: at least for the usual formulations of set theory, the determinacy of sets G_α cannot be formally derived at all without use of C_β where β is of the same order of magnitude as α. As on p. 176, this negative

* In addition, the proof has the virtue of making a very convincing use of (a simple instance of) the very attractive so-called priority argument, which was discovered in recursion theory nearly twenty years earlier, but applied only to the somewhat teratological subject of degrees of undecidability of recursively enumerable sets.

result can be given a positive twist: for any $\alpha < \omega_1$, *if a proposition of suitably simple* (syntactic) *structure is derived from assuming* C_α, *it can also be derived from Borel determinacy*. (A similar positive twist can be given to the results in the literature on the consistency of stronger axioms of infinity than C_{ω_1} relative to the assumption that larger classes of sets than Borel sets are determinate). In short, determinacy is an *alternative* to Gödel's programme.

Superficially, the relation between axioms of determinacy and the use of C_α for large α, is quite similar to that between the axiom of choice and the use of transfinite recursion in measure theory at the turn of the century. As long as only consequences of a simple syntactic structure are formulated, the details of the definitions in the proofs by transfinite recursion are lost in the statement of the theorems (just as the details of the winning strategy defined in Martin (1975) are lost if only simple consequences of Borel determinacy are considered). It remains to be seen whether somebody *discovers* problems, also in measure theory, for which those details are relevant.

Gödel's foundational views: balancing the account

As already mentioned on several occasions, in his publications Gödel used traditional terminology, for example, about *conflicting* views of 'realist' or 'idealist' philosophies. In conversation, at least with me, he was ready to treat them more like different *branches* of the subject, the former concentrating on the things considered, the latter on the processes of acquiring knowledge about these objects or about the processes. (He rejected only so-called positivist philosophy which—at least for logic—is distinctly negative, since it accepts, as arbitrary 'conventions' or as 'facts of our natural history', phenomena which the other branches see as problematic, or at least as capable of a rewarding analysis.) Naturally, for a given question, a 'conflict' remains: Which branch studies the aspects relevant to solving that question? with obvious parallels in mathematics and the natural sciences.

Gödel's successes: mixing the realist and idealist traditions. In mathematics, the idealist tradition is involved, in one form or another, in *constructivist* foundations which stress the use of definitions and proofs in the process of acquiring mathematical knowledge; cf. pp. 183–188 and 201–204 at the end of part II, resp. part III. In particular, Poincaré stressed definitions, Hilbert and Brouwer stressed proofs. (Incidentally, contrary to an almost universal misunderstanding, Hilbert's finitist proofs are much more restricted than intuitionistic ones.) As already mentioned Gödel solved problems which either had been formulated explicitly by the three famous constructivists above or, at least, are patently relevant to their foundational schemes. Gödel himself stressed, most clearly in his letters reprinted in Wang (1974), that his results are best understood in terms of notions from the realist tradition which were rejected or simply ignored in the constructivist schemes, such as: logical validity, arithmetic truth, various fat or thin hierarchies. Gödel's analysis was adopted in this memoir. But another reason for his success was, obviously, his familiarity with the subjects derived from the constructivist programmes: formal systems, intuitionistic logic, ramified hierarchies. By (yet another!) fortunate coincidence, the relative importance of the two elements in Gödel's successes can be illustrated by the case of Zermelo who had an equally staunch realist *Weltanschauung*: so much so that he simply refused to look at the tainted subjects! Thus, the stated reason for his outburst in Zermelo (1932) against Gödel's incompleteness results, was that Gödel considered formal systems at all, establishing their inadequacy instead of dismissing them as obviously inadequate. (An unstated reason could have been that detailed work on any subject is liable to create a vested interest in it, and a reluctance to look at alternatives.) At that time the still little-known Zermelo (1935) was in preparation, sketching what we should now call infinitary systems, with infinitely long formulae and infinite proof figures, intended to represent the meaning of propositions and the structure of mathematical thought adequately: in short, an alternative to formal systems. Whatever his conscious motives may have been, Zermelo's instincts to protect his alternative were more than justified: he did not get beyond his intentions! What he actually said about those infinitistic representations was not only, trivially, formulated in finite terms, but—and this is the critical defect—already fully expressed in *current* systems of set theory, as implied by Gödel's analysis on p. 205 (though Zermelo (1935) is not mentioned in (25) at all)*. Gödel's programme is nothing else but the first genuine proposal for *implementing* those realist intentions, by deriving from them new axioms—to be compared to deriving mathematical laws from a physical conception (or physical 'picture'), Maxwell's derivation of his equations from Faraday's picture being the standard example.

Gödel's programme involves quite different problems from those he had solved earlier: for one thing there was no idealist bias to be corrected by injecting suitable realist elements. He was treading new ground (though surely not 'rushing in', unlike the gamblers on infinite games on pp. 207–208).

Neglected problems: beyond naive idealism. Evidently—as already illustrated on p. 204 in the particular case of sets—recognizing some phenomena as a (legitimate) subject of research is necessary, but by no means sufficient for progress with understanding them. To put it paradoxically: once generalized doubts about them have been removed and some simple useful properties have been noted (here: doubts about infinite sets, and axioms codifying some obvious properties of the C_α), the *principal problem is selection*—selection of objects, among those recognized, which lend themselves to theory by something like available means, and selection of properties which have implications for such a theory. Such selection involves—besides the phenomena—just those processes which are the business of the idealist branch of philosophy, its sophisticated part as it were. When reasons for new axioms, that is, matters of

* It is also not very well known that so-called fully analysed infinite proof figures were considered in intuitionistic mathematics in the twenties; cf. pp. 393–395 in Brouwer (1977). Again, analysis in the sixties of the properties actually stated about those figures (cf. p. 202) shows that they do not go very far either.

evidence, are at issue (as in Gödel's programme), questions belonging to sophisticated idealism must be expected to become important or even dominant. Here it is to be emphasized that the bulk of the constructivist literature at best ignores sophisticated questions of selection, but more generally dismisses them (as a matter of some vague kind of 'convenience' or of sacrosanct 'personal taste'). Instead, that literature assumes miracles from the slogan about mathematics being 'our own construction'—as opposed to some 'external reality'. This assumption leads to what might be called *naive idealism* which is no less widespread than naive realism though the name is not. It is naive on at least two counts, besides those already listed in the remarks on proofs and definitions at the end of parts II and III. First, it forgets the problems arising in those parts of mathematics which are simply intended to be about our own constructions, for example, about computation rules; parts in which realist questions of a correct representation (definition) of a previously understood notion or of the mathematical structure of some external phenomenon do not arise at all. Nevertheless there remains the problem of recognizing whether a construction does or does not have some property: it is no simpler to decide if a diophantine equation has a solution when this problem is interpreted purely computationally than when one thinks of the natural numbers as properties of (extensions of) concepts. Secondly—and, if anything, this assumption is even more naive—the constructivist literature regards as particularly fundamental those parts of (mental) experience of which we are most acutely aware; for example, in the case of definitions and proofs, principal attention is given to the slow early stages in the learning process—not to our predispositions nor to our reasoning after that elementary knowledge has become part of our intellectual reflexes. Parallels in naive natural philosophy are obvious, for example, whenever the visible part is simply assumed to be decisive—not only, trivially, for our view of the world in the literal sense of the term, but also for scientific understanding. This naive part of the idealist tradition which has of course completely overshadowed its sophisticated part, is viewed with great scepticism by the silent majority (whose objections, as expected, are not very articulate).

Naturally, Gödel too had strong reservations about naive idealism, though he would not apply the term 'naive' to any part of traditional philosophy. But at least in mathematics he never seems to have faced squarely the problems raised in sophisticated idealism. This omission is only too obvious from his *obiter dicta* on evaluating the evidence for new axioms; a foretaste was given on p. 197 (c), the full flavour will be conveyed by the samples cited below. If I had not known him personally, I should have dismissed those *dicta* as another 'body guard', to protect his programme until the time was ripe for progress. As it is, the level of his discussions troubles me. It is not much above that of the 'debates' on the paradoxes mentioned on p. 188, more troubling still: it is utterly different from what I remember of our conversations (up to his illness at the end of the sixties), more than 20 years after (19) and (20) were written. Perhaps others, less involved than I am, will one day read the masses of his notes in Princeton, and fit that troublesome material into a more interesting picture of Gödel.

Bibliographical remarks. (a) Both in (19) and (20), Gödel tries to use the parallel between mathematical and physical objects to support his programme: without reference to specific examples, but simply as 'realities independent of ourselves'. This is doubly suspect. Trivially, again by p. 204, this makes his programme only a candidate for research, without the slightest hint of its chances of success. (Some physical phenomena are far from having a satisfactory theory.) Less trivially, there is no emphasis on the fact that mathematical notions enter into the description of the simplest physical phenomena on a par with other notions, not to speak of physical laws; for example, objects have chromatic and arithmetic properties (a yellow table with 4 legs): so one is left without an issue at all—quite apart from the fact that the methods needed for studying very different kinds of physical objects differ markedly among themselves. (b) The proposal in (20), already mentioned on p. 197, to judge new axioms by deciding (demonstrably) formally *undecided* propositions, conflicts with (a), vague as the latter may be. For in judging new scientific hypotheses, essential use is made of consequences which are tested *independently*, for example, observationally. A more convincing parallel involves (the use of new axioms for) *new proofs of old theorems*, as on p. 521 of (20) or p. 265 of (26), but implicitly taken back on p. 271 of (26) where such uses, so-called weak extensions, are described as sterile. For the record: today, some 30 years later, the general level of derivative literature on assessing new axioms is even more embarrassing, for example, in two of the otherwise most brilliant expositions in Barwise (1977). First, on p. 344, a cardinality principle is announced: 'Thus we see, the more problems a new axiom settles, the less reason we have for believing that the axiom is true'. Secondly, on pp. 813–814, in connection with instances of determinacy which are known *not* to hold for L_α, their validity for C_α is regarded as plausible because (i) Borel determinacy holds—but also for L_α if $\alpha > \omega_{\omega_1}$!—and (ii) because the consequences for descriptive set theory are coherent and pleasant—so to speak: fat is beautiful. (c) Gödel developed a remarkable obsession with mere cardinality, later escalated, by (b), on p. 344 of Barwise (1977). Thus (20) suggests in effect that the most fundamental problem about the continuum is to decide whether the continuum hypothesis CH is true or false (for C_α: $\alpha > \omega + 3$)—as if one did not want to know the geometry of the continuum just as much. Obviously, Gödel wanted to forestall the inevitable *conneries* which the expected proof of the formal independence of the CH was to produce. He was doubly unsuccessful. First, even some 30 years later, the peroration to the article on Hilbert's first problem (on the CH) in Browder (1976), questions whether the CH has a definite truth value for the intended meaning at all, because despite many attempts (by looking at many variants of ordinary set theory), the CH has not yet been decided: as if there were not infinitely many false starts, perhaps due to a systematic oversight, for any problem. But also—and this is not at all a matter of mere *conneries*—Gödel's exaggeration gives no hint of the kind of implications which make those independence results significant (apart from the technical uses in (c) on p. 197): obviously not by casting doubt on the precision of the continuum problem, but on its 'fundamental' character, its interest. Specifically, because of those results, the CH may be true even if some perfectly straightforward subset X of $C_{\omega+1}$ can be mapped onto C_ω or $C_{\omega+1}$ only by a very odd map, and the CH may be false, but only because some quite odd X cannot be mapped onto C_ω or $C_{\omega+1}$ (at all). In terms of pp. 176–177, explaining the unexpected usefulness of the general notion of logical validity, the CH *lacks stability* (with respect to perturbation of the domain of sets X involved). Without such stability, the problems of sophisticated idealism become decisive: which sets X and which maps do we want to know about? Examples were considered by Cantor and Brouwer (more than 70 years ago). The former showed that the CH does hold for *closed* sets X, the latter—back in 1908, reprinted on pp. 102–104 of Brouwer (1975)—considered geometrically meaningful, *topological* maps, when the CH is false even for quite simple X. (For spaces of choice sequences, with the usual topology, all maps are automatically continuous and then the CH is obviously false.) (d) Still in connection with geometrical properties, Gödel notes on p. 524 of (20) that not all sets of points are geometrically significant, but calls certain consequences of the CH 'paradoxical': at most a conflict with geometric impressions is involved, but no more than in the case of several well-known consequences of the axiom of choice—and, by (c) on p. 197, in footnote 2 of (20) he had recognized the validity of that axiom for the C_α. (Actually, even without the axiom of choice one gets geometrically meaningless results when $\forall x \exists ! y A(x,y)$ holds but the unique function f which satisfies: $\forall x A[x, f(x)]$ is highly discontinuous, for example, characteristic functions of sets defined by the comprehension principle applied to logically complicated predicates.)

Points (a)—(d) troubled me, as already mentioned on p. 158, before I met Gödel personally. The following gem occurred to him later, concerning weakly and strongly inaccessible cardinals. (The latter are defined on p. 191; the former need not be closed under exponentiation.) For *finite* cardinals, the two properties are not equivalent: 1 (in contrast to: 2) is only weakly, not strongly inaccessible since $0^0 = 1$. From this Gödel concluded that the GCH was implausible, since it implies that all weakly inaccessible *infinite* cardinals are also strongly inaccessible. Before publication on p. 268, § 3 of (26), which expands (20), Gödel told me his discovery. He added, with the expectant look he always had when he thought he was saying something particularly naughty, that I surely regarded it all as no more than a play on words (*Wortspiel*). I still remember my pleasure (and his) when a totally ambiguous comment occurred to me. No similar banter is to be found in (26) itself.

V. Philosophy: speculations and reflection

The three published samples of Gödel's speculations on spectacular topics are about time travel, minds and machines, and the origin of life on earth.

The first was developed by him in considerable detail in (21)–(23), the second was a principal topic of our conversations in the sixties, and the third happens to bring out particularly clearly, by contrast with Crick and Orgel (1973), the single most distinctive point in Gödel's heuristic views, his preference for using general qualitative rather than specific 'empirical' data—in accordance with the ideals of traditional philosophy.

On present evidence Gödel's contributions to the topics above are not conclusive, and certainly not comparable to his successes in parts II and III. As can be gathered from p. 151 it is beyond the scope of this memoir to go into the many differences involved, in the nature of the topics, the stages of development, the attention Gödel gave to them, and so forth. Nevertheless, quite apart from a pleasing freshness and wit, the special twists he gave to his speculations, serve remarkably well the purpose mentioned already in part II: they provide striking illustrations, in the quite different area of natural science, for the lessons learnt on p. 174 from reflecting on his incompleteness theorems in the foundations of mathematics; cf. also p. 161.

General Theory of Relativity

Gödel's early interest in the subject and his close contacts with Einstein were described in part I. The following account of Gödel's publications in the area is due to Professor R. Penrose, F.R.S.

Gödel's writings on the General Theory of Relativity were not extensive, consisting of three quite short articles (21)–(23), but they were highly original and, in the long run, quite influential. In these articles he described a family of cosmological solutions of Einstein's equations that possessed a number of novel features. Most striking among these was the presence of closed timelike curves in his original non-expanding model. Thus, in this model, it would be possible in principle for an observer to travel into his own past. While for the majority of physicists, this feature might be regarded as a sufficient criterion to rule the model out as 'physically unrealistic', Gödel appears to have taken a contrary view. Indeed, in (22) he computed, in a footnote, the amount of fuel required for the execution of such a journey and, finding this to be absurdly large, concluded that his model could not be ruled out as contradicting experience. (He did not, however, consider the vastly 'cheaper' but equally paradoxical possibility of an observer merely sending a signal into his own past.) In the modern theory of global general relativity, for example, in Hawking and Ellis (1973), it has been found necessary to examine the various types of 'pathology' that can exist in space-time models even when these features might be regarded as sufficient to rule out the models as 'physically unreasonable'. Thus, this original Gödel model has provided an interesting and significant example of a space-time precisely because of this 'unphysical' feature. Indeed, the Gödel model was the first simply-connected such example, the closed timelike curves being therefore 'essential' in the sense that they cannot be removed by passing to a covering space. The model is interesting also for a more philosophical reason. It shows that a concept of time that is globally quite different from that seemingly implied by our normal experiences cannot be ruled out merely on the basis of the known local physical laws, once some of the ideas of general relativity are taken into account.

A second feature possessed by Gödel's models is that the matter in them rotates relative to the local inertial frames. Thus, the models show that at least one form of 'Mach's principle' is not a consequence of general relativity (contrary to what Einstein had originally hoped). Gödel also proposed expanding rotating models (without closed timelike curves) which could be serious candidates for the actual large-scale structure of the universe. Gödel's demonstration of the existence of apparently realistic models in which there is a relative rotation between inertial frames and distant matter led to the speculation among cosmologists that such a feature might also be detectable in the actual universe. However, very low observational limits can now be placed on this hypothetical rotation (apparently $< 10^{-16}\,s^{-1}$).

A third feature of the models is that they possess spatial homogeneity but not isotropy. Gödel appears to have been the first to study such models and to introduce the appropriate non-holonomic frame techniques (largely unfamiliar to relativists of the time) for their detailed analysis. However, much of his work in this area remained unpublished and had to be rediscovered by others. The study of spatially homogeneous models has become an important part of theoretical cosmology in more recent years; cf. Heckmann and Schücking (1962). Gödel was concerned only with space-times filled with incoherent matter. A corresponding analysis of empty space-times was made by Taub (1951) shortly afterwards.

Thus, by Professor Penrose's account, the direct physical interest of Gödel's papers (21)–(23) is limited—in accordance with Einstein's comment on (22) in the same volume. (Gödel's papers appeared in the middle of a long period during which the general theory made little progress.) But by that same account, as Professor Penrose emphasizes from his own research experience (according to a letter of 11 September 1979), Gödel's work served as a cross check on mathematical conjectures and proofs in the modern global theory of relativity. This is the first of the striking parallels to his incompleteness theorems promised on p. 214, in particular, to the use, on p. 175, of his second incompleteness theorem as a cross check on proposed consistency proofs (though, as mentioned there, the direct foundational interest of that theorem is quite limited).

Bibliographical remark. In a long typed essay at Princeton, Gödel expanded (22) in the style of academic philosophy, using (21) to interpret Kant's ideas on time. Perhaps closer study will show what more is gained from this pedantic attention to Kant's elaborations than from the simple idea of ghosts which, by p. 155, had long been in Gödel's thoughts (while, as he says in the essay, he never had much sympathy for Kant's general philosophy). In any case, though the typescript dates from the fifties, Gödel did not publish it. But he put it among the items to be published after his death, on lists he made in the seventies, especially on days when he thought he was going to die.

Non-mechanical laws of nature

Throughout his life Gödel looked for good reasons which would justify the most spectacular conclusion that has been drawn from his first incompleteness theorem: minds are not (Turing) machines. In other words, going back to p. 162, the laws of thought are not mechanical, that is, cannot be programmed even on an idealized computer. ('Mechanical' should not be confused with 'mechanistic' in the sense of deterministic; the usual probabilistic laws are mechanically computable.) The popular reasons are quite inconclusive. Certainly by—Matyasevic's improvement of—the incompleteness theorem, those minds which can settle all diophantine problems are not machines. But we have neither found any evidence of such minds nor the slightest hint of any computer programmes which simulate, even in outline, actual proof search—not even for solving problems which do have a mechanical decision procedure, for example, propositional algebra; cf. the use of impure methods stressed in part II.

In conversation Gödel brought up one of his favourite twists: *Either mind is not mechanical or mathematics, in fact, arithmetic, is not our own construction.* The tacit assumption here, one of those reasonable assumptions about our reason on p. 203, is that we can decide all properties of our own constructions. Gödel remained unsympathetic to the admittedly tasteless comparison with our physiological productions which can have painfully unexpected properties. His reaction was quite different to another objection I made in the early sixties, expressed by the question:

Is mechanics mechanical? or, more formally, are the laws of current physics mechanical in the sense that—according to current theory—every analogue computer can be simulated by a digital computer (with the same probability of error)? It is certainly not evident that celestial mechanics is mechanical, in particular, that collision properties of n-body configurations are mechanically decidable—even in finite time intervals. (For specialists: a little care is needed in the formulation, since the data should not be points but neighbourhoods in phase space.) Other candidates for non-mechanical laws came from statistical mechanics of co-operative phenomena such as boiling.

The question above expresses an objection; for if some laws of ordinary matter are non-mechanical, then the notion of machine is not adequate 'in principle' to separate mind and matter. Gödel was at first tempted to dismiss the question, by the familiar *petitio principii* of supposing that only mechanical laws are precise (for a non-mechanical mind?), but he stopped himself in the middle of the sentence, I believe, the only time in all our conversations. Afterwards, he took an active interest in the search for non-mechanical laws both in physics and in the part of logic which studies specifically mental constructions. (In the latter the *petitio principii* to be avoided is the requirement that those constructions must be represented by a mechanical procedure.)

The question above is not yet settled. Here the parallel promised on p. 214 to the lessons learnt in part II from the incompleteness theorem, concerns the evaluation of the 'empirical evidence' provided by existing solutions in mechanics. Certainly, the bulk are mechanical, just as the bulk of ordinary mathematics is easily formalized in Principia (despite its incompleteness). If there are mechanically undecidable problems in some parts of mechanics, they may have been discounted by now—replaced by more tractable questions, just as number-theoretic practice concentrated on more rewarding problems about diophantine equations long before the negative solution of Hilbert's tenth problem so to speak ratified the practice. Corresponding to the positive aspects emphasized in the article on the tenth problem in Browder (1976), in mechanics one would hope to have a new kind of analogue computer. Last but not least, the mere existence of some non-mechanical laws of nature, just as the mere existence of some formally undecided problems in mathematics, does not settle their significance in the sense of their *frequency* in different branches of science. Incompleteness phenomena are, on present evidence, much more significant for set theory than for arithmetic (tacitly, for the questions that strike us as interesting). It certainly cannot be excluded that, similarly, the phenomena of consciousness (that strike us) follow non-mechanical laws as a rule in contrast to the phenomena of ordinary physics, at least those on which physical theory concentrates. (This discussion is sharpened by the *Note* on p. 224).

Bibliographical remarks. (a) Komar (1964) which was overlooked in our conversations, points out that non-mechanical laws arise in those parts of physics where theoretically admissible states σ are represented by so-called primitive recursive sequences s of natural numbers (given by a description of the experimental set up), and some observable relation R between σ_1 and σ_2 corresponds to: s_1 and s_2 differ infinitely often. But Komar (1964) is inconclusive since the theories considered are not shown to permit arbitrary primitive recursive s (or enough for R to be non-mechanical). (b) More recently, on p. 59 of Browder (1976), Arnold mentioned other candidates for non-mechanical laws in statistical mechanics involving vector fields given by polynomials with rational coefficients, though like the n-body problems on p. 216, Arnold's seem to need neighbourhoods instead of discrete coefficients. (Mechanical undecidability is usually easier to establish in the latter case; occasionally there are neat theoretical reasons, even in classical mechanics, for discrete data, for example, Newton's for the exponent -2 in his law of gravitation.) (c) According to p. 326 of Wang (1974), in the seventies Gödel seems to have gone back to his original twist; but his arguments for supposing that physical laws must be mechanical have a, for him, strangely positivistic flavour.

Chemical evolution of living organisms on earth

Though Gödel's published comment on this topic, on p. 326 of Wang (1974), is very brief, it is worth mentioning since it fits in with his general views expressed in many conversations. His particular conjecture was that the probability of a living organism developing in geological time as a result of random chemical operations was vanishingly small. The initial distribution of matter is assumed to be random, and nothing is said about the significant features of either the chemical reactions or of living organisms which would be used in the calculation. Evidently, he hoped that only 'basic' knowledge, for example, schoolboy chemistry would be needed since—as he often said—the use of specific detail could not be convincing in Big Questions.

As it happens, Crick and Orgel (1973) present a perfect example of so to speak the opposite heuristic view. Briefly, they use two 'very' specific details about molybdenum: it is rare in our part of the universe, and it occurs in living organisms. So it seems a foregone conclusion that, with these two additional hypotheses, Gödel's conjecture holds, and should be easy to prove formally. But also, while Gödel's conjecture, as formulated, gives no hint at all of any positive theory about the origin of life on earth, Crick and Orgel (1973) inevitably looked for a source in regions of the universe where molybdenum is more plentiful and where chemical evolution could have succeeded, free from terrestrial constraints. (After that they followed *Genesis*: like Jehovah those extra-terrestrial beings set about populating the Earth, in their fashion.)

Their speculations have not settled the origin of life on earth. But their use of 'specific detail' about molybdenum provides a neat parallel to one of the lessons on incompleteness on p. 174. The aim of (4) and its improvements in the thirties was independence of subject matter, for as broad a class of formal systems as possible. The so far most successful applications of incompleteness involve 'specific' properties, such as the size of ordinals in set theory or the rate of growth of number-theoretic functions, and above all the informal notion of arithmetic truth, at least, for diophantine problems. *Remark* (on another spectacular topic). The literature on hidden variables in the quantum theory contains several impossibility proofs which are also *incomparable*, without stressing this fact. By and large philosophers and logicians try to avoid specific details of the theory, and prefer to use (familiar) properties studied in logic or probability theory.

General interests—and a contrast

Judged by the amount of space in Gödel's note books dealing with general philosophy and theology, including demonology, these subjects occupied a great deal of his attention ever since his student days. They were rarely touched in our conversations since there was not enough common interest. However, during the 15 years or so when I saw a great deal of him, he would occasionally quote passages from his preferred reading at the time—Kant, then the slow-paced Husserl, then so to speak the opposite extremes, Fichte and Schelling. The quotations were not at all well-known, and, at least for me, very perceptive. Given Gödel's methodical habits mentioned on p. 151, he may well have kept a record of these and similar passages. The publication of such an anthology is likely to produce a minor revolution in philosophy: if we came to associate Hegel or Husserl with a dozen crisp and memorable ideas, we could cherish them as much as—what we know of—Heraclitus.

Gödel's conversations on the general topics above, at least until his illness, had the light touch and exquisite discretion noted already elsewhere in this memoir, in contrast to the impression left by some of his more popular writings (p. 158). In this respect there is a striking parallel to the difference between the letters of Archimedes and his public image which has him look for a fulcrum in outer space to move the earth—as might be expected from some kind of misfit, ill at ease on this planet. Incidentally, if Gödel's work is to be compared to that of one of the ancients, Archimedes is a better choice than Aristotle (who invented logic, but proved little about it). Archimedes did not invent mechanics, as Gödel did not invent logic. But both of them changed their subjects profoundly by work with an almost unsurpassable ratio of interest of the results to effort, as seen in part II above or in the laws of the lever.

VI. Foundations and the common understanding

As promised at the outset this memoir has described Gödel's contribution to our present understanding of formal and (non-elementary) axiomatic notions; in particular, logical validity and arithmetic truth (or, equivalently, in fancy language: consequence from Peano's non-elementary axioms) in part II, and truth for segments of the cumulative hierarchy of sets in part III. Those notions had been neglected by most logicians for a quarter of a century before Gödel's famous results put them back into circulation, by establishing memorable relations between them and formal notions. Apart from any heuristic value which non-elementary notions may have had for Gödel's own discoveries, they continue to be essential—even for an effective use of elementary logic itself (pp. 180–182). The interest of Gödel's contribution is in no way diminished by the checkered development of the subject since then: by the efforts needed to discover rewarding applications, the limitations of Gödel's general programme to apply traditional philosophical notions more broadly (in parts IV and V), and not even by the endless refinements of his work which have gone far beyond the point of diminishing returns.

Also as promised at the outset, the refutations of the best known foundational schemes of this century by use of Gödel's results were compared with an alternative critique, by inspecting (later) mathematical experience. The comparison is familiar from so-called purely mathematical and experimental refutations of theories in the natural sciences; the former involve conflicts with very familiar facts, so to speak with the bare minimum expected of the theories, a standard example being Galileo's refutation of his (first) proposal that the velocity of a freely falling body is proportional to the distance covered (and so a body at rest would never start to fall at all, contrary to very familiar experience). So-called experimental tests of theories, even of those presented as abstractly as Newton's or Einstein's theories of gravitation, generally require a high level of unfamiliar extensions of ordinary experience (and if positive the tests supersede mathematical refutations of competing theories). In the case of foundational schemes, Gödel's results provide mathematical refutations, while details of mathematical experience are used to pin-point less obvious defects of the schemes.

As a corollary, already asserted on p. 149: since the silent majority has the experience needed for the alternative critique, Gödel's results could not be expected to affect significantly the conception, let alone the practice of that majority. Sooner or later, it would discount foundational ideals, either ignoring them altogether or putting them in their place by reference to experience. For the same reason, the majority has no need for a pedantic formulation of the ideals themselves.

The presentation above leaves out of account a side of Gödel's contribution to foundations, in fact, of the subject of foundations itself, which is literally of the highest interest, for two principal reasons.

First of all, the scientific experience needed for the alternative critique has not been, and cannot be absorbed at all widely. Some philosophers, including Wittgenstein, have attempted something like that alternative, using only examples from quite elementary mathematics. This was unconvincing. It left a nagging doubt whether the examples were representative. More formally, as shown in logic (cf. p. 186), large parts of mathematics can be set out in accordance with conflicting foundational schemes, usually quite elegantly after some practice with the style involved. So, quite objectively, elementary experience is not enough for a decision between such schemes, let alone against (all of) them.

Secondly, foundational questions occur to us when we know little; as little as a school boy in his teens or even as little as the Greeks 2300 years ago. At this stage of experience the familiar foundational answers or schemes have a great attraction—in keeping with the objective fact, mentioned in the last paragraph, that limited experience does not decide against them. In such circumstances it is rare indeed that anything significant, let alone conclusive can be done using only a mild extension of familiar experience. Gödel's results are significant, and, especially in part II, use no more additional knowledge than the elementary parts of logic available in the twenties, practically no more than needed to state the foundational schemes in mathematical terms. (His notes to later reprints or translations give, with loving care, the most economical formulations.)

In short, foundational interests exist; Gödel's results which are relevant here, are significant and can be *fully* understood with a minimum of background. So they have an exceptional value, measured by the simplest criteria of all: the size and probable duration of the market for his contributions (or, equivalently, measured by the particular kind of fame which Schopenhauer analysed in ch. 4 of his *Aphorismen der Lebensweisheit*). This value of Gödel's results is of course quite separate from their value—for foundations or for science—at a more developed stage, perhaps to be compared to those elements which are valuable or even vital at an early stage of evolution, and less rewarding or even superfluous later, with one difference. In the case of the evolution of knowledge, each generation starts off at an early stage. Besides, for all of us there are areas about which we know little and have first impressions analogous to foundational schemes. Gödel's results on the famous schemes of Russell and Hilbert, at least when looked at in the way just described, give one confidence in the possibility of analysing other schemes of this sort instead of simply suppressing them (and the analyses of other foundational schemes mentioned at the end of parts II and III, support this confidence).

Sub specie aeternitatis, or at least as long as our age of intellectual affluence lasts, the value to the common understanding described above, may well be seen as the most extraordinary part of Gödel's contributions—memorable as their scientific uses, reported in parts II and III, undoubtedly are.

The photograph reproduced as a frontispiece was taken by A. Eisenstaedt in 1966; the snapshot in the text was taken at the time of Gödel's most famous discovery.

References

Ackermann, W. 1924 Begründung des 'tertium non datur' mittels der Hilbertschen Theorie der Widerspruchsfreiheit. *Math. Annln* **93**, 1–36.

Ax, J. & Kochen, S. 1965 Diophantine problems on local fields. *Am. J. Math.* **87**, 605–630.

Barwise, J. (ed.) 1977 *Handbook of mathematical logic*. Amsterdam: North-Holland Publ. Co.

Bell, J. S. 1977 *Boolean-valued models and independence proofs in set theory*. Oxford: Clarendon Press.

Bernays, P. 1961 Die hohen Unendlichkeiten und die Axiomatik der Mengenlehre. pp. 11–20 of *Infinitistic methods*. Oxford: Pergamon Press.

Boole, G. 1854 *An investigation of the laws of thought, on which are founded the mathematical theories of logic and probability*. London: Walton & Maberley.

Bourbaki, N. 1948 L'architecture des mathématiques. pp. 35–47 of *Les grands courants de la pensée mathématique*. (ed. F. Le Lionnais) Paris: Cahiers du Sud.

Brouwer, L. E. J. 1974 *Collected works*, vol. I. (ed. A. Heyting) Amsterdam: North-Holland Publ. Co.

Browder, F. E. (ed.) 1976 *Proc. Symposia pure Math.* **28**. Providence, R.I.

Chevalley, C. 1936 Démonstration d'une hypothèse de M. Artin. *Abh. math. Semin. Univ. Hamburg* **11**, 73–78.

Church, A. 1953 Non-normal truth tables for the propositional calculus. *Boln Soc. mat. mex.* **10**, 41–52.

Cohen, P. J. 1963 The independence of the continuum hypothesis. *Proc. natn Acad. Sci. U.S.A.* **50**, 1143–1148.

Crick, F. H. C. & Orgel, L. E. 1973 Directed panspermia. *Icarus* **19**, 341–346.

Dedekind, R. 1888 *Was sind und was sollen die Zahlen*? Braunschweig: Vieweg.

Dreben, B., Andrews, P., & Aanderaa, S. 1963 False lemmas in Herbrand. *Bull. Am. math. Soc.* **69**, 699–706.

Eklof, P. 1976 Whitehead's problem is undecidable. *Am. math. Mon.* **83**, 775–788.

Galvin, F. & Prikry, K. 1976 Infinitary Jonsson algebras and partition relations. *Algebra Univ.* **6**, 485–494.

Gentzen, G. 1969 *Collected papers* (ed. M. E. Szabo). Amsterdam: North-Holland Publ. Co. Reviewed in *J. Phil.*, (1971), **68**, 238–265.

Goldfarb, W. D. 1980 On the Gödel class with identity. *J. Symb. Logic* **45**, to appear.

Grattan-Guinness, I. 1979 In memoriam: Kurt Gödel. *Hist Math.* **6**, 294–304.

Hajnal, A. 1956 On a consistency theorem connected with the generalized continuum problem. *Z. math. Logik Grundlagen Math.* **2**, 131–136.

Hawking, S. W. & Ellis, G. F. R. 1973 *The large scale structure of space-time.* Cambridge University Press.
Heckmann, O. & Schücking, E. 1962 Relativistic cosmology. pp. 428–469 of *Gravitation: an introduction to current research.* (ed. L. Witten) New York: John Wiley & Sons.
Herbrand, J. 1930 *Recherches sur la théorie de la démonstration.* Thèse. Paris. *Pr. Tow nauk. warsz.* III, no. 33.
Heyting, A. 1956 *Intuitionism: An introduction.* Amsterdam: North-Holland Publ. Co.
Hilbert, D. 1899 *Grundlagen der Geometrie.* Leipzig: Teubner.
Hilbert, D. 1930 Probleme der Grundlegung der Mathematik. *Math. Annln* **102**, 1–9.
Hilbert, D. 1931 Grundlegung der elementaren Zahlentheorie. *Math. Annln* **104**, 484–494.
Hilbert, D. & Ackermann, W. 1928 *Grundzüge der theoretischen Logik.* Berlin: Springer-Verlag.
Jensen, R. B. 1972 The fine structure of *L. Ann. Math. Logic* **4**, 229–308.
Kanamori, A. & Magidor, M. 1978 The evolution of large cardinal axioms in set theory. *Springer Lecture Notes in Mathematics* **669**, 99–275.
Kleene, S. C. 1976 The work of Kurt Gödel. *J. Symb. Logic* **41**, 761–778. Reviewed in *Zentbl. Math.* (1978), **366**, 6–7.
Kleene, S. C. 1978 An addendum. *J. Symb. Logic* **43**, 613. Reviewed in *Zentbl. Math.* (1979), **401**, 12–13.
Knorr, W. R. 1978 Archimedes and the spirals: the heuristic background. *Hist. Math.* **5**, 43–75.
Komar, A. 1964 Undecidability of macroscopically distinguishable states in quantum field theory. *Phys. Rev.* **133**, B542–544.
Lévy, A. 1957 Indépendance conditionnelle de V = L et d'axiomes qui se rattachent au système de M. Gödel. *C. r. hebd. Séanc. Acad. Sci., Paris* **245**, 1582–1583.
Mahlo, P. 1912 Zur Theorie und Anwendung der ρ_0 Zahlen. *Ber. Verh. sächs. Akad. Wiss.* **64**, 190–200.
Malcev, A. 1936 Untersuchungen aus dem Gebiete der mathematischen Logik. *Mat. Sb.* **1**, 323–336.
Malcev, A. 1941 On a general method for obtaining local theorems in group theory. *Ivanov Gos. Ped. Inst.* **1**, 3–9.
Martin, D. A. 1975 Borel determinacy. *Ann. Math.* **102**, 363–371.
Milnor, J. 1958 Some consequences of a theorem of Bott. *Ann. Math.* **68**, 444–449.
Montague, R. & Vaught, R. L. 1959 Natural models of set theoreis. *Fundam. Math.* **47**, 219–242.
Neumann, J. v. 1927 Zur Hilbertschen Beweistheorie. *Math. Z.* **26**, 1–46.
Neumann, J. v. 1928 Die Axiomatisierung der Mengenlehre. *Math. Z.* **27**, 339–422.
Nöbeling, G. 1968 Verallgemeinerung eines Satzes von Herrn E. Specker. *Inventiones Math.* **6**, 41–55.
Ramsey, F. P. 1928 On a problem of formal logic. *Proc. Lond. math. Soc.* **30**, 338–384.
Robinson, A. 1974 *Non-standard analysis,* (second ed.) Amsterdam: North-Holland Publ. Co.
Robinson, A. & Roquette, P. 1975 On the finiteness theorem of Siegel and Mahler concerning diophantine equations. *J. Number Theory* **7**, 121–176.
Scott, D. S. 1961 More on the axiom of extensionality. pp. 115–131 of *Essays on the foundations of mathematics.* Jerusalem: Magnes Press.
Shoenfield, J. R. 1959 On the independence of the axiom of constructibility. *Am. J. Math.* **81**, 537–540.
Skolem, T. 1922 Einige Bemerkungen zur axiomatischen Begründung der Mengenlehre. *Proc. Fifth Congr. Scandinavian Mathematicians.* Helsinki. pp. 217–232.
Skolem, T. 1934 Über die Nicht-charakterisierbarkeit der Zahlenreihe mittels endlich oder abzählbar unendlich vieler Aussagen mit ausschliesslich Zahlenvariablen. *Fundam. Math.* **23**, 150–161. Reviewed in *Zentbl. Math.* (1934), **7**, 193–194.
Specker, E. P. 1950 Additive Gruppen von Folgen ganzer Zahlen. *Port. Math.* **9**, 131–140.
Spector, C. 1962 Provably recursive functionals of analysis: A consistency proof of analysis by an extension of principles formulated in current intuitionistic mathematics. *Proc. Symposia pure Math.* **5**, 1–27.
Taub, A. H. 1951 Empty space-times. *Ann. Math.* **53**, 472–490.
Troelstra, A. S. 1977 *Choice sequences: a chapter of intuitionistic mathematics.* Oxford: Clarendon Press.
Turing, A. M. 1939 Systems of logic based on ordinals. *Proc. Lond. math. Soc.* **45**, 161–228.
Wang, H. 1974 *From mathematics to philosophy.* London: Routledge & Kegan Paul.
Weyl, H. 1918 *Das Kontinuum.* Leipzig: Gruyter.

Zermelo, E. 1896 Über einen Satz der Dynamik und die mechanische Wärmetheorie. *Annln. Phys.* **57**, 485–494.
Zermelo, E. 1904 Beweis, dass jede Menge wohlgeordnet werden kann. *Math. Annln* **59**, 514–516.
Zermelo, E. 1908 Untersuchungen über die Grundlagen der Mengenlehre. I. *Math. Annln.* **65**, 261–281.
Zermelo, E. 1912 Über eine Anwendung der Mengenlehre auf die Theorie des Schachspiels. *Proc. Fifth Int. Congr. Math.* **2**, 507.
Zermelo, E. 1930 Über Grenzzahlen und Mengenbereiche. *Fundam. Math.* **16**, 28–47.
Zermelo, E. 1932 Über Stufen der Quantifikation und die Logik des Unendlichen. *Jber. dt. MatVerein* **41**, 85–88.
Zermelo, E. 1935 Grundlagen einer allgemeinen Theorie der mathematischen Satzsysteme. *Fundam. Math.* **25**, 136–146.

Bibliography

(1) 1930 Die Vollständigkeit der Axiome des logischen Funktionenkalküls. *Mh. Math. Phys.* **37**, 349–360. See item 28.
(2) Einige metamathematische Resultate über Entscheidungsdefinitheit und Widerspruchsfreiheit. *Anz. Akad. Wiss. Wien.* **67**, 214–215. See item 28.
(3) 1931–32 Diskussion zur Grundlegung der Mathematik. *Erkenntnis* **2**, 147–151.
(4) 1931 Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I. *Mh. Math. Phys.* **38**, 173–198. Italian translation In: *Introduzione ai problemi dell'assiomatica*, by Evandro Agazzi, Milano 1961. See also items 27, 28.
(5) 1932 Zum intuitionistischen Aussagenkalkül. *Anz. Akad. Wiss. Wien.* **69**, 65–66.
(6) 1929–30 Ein Spezialfall des Entscheidungsproblems der theoretischen Logik. (ed. Karl Menger) *Ergebn. math. Kolloq.* **2**, 27–28.
(7) 1930–31 Über Vollständigkeit und Widerspruchsfreiheit. *Ergebn. math. Kolloq.* **3**, 12–13. See item 28.
(8) Eine Eigenschaft der Realisierungen des Aussagenkalküls. *Ergebn. math. Kolloq.* **3**, 20–21.
(9) 1931–32 Eine Interpretation des intuitionistischen Aussagenkalküls. *Ergebn. math. Kolloq.* **4**, 39–4.
(10) Über Unabhängigkeitsbeweise im Aussagenkalkül. *Ergebn. math. Kolloq.* **4**, 9–10.
(11) Zur intuitionistischen Arithmetik und Zahlentheorie. *Ergebn. math. Kolloq.* **4**, 34–38. See item 27.
(12) 1932–33 Bemerkung über projektive Abbildungen. *Ergebn. math. Kolloq.* **5**, 1.
(13) 1934–35 Über die Länge von Beweisen. *Ergebn. math. Kolloq.* **7**, 23–24. See item 27.
(14) 1933 Zum Entscheidungsproblem des logischen Funktionenkalküls. *Mh. Math. Phys.* **40**, 433–443. For a correction, see Goldfarb (1980).
(15) 1934 On undecidable propositions of formal mathematical systems (Mimeographed notes of lectures given in 1934), 30 pages. See item 27.
(16) 1938 The consistency of the axiom of choice and of the generalized continuum-hypothesis. *Proc. natn. Acad. Sci. U.S.A.* **24**, 556–557.
(17) 1939 Consistency-proof for the generalized continuum-hypothesis. *Proc. natn. Acad. Sci. U.S.A.* **25**, 220–224.
(18) 1940 The consistency of the continuum hypothesis. *Ann. Math. Stud.* no. 3, 66 pages. Princeton University Press. Second printing, revised and with some notes added, 1951, 69 pages. Seventh printing, with some notes added, 1966, 72 pages.
(19) 1944 Russell's mathematical logic. In: *The philosophy of Bertrand Russell.* (ed. P. A. Schilpp), pp. 123–153. Evanston and Chicago. See item 26.
(20) 1947 What is Cantor's continuum problem? *Am. math. Mon.* **54**, 515–525. See item 26.
(21) 1949 An example of a new type of cosmological solutions of Einstein's field equations of gravitation. *Rev. mod. Phys.* **21**, 447–450.
(22) A remark about the relationship between relativity theory and idealistic philosophy. In: *Albert Einstein, Philosopher-Scientist.* (ed. P. A. Schilpp) pp. 555–562. Evanston. German translation with some additions to the footnotes in: *Albert Einstein als Philosoph und Naturforscher.* pp. 406–412. Kohlhammer 1955.
(23) 1950 Rotating universes in general relativity theory. *Proc. Int. Congr. Math., Cambridge, Mass.* vol. 1, 175–181.

(24) 1958 Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes. *Dialectica* **12**, 280–287.
(25) 1965 Remarks before the Princeton Bicentennial Conference on Problems of Mathematics. *The undecidable.* (ed. Martin Davis), pp. 84–88. New York: Raven Press.
(26) 1964 A reprint of item 19 and a revised and enlarged edition of item 20 were published in: *Philosophy of mathematics*, (ed. P. Benacerraf & H. Putnam), pp. 211–232, 258–273. Englewood Cliffs, N.J.: Prentice Hall.
(27) 1965 English translations of items 4, 11, 13 and a revised and enlarged edition of item 15 were published in: *The undecidable.* (ed. Martin Davis), pp. 4–38, 75–81, 82–83, 39–75. New York: Raven Press.
(28) 1967 English translations of items 1, 2, 4, 7, with some notes by the author, were published in: *From Frege to Gödel.* (ed. Jean van Heijenoort), pp. 583–591, 595–596, 596–616, 616–617. Harvard University Press.
(29) 1974 Preface to Robinson (1974) (see References).

(Added in proof.) *Note on non-mechanical laws of nature* (p. 217). M. B. Pour El & I. Richards (*Abstr. Am. math Soc.* 1, abstr. 80T–E53 (1980)) have found computable initial data (in dimensions >1) for which the wave equation has a unique, but non-computable solution. It remains to be seen if a physical system, perhaps by use of lasers, can realize these solutions with the kind of probability of error expected in the execution of a computer programme (by a digital computer).