

MICHAEL DUMMETT

FREGE

philosophy of
mathematics

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Philosophy of Mathematics

Harvard University Press
Cambridge, Massachusetts

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Library of Congress Cataloging-in-Publication Data

Dummett, Michael A. E.

Frege : philosophy of mathematics / Michael Dummett.

p. cm.

Includes index.

ISBN 0-674-31935-4 (cloth)

ISBN 0-674-31936-2 (pbk.)

1. Mathematics—Philosophy. 2. Mathematics—Foundations.

3. Logic. Symbolic and mathematical. I. Title.

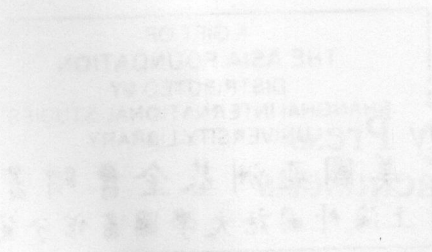
QA8.4.D86 1991

510'.1—dc20

91-26574

CIP

03005018



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Philosophy of Mathematics

20 0020 12

000028 12

— F E R G E
— P H I L O S O P H Y
— O F M A T H E M A T I C S

Harvard University Press
Cambridge, Massachusetts

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This book has been published by Knopf
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Preface

A book of this title was advertised as forthcoming in Duckworth's catalogue for 1973, the year in which my *Frege: Philosophy of Language* was published. I therefore feel some need to explain why it is coming out only now to all who have been asking me, over the years, when it was going to appear. It was not in fact until 1973 that I started to write, as a separate book, this sequel to the earlier one. For the new book, I formed the plan of setting out systematically the problems of the philosophy of mathematics, and considering in order Frege's responses to them, to the extent that he said anything relevant: the architecture of the book was to be that of the subject, not of Frege's writings, that is to say of the subject as I saw it, not as Frege saw it.

I completed about two-thirds of the book in 1973. Though I was fortunate to hold, for a few years, a Senior Research Fellowship at All Souls' College, other writing commitments, including the preparation, with much help from Mark Helme and Charles Donahue, of the second edition of *Frege: Philosophy of Language* and the composition of the introduction to it, which turned into *The Interpretation of Frege's Philosophy*, prevented me from attending to the book, which remained untouched on my shelves, until 1982. In that year I was awarded an Alexander von Humboldt-Stiftung prize for study in Germany, and spent four months at the University of Münster in Westphalia, taking with me the typescript of *Frege: Philosophy of Mathematics*. There are two well-known reactions to reading what one has written long ago: to think, 'How brilliant I was then: I could never do that now'; and to wonder how one could have written such poor stuff. Mine was the second, and I started to rewrite the whole book, still on the same plan, from Chapter 3 onwards. To my disappointment, I did not finish. During four long vacations, from 1983 to 1986, I gave my main attention to trying to finish the book. Each time, it was difficult to recall just what my previous intentions had been, and each time I failed. In 1985, I decided to extract all the material on Frege's theory of real numbers and publish it as a separate monograph, including both philosophical and mathematical material, the latter including the solution, due to Dr Peter

Neumann, of the independence problem that troubled Frege;¹ I then worked simultaneously on the monograph and on the main book. But even with this excision, the latter grew beyond all reasonable size; and still I did not finish either.

During 1988–9, I enjoyed, for the first time in my life, a whole sabbatical year. I was lucky enough to spend from September to June at the Center for Advanced Study in the Behavioral Sciences at Stanford. I went hoping to complete two long unaccomplished tasks, one of them the Frege book, for which I took with me for an enormous pile of typescript and collection of discs. It was a toss-up which of the two tasks I should start on first; but I happened to select the William James lectures on *The Logical Basis of Metaphysics*. I succeeded in sending off a completed text of that book to Harvard University Press just before I left in June. I had also to revise a much shorter book, *I Tarocchi Siciliani*, in accordance with the suggestions of my then collaborator and now sorely missed friend, the late Marcello Cimino.² The result of all this was that I did not devote one minute of my time at Stanford to *Frege: Philosophy of Mathematics*, and crossed the Atlantic again with all my typescripts and discs unused.

For those who think in terms of completion rates, mine is disgraceful. 'Completion rates' – the very phrase is like a bell. British universities are in the course of being transformed by ideologues who misunderstand everything about academic work. The transformation is of course merely part of a transformation of society as a whole. The official stance of the ideologues is that they do not believe that there is any such thing as society; in point of fact, however, they do not believe in anything else. They are concerned, for example, with the performance of 'the economy': not with whether individual people are prospering, but with the economy as a distinguishable system on its own. The successful performance of the economy will grossly enrich some, and deprive others of all hope or comfort: but the aim, if one is not to take a cynical view of it, cannot be either to reward those who scramble to the top of the economic mountain or to punish those who are cast on to the scrapheap at its foot, but simply to ensure efficient functioning of the economy as such. The vision which the ideologues have of the successful functioning of the economy or of any other social mechanism is that it works well only if operated by human beings engaged in ruthlessly biting and clawing their way to the top, where they will be able to obtain a disproportionate share of limited rewards.

¹ Published in S.A. Adeleke, M.A.E. Dummett and Peter M. Neumann, 'On a Question of Frege's about Right-Ordered Groups', *Bulletin of the London Mathematical Society*, vol. 19, 1987, pp. 513–21.

² I had, however, also had to devote much time to extensively revising *La Storia dei Tarocchi* which had been commissioned by Bibliopolis of Naples in 1982, and had become seriously out of date in the interim. I was compelled to give priority to this task, because the director of the publishing house, Signor Francesco del Franco, had promised to bring the book out by Christmas 1989, and wanted the revisions urgently. It has not yet appeared, but I still have hopes.

For this purpose, the people so competing with one another should not be encouraged to believe in the good of anything but themselves as individuals; if they were to believe in society as a whole, they might form ideas about protecting the weak or unfortunate that would clog the efficiency of the system. A glance at the universities as they used to be revealed a social sector not functioning in this manner; it therefore obviously could not be functioning efficiently, or justifying the money spent on it, and hence must be transformed in accordance with the model decreed by ideology.

The plan of the ideologues is to increase academic productivity by creating conditions of intense competition. Those who compose what is known, in today's unlovely jargon, as academic and academic-related staff are now to be lured by the hope of gaining, and goaded by the shame of missing, extra payments and newly invented titular status. Their output is monitored by the use of performance indicators, measuring the number of words published per year. Wittgenstein, who died in 1951 having published only one short article after the *Tractatus* of 1922, would plainly not have survived such a system. Those most savagely affected by the new regime are, as always, the ones on the bottom rung of the ladder: the graduate students working for their doctorates. The degree of Ph.D. (in Oxford, D.Phil.) fitted rather awkwardly into the system of doctorates as it had evolved in Britain out of the mediaeval one, and was originally instituted here to satisfy the needs of foreign students, for whom it was a necessary professional qualification. Only in recent years has it become an indispensable minimum qualification for British academic posts in arts subjects: candidates for them stand little chance if they cannot also show, at the start of their careers, an impressive list of publications. Relentless pressure is applied to students and their universities by the Government and its agencies – the research councils and the British Academy – to force them to complete their doctoral theses within three years of graduating; but it is hardly needed. Nervously conscious from the start that they must jostle one another for the diminished number of posts, they are anxious to jump the first hurdle of the Ph.D. degree as quickly as possible, and then rush to submit their unrevised theses for publishers to turn into books.

The universities have no option but to co-operate in organising the squalid scramble that graduate study has become, in introducing the new 'incentives' for their professors and lecturers and in supplying the data for the evaluation process. The question is to what extent they will absorb the values of their overlords and jettison those they used to have. Once more, it is the graduate students who are the most at risk, for they are in effect being taught that the rat-race operates as ferociously in the academic as in the commercial world, and that what matters is not the quality of what you write but the speed at which you write it and get it into print. It is obviously as objectionable in a capitalist as in a communist country that politicians should decide how the universities are to be run; but it is catastrophic when those politicians display

total ignorance of the need to judge academic productivity on principles quite different from those applicable to industry. Our masters show some small awareness that, as in industry, quality is relevant as well as quantity: their performance indicators are sometimes modified by the use of more sophisticated criteria, such as counting the number of references made by other writers to a given article. Frege would never have survived such a test: his writings were very seldom referred to in his lifetime. It is not, however, that quantity is not the only criterion, but that it is positively harmful. The reason is that overproduction defeats the very purpose of academic publication. It long ago became impossible to keep pace with the spate of books and of professional journals, whose number increases every year; once this happens, their production becomes an irrelevance to the working academic, save for the occasional book or article he happens to stumble on. This applies particularly to philosophy. Historians may be able to ignore much of their colleagues' work as irrelevant to their periods; but philosophers are seldom so specialised that there is anything they can afford to disregard in virtue of its subject-matter. Given their need for time to teach, to study the classics of philosophy and to think, they cannot afford to plough through the plethora of not bad, not good books and articles in the hope of hitting on the one that will truly cast light upon the problems with which they are grappling; hence, if they are sensible, they ignore them altogether.

Academics who delivered their promised manuscripts twenty years late used to cause us amusement; but it was a respectful amusement, because we knew the delay to be due, not to idleness, but to perfectionism. Perfectionism can be obsessive, like that which prevented Wittgenstein from publishing another book in his lifetime, and probably would have done so however long he had lived; but, as the phrase goes, it is a fault on the right side. Every learned book, every learned article, adds to the weight of things for others to read, and thereby reduces the chance of their reading other books or articles. Its publication is therefore not automatically justified by its having some merit: the merit must be great enough to outweigh the disservice done by its being published at all. Naturally, no individual writer can be expected to be able accurately to weigh the one against the other; but he should be conscious of the existence of such a pair of scales. We used to be trained to believe that no one should put anything into print until he no longer sees how to make it any better. That, I still believe, is the criterion we should apply; it is the only means that exists of keeping the quality of published work as high as possible, and its quantity manageably low. The ideologues who in their arrogance force their misconceived ideals upon us attempt to make us apply virtually the opposite criterion: publish the moment you can get editor or publisher to accept it. We are compelled outwardly to comply with their demands; let us inwardly continue to maintain our own values.

When I returned from Stanford in 1989, it was early June, and I still had

more than three clear months of my sabbatical to run. I plunged straight away into work on the present book. Instead of revising, compressing and tailoring the enormous amount of material I had already amassed, however, I ignored it altogether, and started writing afresh, on an entirely different plan, indeed virtually the opposite plan. Instead of arranging the book as one might arrange a systematic non-historical treatise on the philosophy of mathematics, I composed it as a close study of Frege's texts: that is, of his *Grundlagen*, followed by selected parts of the later *Grundgesetze*. Into this were to be inserted some comparative matter concerning Frege and Dedekind, and Frege and Husserl: not, however, for its own sake, but as illuminating Frege's texts. The *Grundlagen* is written with a deceptive clarity: it is in fact a very easy book to misunderstand. My original plan for my book on Frege's philosophy of mathematics had left readers without a helpful guide to the subtleties, and artfully concealed lacunae, in the argument of *Grundlagen*: I should do them much better service, I now thought, by providing one.

Furthermore, the new plan concentrated attention on what was central to Frege's philosophy of arithmetic. I had not intended, at the outset, to write a treatise of length comparable to that of *Frege: Philosophy of Language*. What had swollen the book to beyond that size was a misguided ambition to achieve comprehensiveness: I had thought I must include everything relevant to the philosophy of mathematics. A great deal of this – such as a chapter on Frege's philosophy of geometry – has now been excised. Among the casualties has been a discussion of Frege's views on the consistency of mathematical theories, in terms of his controversy with Hilbert, which had been written in 1973 and survived successive revisions intact. It had in fact been published in Matthias Schirn's collection *Studien zu Frege/Essays on Frege* of 1976: when he asked me for a contribution, I selected the most self-contained passage from the uncompleted typescript of the book. Since it has been published, and republished in my *Frege and Other Philosophers*, its omission from here is no loss. The topic is indeed of some interest; and there were other discussions, unpublished and now suppressed, on topics of similar interest. I decided, however, that the attempt to discuss everything in Frege's writings that bore on the philosophy of mathematics had resulted, and could only result, in a diffuse, rambling book. I have tried to replace it by one that goes to the heart of Frege's philosophy of arithmetic, setting aside everything not of central importance for that purpose.

Frege's reputation as a philosopher of logic, of language and of thought has grown steadily from about 1950 onwards; he is generally perceived as the founder of analytical philosophy. Not so his reputation as a philosopher of mathematics. His work in this field has tended to be equated with maintenance of the logicist thesis, and consequently dismissed as a total failure; it is ironic that, in his last years, he would have concurred with this judgement. He would have done so because he had aimed at, and for a time had believed that he

had achieved, total success; but, since no one has achieved total success, it requires explanation why that judgement should be made now. Hilbert, too, propounded a programme that proved impossible of execution as he formulated it; and his philosophy of mathematics, as a system, would have been tenable only if that programme could have been carried out: yet no one regards Hilbert's views on the subject as negligible. Probably the reason is that Frege's work does not prompt any further line of investigation in mathematical logic, unlike the modifications of Hilbert's programme studied by Georg Kreisel. It does not even appear to promise a hopeful basis for a sustainable general philosophy of mathematics: while it is appealing to be a neo-Dedekindian like Paul Benacerraf, or a neo-Hilbertian like Hartry Field, neo-Fregeanism, though espoused by Crispin Wright and by David Bostock,³ seems to most to be considerably less attractive.

Various features of Frege's work in the philosophy of mathematics have contributed to the general neglect of it. An inborn obstinacy combined with his increasing bitterness to make him ever less receptive to the ideas of others. He had a great early interest in geometry, particularly projective geometry; and in *Grundlagen* he alluded to non-Euclidean geometry in a perfectly reasonable way, categorically affirming the consistency of elliptic geometry but observing that we cannot imagine such a space. Subsequently, he became a fierce opponent of non-Euclidean geometry, descending, in a fragment of his *Nachlass* of which it is to be hoped that he was not later proud, to comparing it, as a pseudo-science, with alchemy. He allowed no merit to Hilbert's *Grundlagen der Geometrie*, nor, in his *Grundgesetze*, Volume II, to either Cantor's or Dedekind's theory of real numbers; and, although he lived until 1925, he paid scarcely any attention to the work of his successors in mathematical logic. Some explanations, psychological or intellectual, can be given for these attitudes. He continued to regard geometry as the science of physical space, and so held that there can be only one true geometrical theory. His early respect for Cantor, manifested in *Grundlagen*, was repaid by the cruelty of Cantor's mean-spirited review of that book. Yet, whatever may be said in mitigation, these evidences of the blindness and lack of generosity which were such marked features of Frege's work after 1891 combine with his great blunder in falling into the contradiction to suggest that he cannot have much to teach us.

Nevertheless, his work in this field deserves great respect. It certainly cannot be reduced to the bare statement of the logicist thesis. There is much that he found worth saying, or said for the first time, that is either obvious to us or a received part of very elementary logic or mathematics; but there is also much that remains challenging. A good deal, indeed, is patently wrong; but of which philosopher of mathematics is that not true? Despite his blindness to things

³ See C. Wright, *Frege's Conception of Numbers as Objects*, Aberdeen, 1983, and D. Bostock, *Logic and Arithmetic*, vol. I, *Natural Numbers*, Oxford, 1974, vol. II, *Rational and Irrational Numbers*, Oxford, 1979.

his contemporaries perceived, despite his unawareness of much that concerns us but wholly failed to strike him, or could not even be formulated until logic had made further advances, he is, in my judgement, the best philosopher of mathematics. This book is a historical study: but it has been written in the belief that we can still profit greatly by reflecting on what Frege wrote about the foundations of arithmetic, and therefore in the hope that it is not merely a historical study.

Oxford, July 1990

M.D.

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CHAPTER 1

The Significance of Grundlagen

Die Grundlagen der Arithmetik is Frege's masterpiece: it is his most powerful and most pregnant piece of philosophical writing, composed when he was at the very height of his powers. It was written as a prolegomenon to his *magnum opus*, *Grundgesetze der Arithmetik*: a first rough sketch of Part II of that work, presented without unfamiliar symbolism and with a minimum of symbolism of any kind, in the hope of reaching as wide an audience as possible. But it occupies both a more central and a more problematic place in his work on the philosophy of arithmetic than this intention would suggest. What he did not foresee, when he was composing it, was that, in starting work on *Grundgesetze*, he would be led to make fundamental changes both in his formal logical system and in his underlying philosophy of logic. It is the system of logical and philosophical doctrines that Frege elaborated as embodying these changes which we think of as constituting his philosophy; and it was in the framework of this system that the two volumes of *Grundgesetze* were written. This suggests that *Grundlagen* should be set aside as a brilliant but immature work, and that we should study Frege's philosophy of arithmetic primarily from his *Grundgesetze*. We cannot do that, however, because he chose not to carry out, on a revised basis, a philosophical justification of his theory of natural numbers of the kind that had occupied most of *Grundlagen*: Part II of *Grundgesetze*, which corresponds to *Grundlagen* in subject-matter, is wholly formal in character, being written almost exclusively in Frege's logical notation, and thus entirely omitting the philosophical argumentation. It was not that Frege had come to consider such argumentation superfluous, for he supplies it at great length in Part III of *Grundgesetze*, which treats of the foundations of the theory of real numbers, a subject left untouched in *Grundlagen*. It must have been, rather, that he considered that readers could easily transpose the argument of *Grundlagen* into the mode of his new system of philosophical logic. If so, he gravely underestimated the difficulty of the task, which to this day creates problems not easily solved. We have no choice, however, but to treat *Grundlagen* as presenting the greater part of the philosophical underpinnings of the theory of the foundations of arithmetic expounded in *Grundgesetze*, while bearing in

mind that, if he had incorporated this material into *Grundgesetze*, he would have subjected it to substantial modification.

Grundlagen is deceptively lucid. That is not at all to say that it is deliberately misleading; only that it is so persuasively written, and so adroit in its selection of the rival views that are then so skilfully refuted, that it is easy to overlook the options that have not been presented to their best advantage, or at all, and to misconstrue the architecture of the argument as it is developed from beginning to end of the book. We have here to review the course of that argument so as to bring to light all that is not apparent on first reading.

Grundlagen is written in the framework of a Kantian terminology, not used by Frege in any of his writings after 1890, save those composed at the very end of his life. This terminology does not indicate his acceptance of any specifically Kantian doctrines: indeed, despite the tone of deep respect he frequently, though by no means invariably, adopts when speaking of Kant, he overtly discusses Kant's views almost exclusively to disagree with them. Frege's use of his terminology may be due to a special effort to make himself understood by the professional philosophers; more probably, to his simply assuming that a Kantian framework was the proper one within which to pose philosophical questions. The brilliance of *Grundlagen* makes it easy to forget that it was, after all, his first full-fledged incursion into philosophy.

The status of *Grundlagen*

The principal problem of Frege exegesis is to determine the relation between the writings of Frege's early period, up to 1886, and those of his middle period, beginning in 1891. During the years 1887–1890, he published nothing, but was engaged in thinking through afresh his system of philosophical logic and redesigning, in accordance with it, the formal system he had presented in *Begriffsschrift*. He announced his new ideas in the lecture *Function und Begriff* of 1891. The principal changes in his philosophical logic were the introduction of the far-reaching distinction between sense and reference, and the identification of truth-values as objects and as the references of sentences. The principal changes in his formal system were the introduction of value-ranges, and the obliteration of any formal distinction between sentences (henceforward called by him 'names of truth-values') and singular terms ('proper names'); the addition of a description operator was an important secondary development. During the middle period, lasting from 1891 to 1906, his thought evolved little. Doubtless much of what he wrote was newly thought out: but there is no reason to suppose that he ceased, at any later time within this period, to believe anything that he wrote for publication at any time during it. The logical basis of all the work of the middle period was presented complete and entire in *Function und Begriff*; and it scarcely altered throughout the whole period.

The early period, by contrast, was one of considerable development, during

which Frege's views changed, sometimes subtly and, in some instances, radically. To recognise this, it is sufficient to compare what Frege wrote in *Grundlagen* with the remark in the article 'Booles rechnende Logik und die Begriffsschrift', which in 1881 – only three years before the appearance of *Grundlagen* – he unsuccessfully submitted for publication, that 'individual things cannot be assumed to be given in their totality, since some of them, such as numbers for example, are first created by thinking'.¹ We therefore cannot presume that what he wrote at one time during his early period he would have continued to endorse at a later time, though it is natural to suppose that he regarded later thoughts as better. The greatest difficulty is to decide how much carried over from the early to the middle period. Naturally, when what he wrote in his middle period expressly corrected or modified something he had said in the early period, we know exactly where we are: but what when he was simply silent?

This question is particularly acute in relation to *Grundlagen*, because three salient doctrines of that book were never afterwards explicitly reaffirmed by Frege, but never explicitly denied by him, either. The first is the 'context principle', that it is only in the context of a sentence that a word has meaning. This has been much discussed: I believe that a definitive answer can be given to the greatly controverted question whether he repudiated or maintained it, and shall give that answer in its proper place. The second is the adoption by Frege, in § 3 of *Grundlagen*, of the Kantian classification of true propositions into analytic, synthetic a priori and a posteriori, and his recharacterisation of these three classes. The very object of the book is stated, in § 87, as having been to make it probable that 'the laws of arithmetic are analytic judgements and consequently a priori'; and yet, throughout his middle period, Frege never employed these or any equivalent terms. It is instructive to read the different way in which he stated the object of *Grundlagen* in the first sentence of his Introduction to *Grundgesetze*: 'in my *Grundlagen der Arithmetik* I sought to make it probable that arithmetic is a branch of logic and that no ground of proof needs to be drawn either from experience or from intuition.' Not only is this more accurate, in that to call a proposition 'analytic', in the sense of *Grundlagen*, is not to say that it is expressible in purely logical terms: more importantly, it relates, not to individual propositions, but to an entire theory, taken as a whole. It is possible that Frege came to be dissatisfied, either with the manner in which he had defined 'analytic' and 'a priori', or with those concepts themselves; if so, it is puzzling that he never said so, but, if not, equally puzzling that he refrained from ever employing them again until 1924. The third doctrine never again heard of after *Grundlagen* is that which introduced the pregnant concept of a criterion of identity: 'if we are to use the symbol *a* to designate an object', he pronounced in § 62, 'we must have a criterion which decides in all cases

¹ *Nachgelassene Schriften*, p. 38, *Posthumous Writings*, p. 34.

whether b is the same as a , even if it does not always lie within our power to apply this criterion.' This is an immensely important dictum: in this third example, it is especially mysterious that the whole topic should apparently have vanished from his thinking.

More important than whether, or to what extent, Frege continued during his middle period to maintain these three particular doctrines is the question whether or not we may take the philosophy of arithmetic expounded in *Grundlagen* to be essentially that to which he subscribed during the middle period. That the actual logical construction of the theory of the natural numbers, and of cardinal numbers generally, remained the same is beyond question, since it is repeated in *Grundgesetze* in more detail but in essentially the same way that it is sketched in *Grundlagen*: what needs to be decided is whether the philosophical ideas remained the same, allowing for the more sophisticated philosophical logic Frege had elaborated in the meantime. This question can be answered by considering the architecture of *Grundgesetze*.

The structure of *Grundgesetze*

Grundgesetze, as we have it, is divided into three Parts; but it is an uncompleted work. The division into volumes has scarcely any relation to the segmentation of the book: it looks as though Frege had an agreement with his publisher that a certain number of pages constituted a volume, and the publisher brought out a volume as soon as he had copy amounting to that number of pages. At any rate, the two volumes are of almost precisely the same length: Volume I has 254 pages of text, with 32 pages of Preface and Contents, making 286 in all, while Volume II has 253 pages of the main text, with 16 pages of Contents and 13 pages of the Appendix dealing with Russell's contradiction, which we know to have been added in proof, making 282 pages in all; perhaps Frege withdrew a section in order to make room for the Appendix. Volume I contains all of Part I and about three-quarters of Part II; Volume II contains the rest of Part II and about two-thirds of Part III: possibly Frege planned a fourth Part, or possibly Volume III, had it appeared, would have been shorter.

Volume III did not appear because Frege came to realise that his solution to Russell's contradiction, set out in the Appendix, was inadequate. The last paragraph but one of the Appendix, dated October 1902, reads as follows:

It would take us too far here to pursue further the consequences of replacing [the original axiom] (V) by [the proposed modification] (V'). It must be acknowledged that to many of the propositions auxiliary hypotheses will have to be added; but there need be no anxiety that any essential obstacles to carrying out the proofs will arise from this. It will nevertheless be necessary to check thoroughly all propositions discovered up to this point.

That of course is correct: when one of the axioms of a theory is weakened, it